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THE
PHILOSOPHICAL TRANSACTIONS

OF THE
ROYAL SOCIETY OF LONDON,

FROM THEIR COMMENCEMENT, IN 1665, TO THE YEAR 1800;

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

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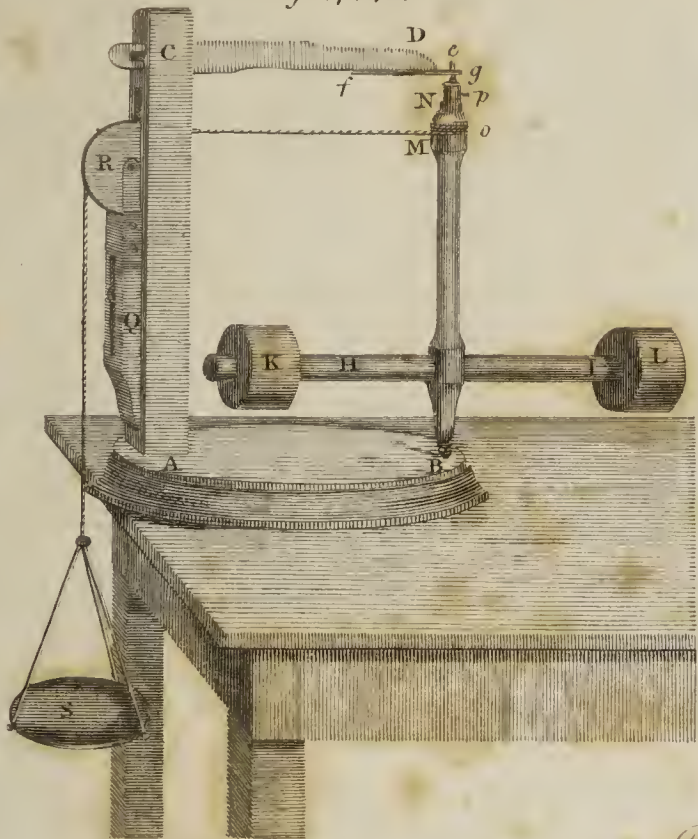
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Fig. 2.



Fig. 6.



Fig. 3.

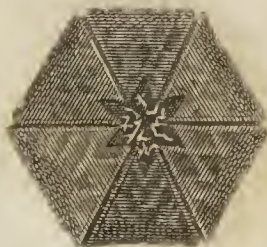


Fig. 7.



Fig. 5.

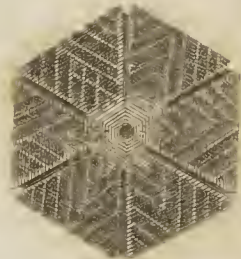


Fig. 4.



Fig. 8.



Fig. 9.



Fig. 11.

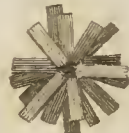


Fig. 10.



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Fig. 12.

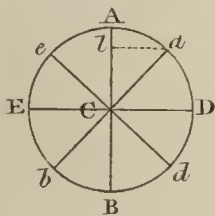


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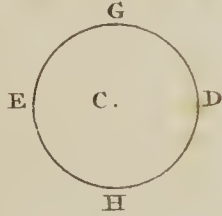


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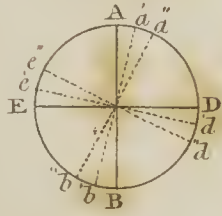


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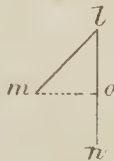


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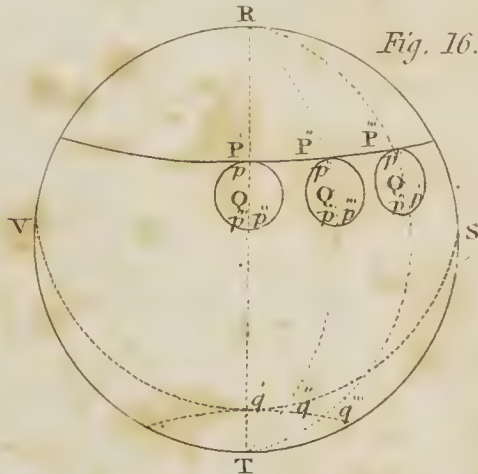


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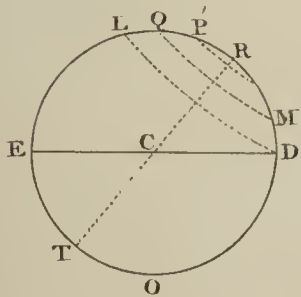


Fig. 18.

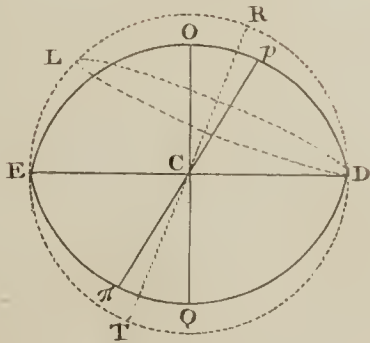


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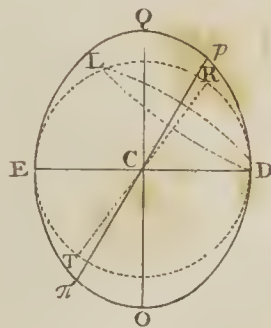


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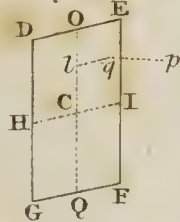


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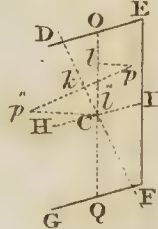


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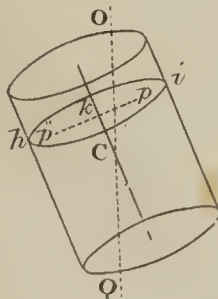


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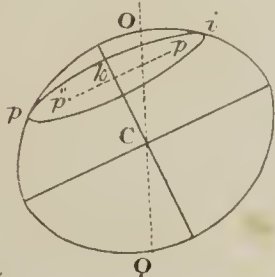


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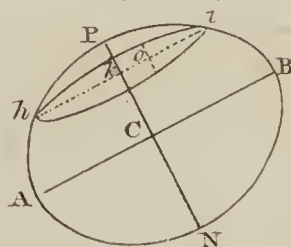


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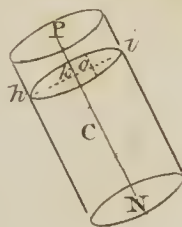


Fig. 1.

Pa. 178.

Fig. 2.

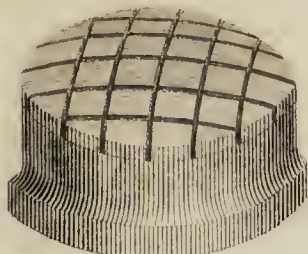
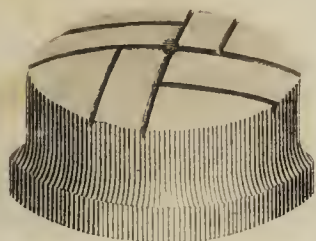


Fig. 4.

Pa. 187.

Fig. 5.

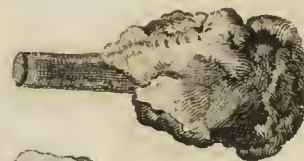


Fig. 6.



Fig. 3.

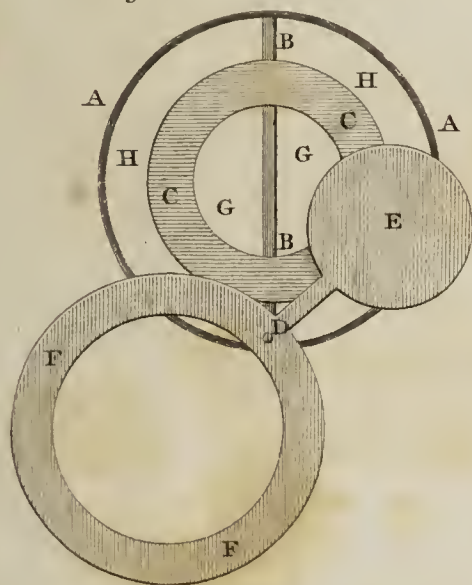
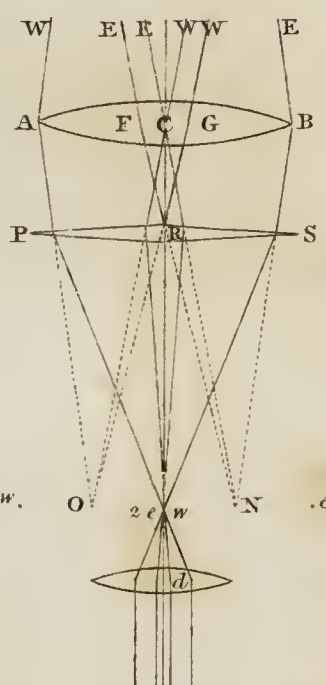
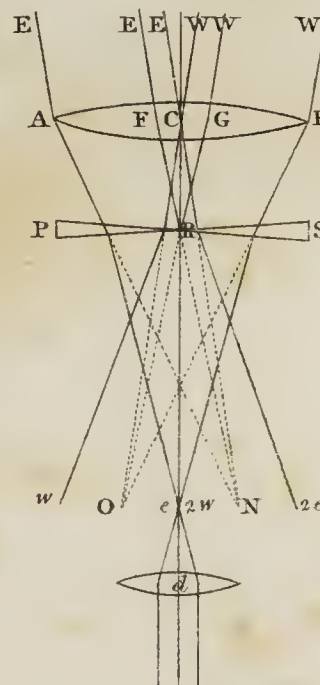
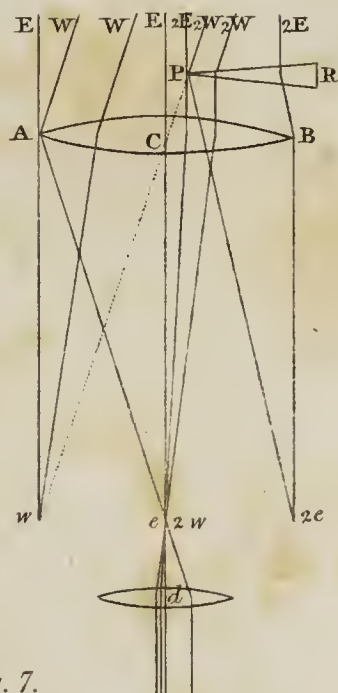


Fig. 8.

Pa. 252. &c.

Fig. 9.

Fig. 10.



Pa. 259. Fig. 11.

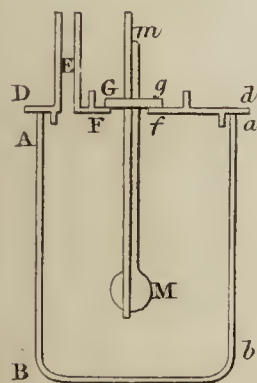
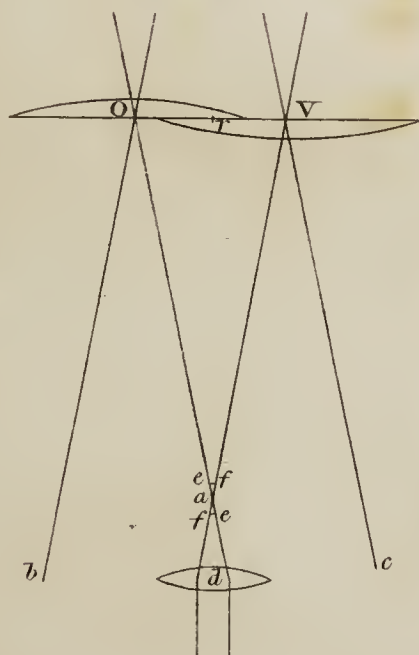


Fig. 7.
Pa. 251.



Pa. 268. &c. Fig. 12.



Fig. 13.

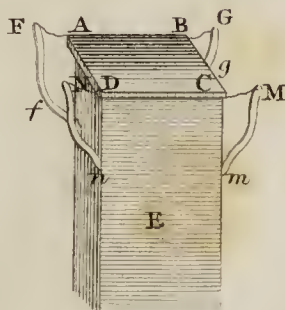


Fig. 16.

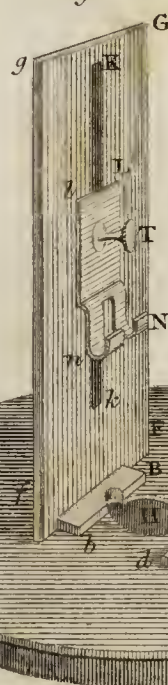


Fig. 17.

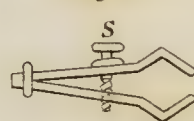


Fig. 14.

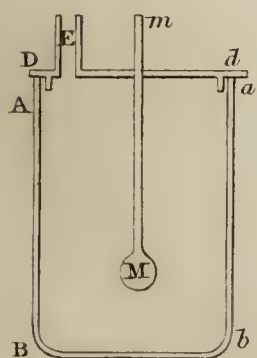
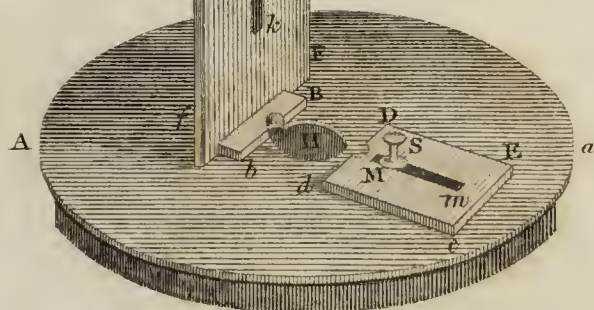
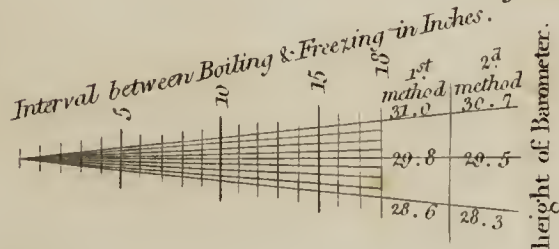
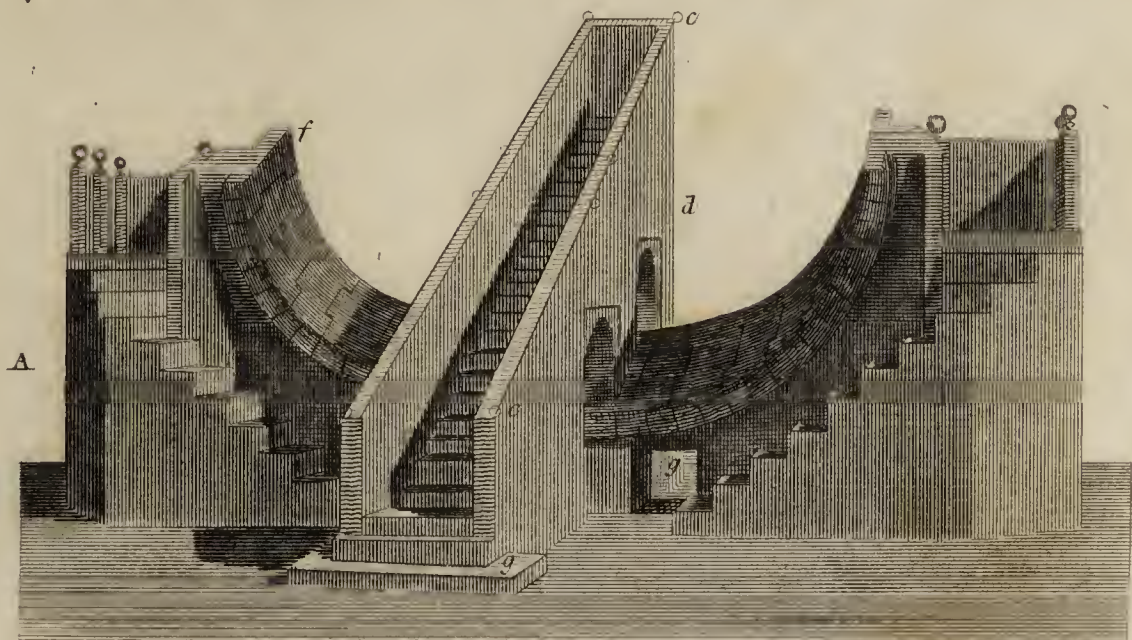
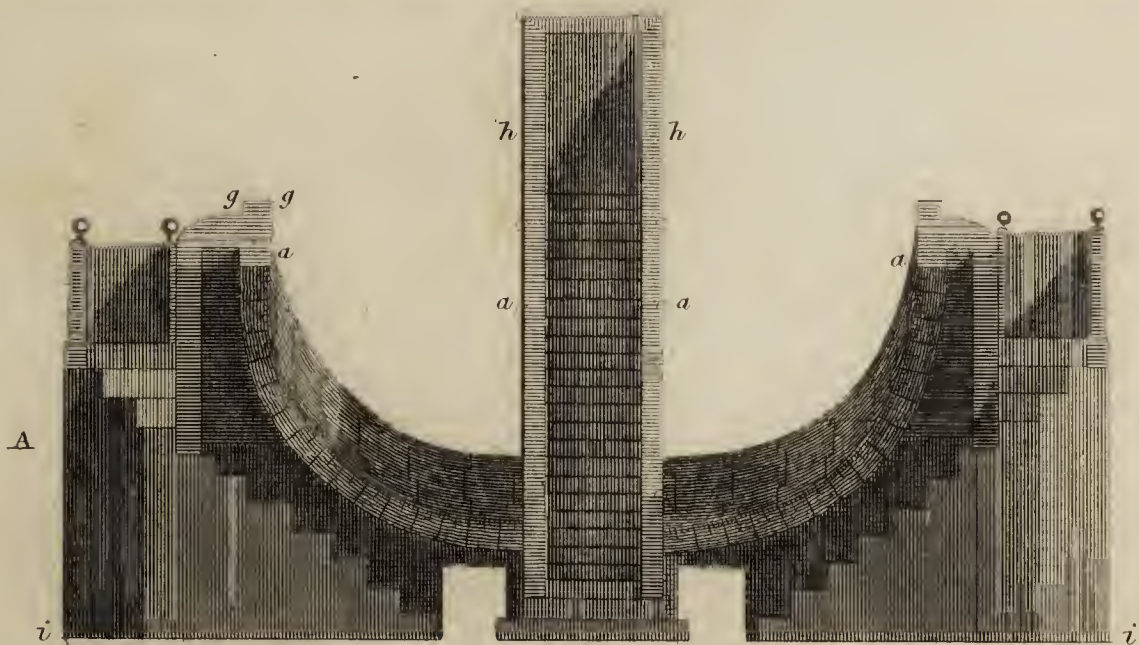
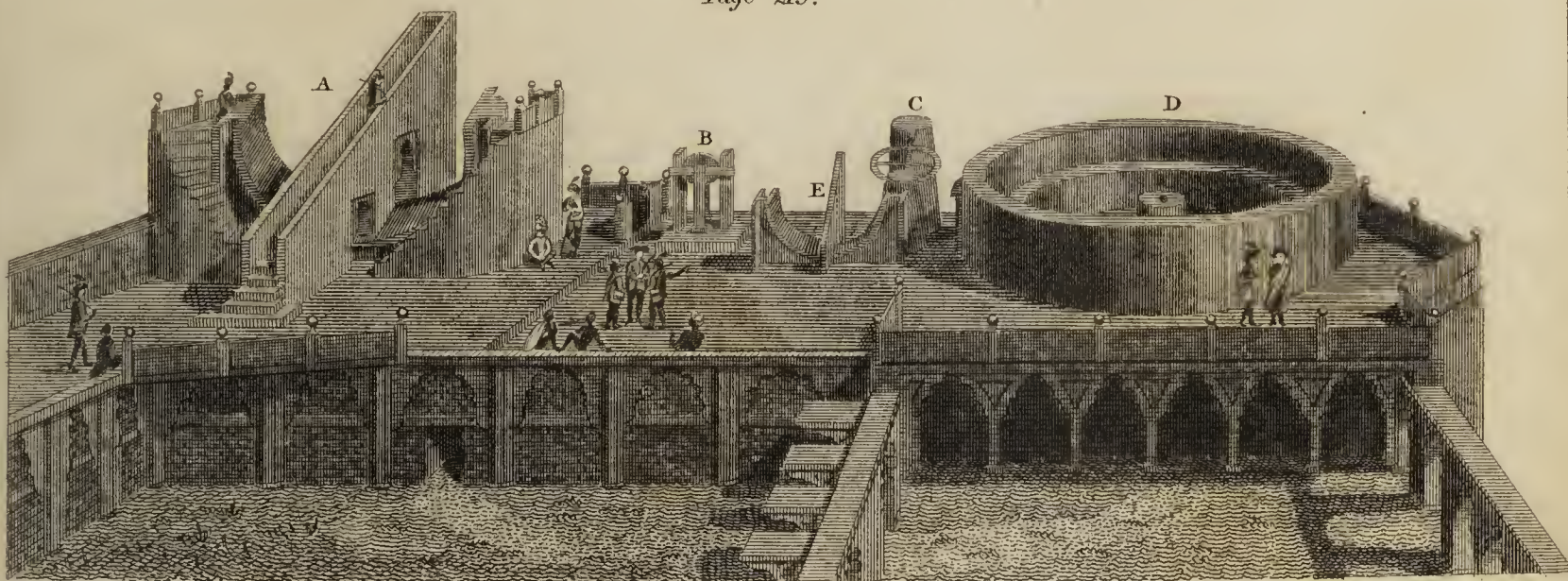


Fig. 15.





Page 215.



0 1 2 3 4 5 6 7 8 9 10 11 12 Scale of Feet.

Mutlow Sc. Russell & Co.

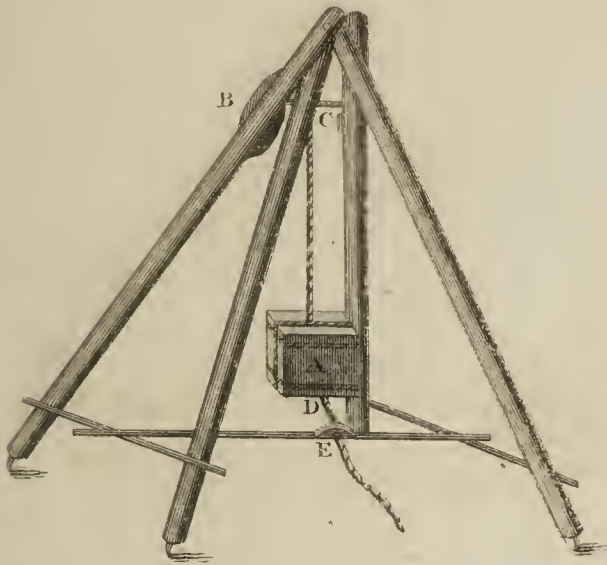
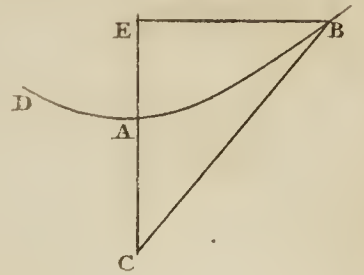
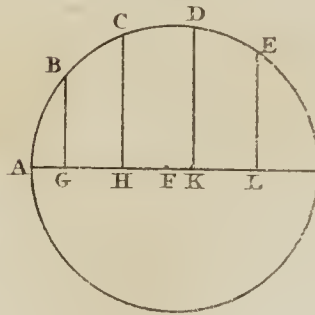
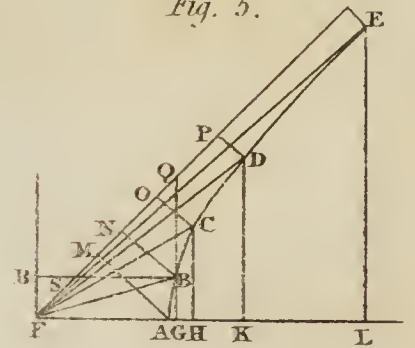
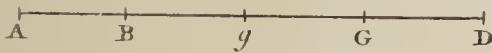
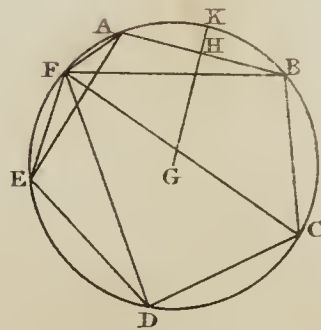
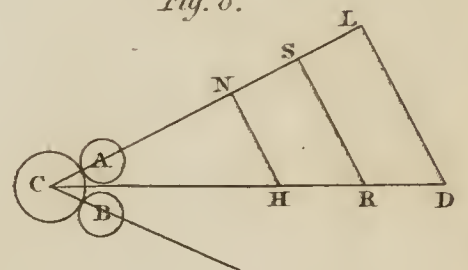
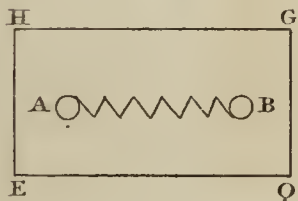
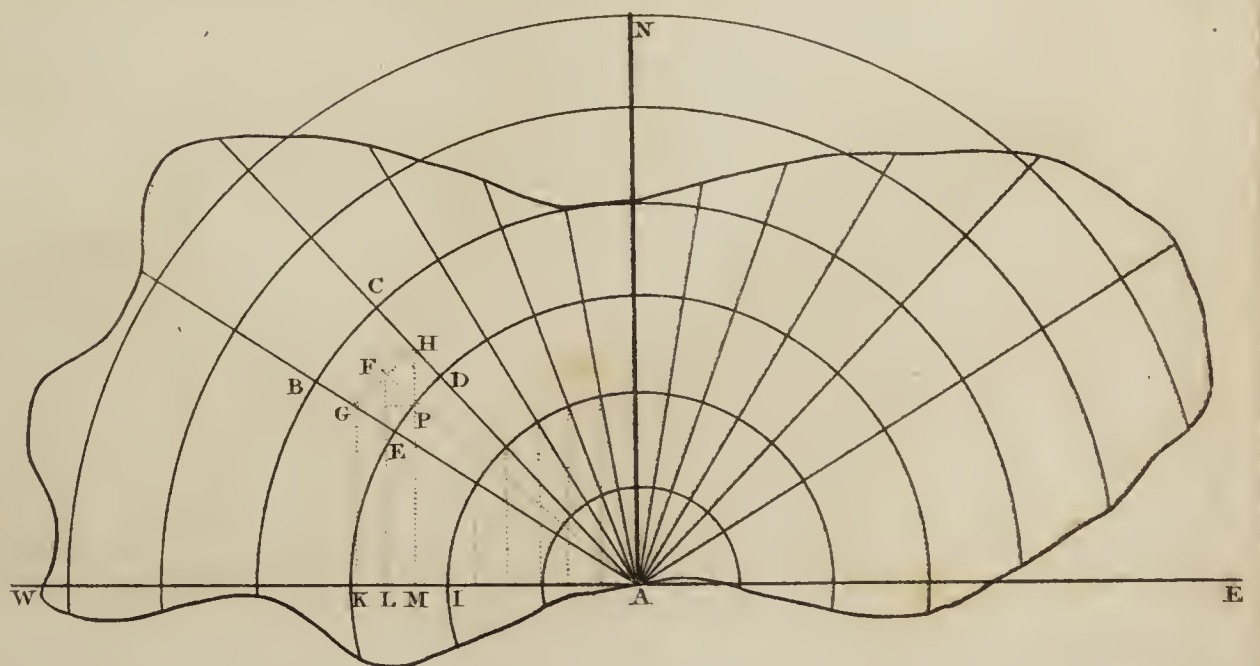
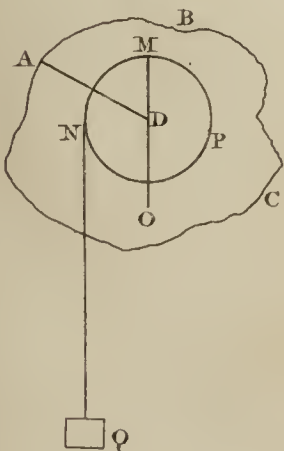
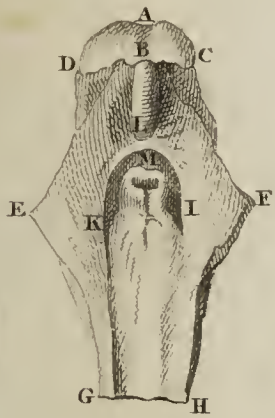
*Ballistic Pendulum.**Fig. 1. Pa. 285.**Fig. 2.**Pa. 356. &c.**Fig. 3.**Fig. 4.**Fig. 5.**Fig. 7. p. 372.**Fig. 6.**Fig. 8.**Fig. 9.**Fig. 11. Pa. 413.**Fig. 10.*

Fig. 1.



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Fig. 2.

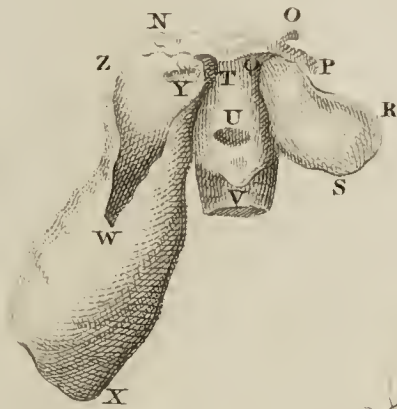


Fig. 6.



Fig. 7.

Pa. 526.

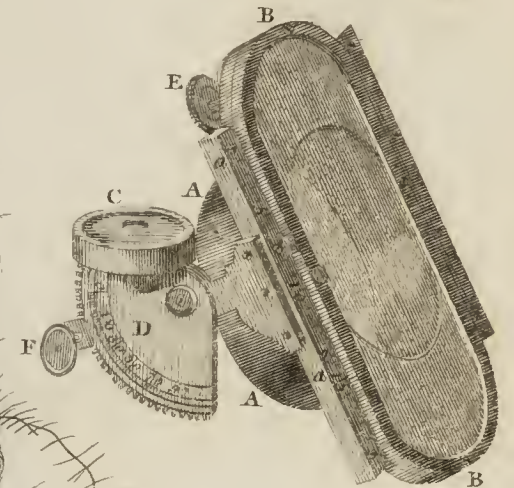


Fig. 3.

Page 510.



Fig. 4.



Page 510.

Fig. 5.

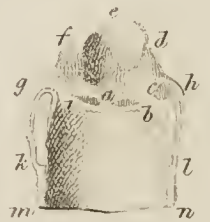


Fig. 8.

Page 559.

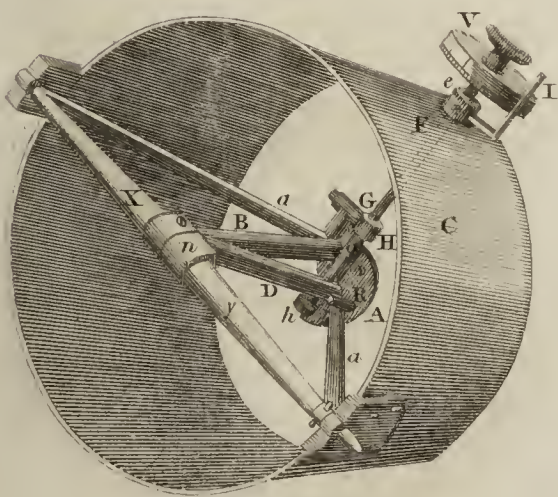
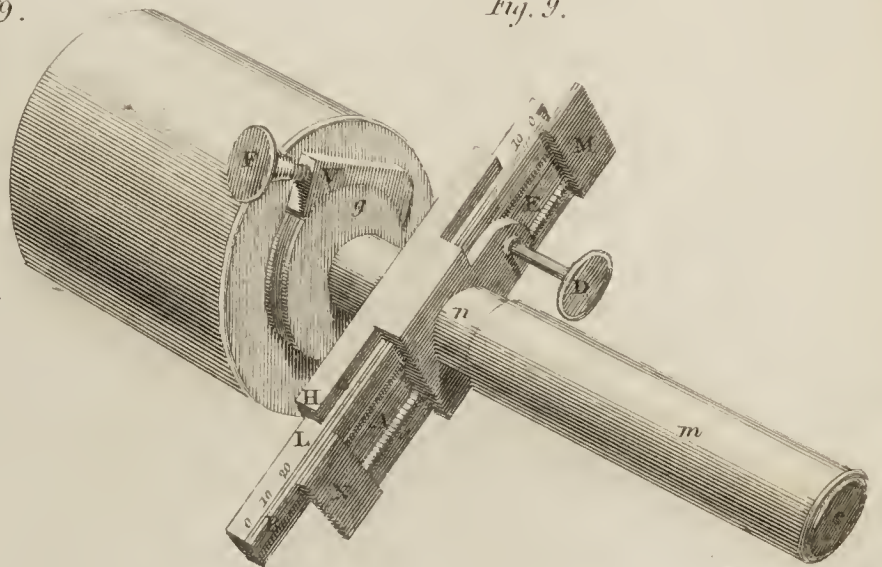
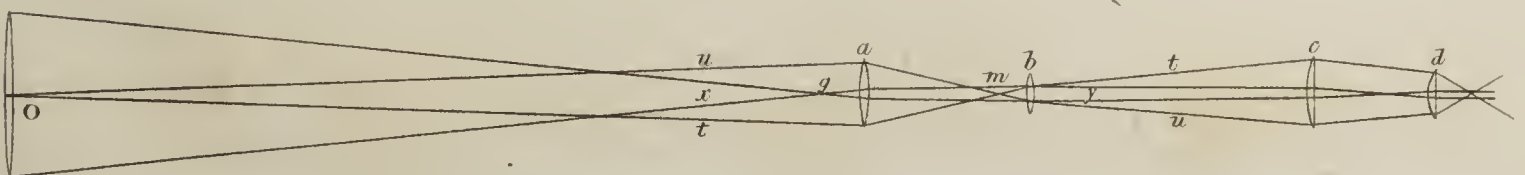


Fig. 9.



Page 562.

Fig. 10.



p. 577. to. 584.
Fig. 2.

Fig. 1.

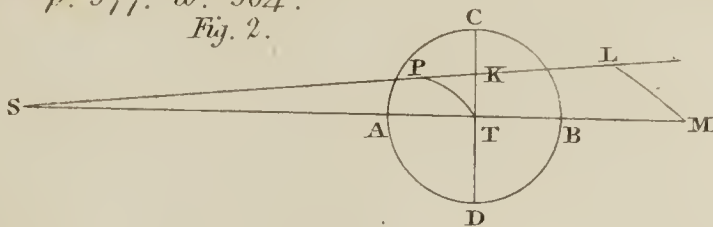
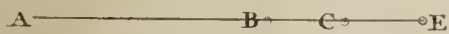


Fig. 3.

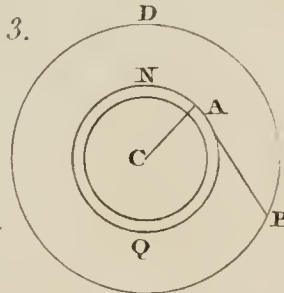


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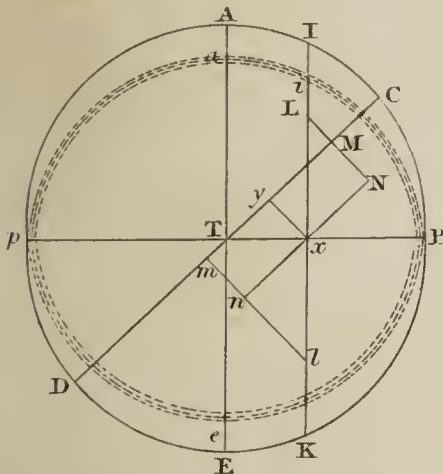


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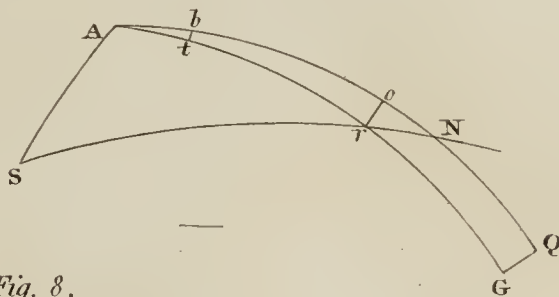


Fig. 6.

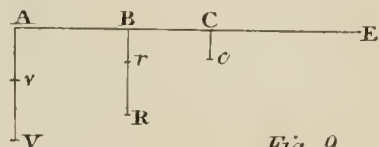
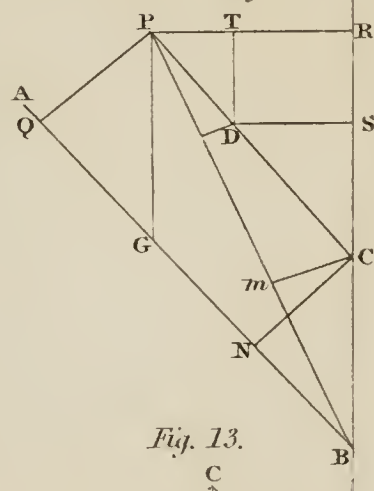


Fig. 9.



p. 603.
Fig. 10.

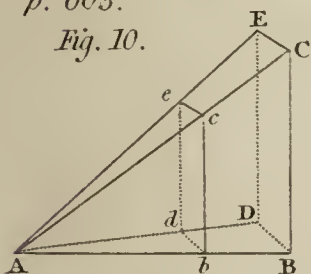


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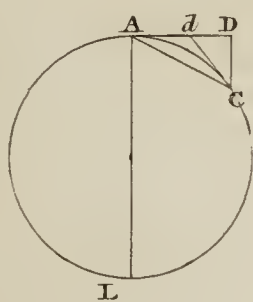
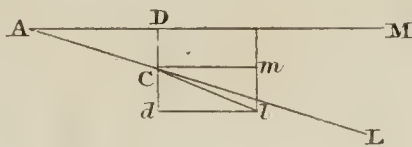


Fig. 7.



p. 604.

Fig. 14.

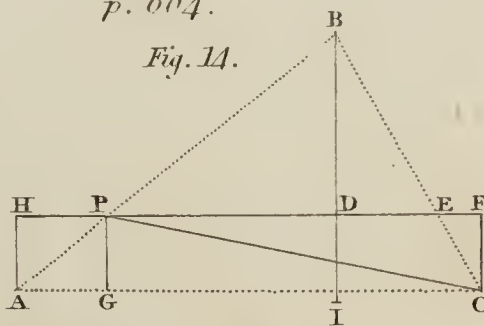


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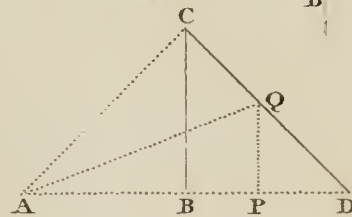


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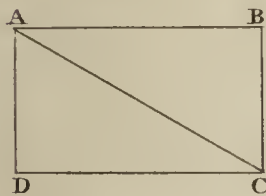


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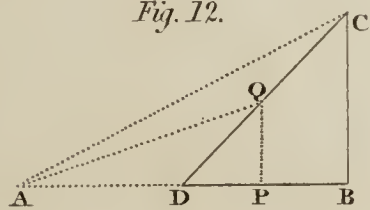


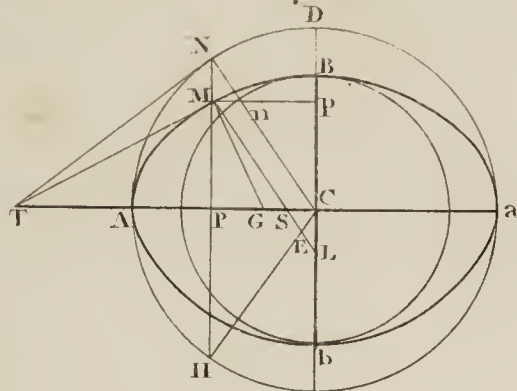
Fig. 15. p. 639.



Fig. 1. p. 680.



Fig. 2.



p. 701.

Fig. 3.

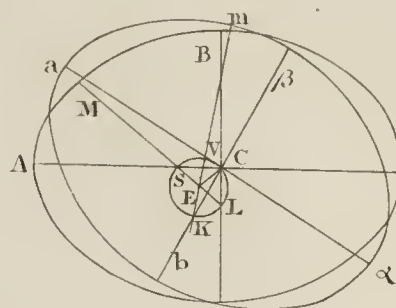


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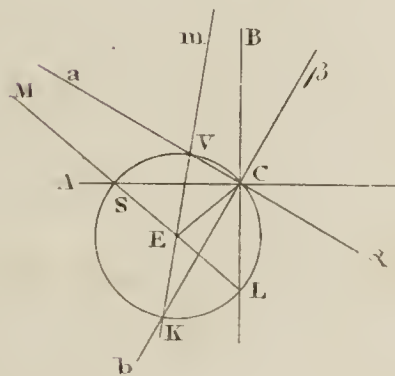


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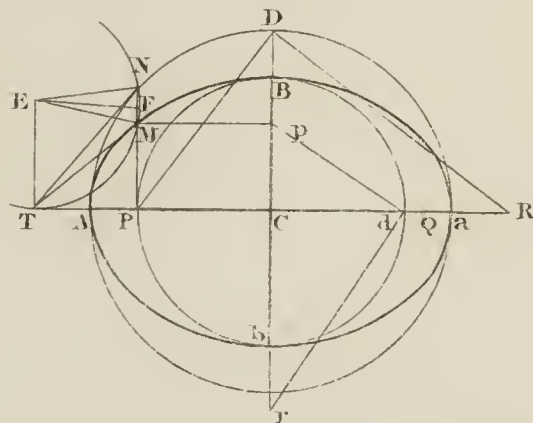


Fig. 6.

p. 717.

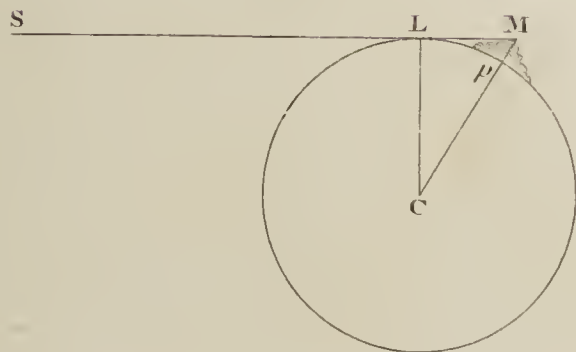


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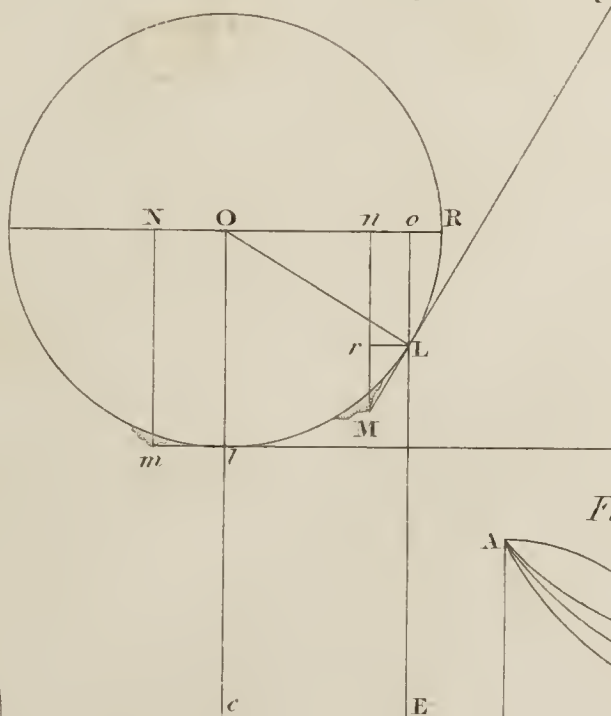


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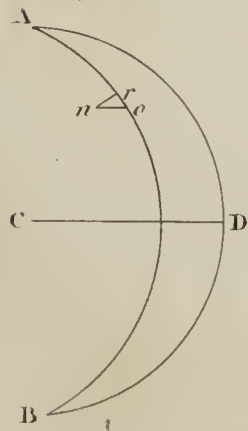


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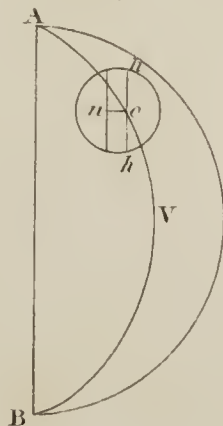


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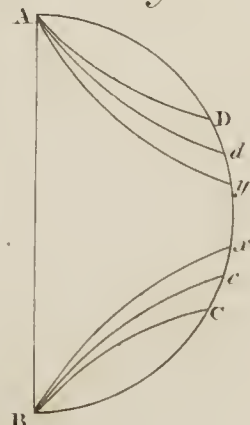


Fig. 1.

p. 727. to 737.

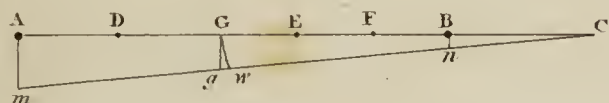


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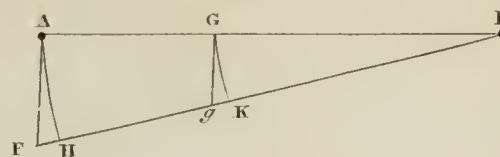


Fig. 3.

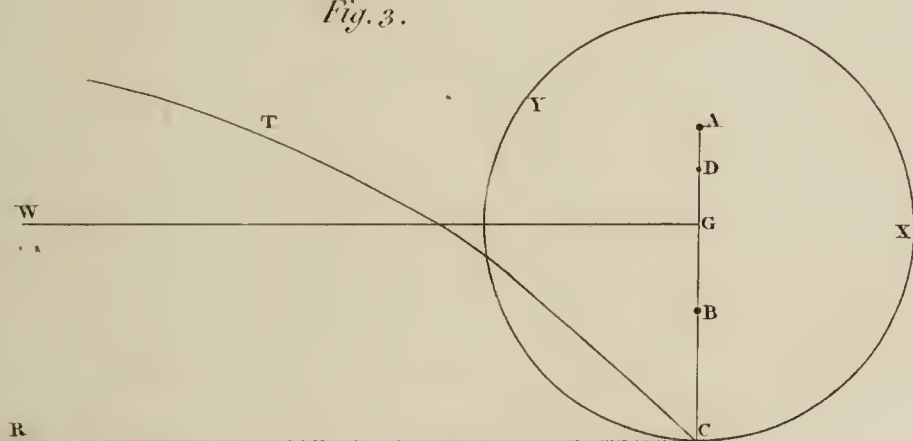


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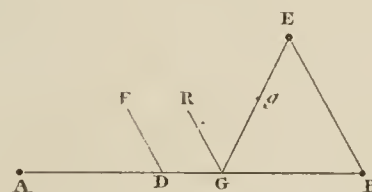


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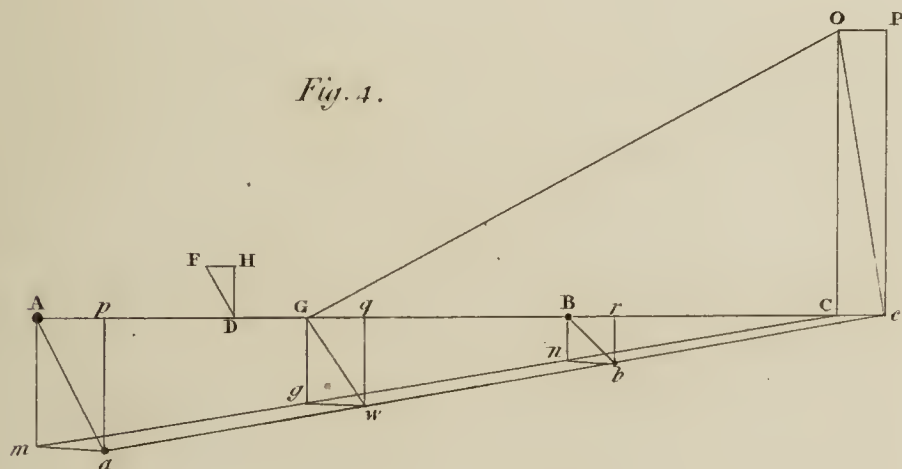


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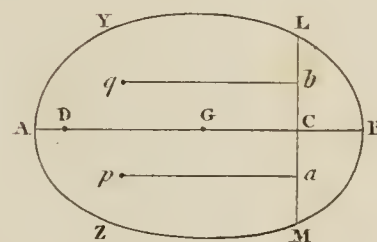


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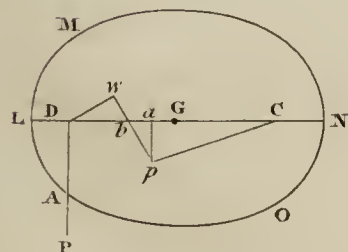


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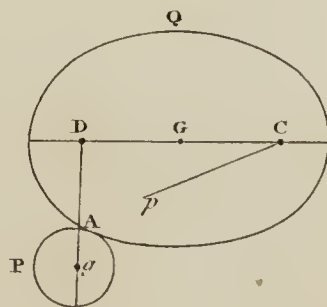
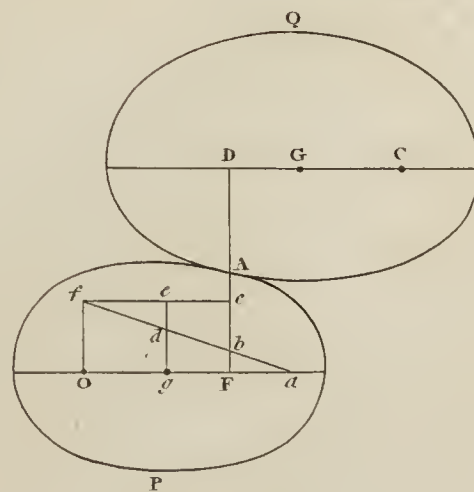


Fig. 9.



Mathew & Rayford

THE
PHILOSOPHICAL TRANSACTIONS

OF THE
ROYAL SOCIETY OF LONDON;

ABRIDGED.

V. An Account of Falkland Islands. By Wm. Clayton, Esq., of his Majesty's Navy. p. 99.

Falkland's Islands, or, as the Spaniards and French call them, the Maloine Islands, are situated between the latitude of $52^{\circ} 26'$ and $51^{\circ} 6'$ s. and longitude from London 56° to $60^{\circ} 30'$ w. They are very numerous, forming a mass of broken high lands, or very low sedgy keys and sunken rocks. The largest is the easternmost island, and on the eastern side the Spaniards had a settlement, which the crown of Spain purchased of M. Bougainville, who, on his private account, had formed a settlement in the year 1764, at the time that Commodore Byron had first discovered Port Egmont. The next large island is of a very considerable extent, and has many excellent harbours on it. Between these two runs Falkland's Sound, which is navigable through; but the south entrance is pretty full of low sandy keys. Adjoinin to the 2d large island, to the westward, lies Saunders's Island, on which the English settlement was made, a blockhouse erected, several spots inclosed for gardens, and 3 storehouses, with 5 dwelling-houses or huts, built at different times by the ships' crews who were stationed there. The harbour of Port Egmont was formed by these islands, and another high, barren, rocky island, named Kepple's Island, and some other lesser islands to the N. E. and eastward, and was entirely land-locked, or inclosed by the land, on every point; it was very spacious; the bottom was muddy and good holding ground. From the hills through the bogs drained several runs of water; and as the landing places were good, and a natural small cove for boats to lie in safety on the north side of Saunders' Island sheltered from the s. w. winds, it induced Captain Macbride to begin the settlement on it.

The larger islands are overspread with a short, tufty, round grass; a shrub with a smell like rosemary; a shrub of the myrtle kind, which in March and April blossoms; a white flower, of a faint violet smell; a small annual plant, of the wormwood kind. Near the shore, wherever there is a sandy soil, a species

of grass grows, called Penguin grass, from the birds of that species making their nests, and burrowing under ground like rabbits in holes. This grass grows 4 or 5 feet high; the blades are broad and coarse like rushes; the roots, when roasted, eat like almonds. Ground sorrel every where abounds in the greatest plenty, is extremely tart, and a most excellent antiscorbutic; the flower it produces is exactly like the wild rose which grows in the hedges in England. Celery, pepper-grass, and scurvy-grass, also abound on every island. Maidenhair, improperly so called, is plentiful; the berries are ripe in February and March, and very pleasant. A small species of cranberry abounds, and is the food of the wild geese all the autumn, when the geese are best. In the spring season, and part of the summer, there springs up an extremely pretty humble flower, which nearest resembles in leaf the auricula, but in flower the primrose; only they blow quite white. In very barren craggy spots, and even out of the cliffs of the rocks near the sea-shore, grows in the summer season, a small shrub which produces an uncommon but pretty flower, shaped like a lady's pocket; the colour is a rich yellow; the seeds are very small. Wood strawberries grow on these islands, and are ripe in March; are of an earthy insipid taste, and grow to the size of the common small strawberry in England. These are all the natural vegetable productions, and nothing rises to any size, nor does any tree grow on any of these islands.

The prevailing winds are from the s. to the w. for two-thirds of the year, and in general boisterous and stormy. The n. and n.w. winds are mild and warm; but seldom of long continuance. The winds from the n.e. are moist, foggy, and unwholesome. From e. to s. are most pernicious, blighting, and tempestuous; they affect man, bird, beast, and vegetation; nothing exposed can withstand it. Happily its duration is short, seldom continuing above 24 hours. It cuts the herbage down as if fires had been made under them; the leaves are parched up, and crumble to dust. The fowls are seized with cramps, and never recover; but continue to decline till the whole side is decayed which was first affected. Hogs and pigs are suddenly taken with the staggers, turn round and drop, never to recover. Men are oppressed with a stopped perspiration, heaviness at the breast, sore throats; but they soon get over it, by due care.

The sea abounds with mullets, and some of a very large size up to 10 pounds weight. Smelts in abundance, and as large as 14 and 15 inches in length; they may be taken with an angling line and rod. Transparent fish, shaped like a pike about the head, but not larger than a herring: these transparent fish are so clear when caught, that you may see through them; they have no red blood, but when cut a slimy water issues out, which may be their blood. There are 3 or 4 species of the common loggerhead, or sculpa fish, common on the English coasts. A small sand-crab, small cray-fish, are to be got. Muscles are plentiful, with

limpets, and a few small clams. The muscles are very large and fine, and no way dangerous. In the river on the large island, are small fish like trout, very delicious; and no other sort whatever.

The amphibious animals are of 4 kinds, though seemingly of the same genus; the sea-lion and the seal are distinct; the clapmatch seal and the fur seal are also distinct animals. The sea-lion and lioness are bull-faced, with long shaggy hair; the common seal is smooth; the clapmatch is best pictured in Lord Anson's voyage, under the name of sea-lion, in the drawings; the fur seal has its name from its coat, which is a fine soft fur, and is thinner skinned than any of the others. They all come on shore in December, to produce their young; and remain mostly on land till they engender again. During this season it is rather dangerous coming near them, for the males are then vicious, and will endeavour to hurt any one who approaches their females; but at all other times they endeavour to make to the water, where they are safe. In mild warm days, during the summer, they come on shore, and lie basking in the sun.

Mr. Clayton considers the penguins as amphibious animals, partaking of the nature of birds, beasts, and fishes. There are 4 kinds: the yellow, or king penguin; the red; the black or holey, from their burrowing under ground; and the jumping jacks, from their motion. These creatures generally live in the sea, have very short wings, which serve for fins, are covered with short thick feathers, and swim at an amazing rate. On shore they walk quite erect with a waddling motion, like a rickety child; and their breasts and bodies before being quite white, at a distance have, at first sight, the look of a child waddling along with a bib and apron on. They come on shore to lay and hatch their eggs in October; the yolks of the yellow, the holey, and jumping penguins, are yellow; but of the red penguins, it is red. All their eggs are good nourishing food, and a great refreshment to the seamen; but the flesh of these animals is coarse, fishy, and wholly unfit to eat. The only beast on these islands is a fox, very nearly resembling the English fox; it is now very shy and scarce to be got.

There are 3 sorts of wild geese: the mountain goose is somewhat larger than a Muscovy duck, feeds always on the mountains, is pleasant tasted, and preferable to the other sorts, but is scarce. Its plumage on the back is speckled with brown and black, of a greenish hue, and towards the neck turns of a glossy beautiful golden colour; the breast is coloured like a pheasant. The other goose feeds in the vallies on the wild cranberries and grass, and is as large as a tame goose; the gander is black and white speckled; the goose is almost like the mountain goose, but darker and not so beautiful. These are good food in general; but best and fattest in February, March, and April. Of the sea-goose, the gander is white, the goose mottled, black and white; they feed always on the sea-shore, and are scarcely eatable. Wild ducks, widgeon, teal, and the

shelldrakes, are the same as in Europe. But here is a species of ducks, called the loggerhead, from its large head. They have short wings, are unable to fly, and only swim and flap along on the water at an extraordinary rate. When driven ashore with boats they run fast, but soon squat down and are easily caught; they are eatable, though but indifferent food; they are of a dark brown dirty colour. Snipes are plenty, and so exceedingly tame that we could shy at them with sticks, and get a dish whenever we wanted. Of small birds there are several sorts: the red breast, speckled on the back like a partridge; the yellow breast, the white throat; the quaker, from its plumage being of the colour those people wear; the sparrow; tom-tit; linnets; and a bird like a goldfinch. Hawks are numerous; the eagle, the goshawk, the sparrow and the common hawk. Of every kind our crew ate, and found them very good and nourishing; owls are not numerous.

The latter end of September or beginning of October, the sea birds begin to come on shore to build nests and lay. The first which appear are the albatross, which are about the size of a large goose, quite white, except their wings, which are a dark brown; the bills are of a dirty yellow, about 3 inches long; very strong, and the edges sharp as a knife, hooked at the point; they breathe hard through 2 small holes in the bill close to the head, and frequently make a sound like a trumpet which children buy at fairs. Their wings are very long and narrow, with 4 joints in each wing, and extend 10 or 12 feet from tip to tip. Their feet are webbed, very thin, have 3 claws; on the outer claw are 4 joints, the middle 3, and the inner 1. They come to their nesting places by hundreds. They sit very tame, and some continually sound their bills. They never move off their nests let what will approach, and it was necessary to shove them off whenever they wanted their eggs. The egg is much larger than a goose's. The yolk is yellow; the white never boils hard, and always continues as clear as isinglass. The nests of these birds are made on the ground with earth; are round, about 1 foot high, and dented at top. While the hen sits, the male keeps constantly on the wing, and morning and evening returns with food to her. As soon as these birds have hatched, and the young ones are able to leave their nests, the jumping penguins repair to the nests and occupy them. The young albatrosses remain among them, while the old ones go and seek food, with which they regularly return morning and evening. The season for every species of birds, wild and tame, laying and hatching, is from Sept. to Dec. or Jan., and as all the eggs are very eatable, navigators, touching at these islands in those months, will meet great refreshment. In those 3 months we never meddled with the land geese, as they were breeding and could not be good.

Over the several islands is a surprising species of vegetation. It somewhat resembles molehills in the marshy grounds in England. It is circular, sometimes

6 feet round, sometimes less. From the surface oozes out a gum in round blebs, of the smell and taste of balsam copaiva. The body of these hills is formed within by a number of small substances, like the cones of pines. The outside is crusted over with dark green small leaves, running into each other, and cemented as if with glue. Mr. C. opened several, and found that no vermin formed them, but there actually was a kind of vegetation: the wild cranberries vegetate when the seed is lodged on them. Fern abounds, but of a weaker sort than that in Europe. They tried the furze seed, and it came up; but so weak and poor that it would never increase or thrive. The season for sowing all kinds of garden seeds is about 3 weeks later than directed in the spring or fall by Miller, remembering to reverse his months, calling September, March, &c.; but all kind of culinary herbs and roots came to as great perfection as in England, and in great plenty, only they were forced to shelter every bed in the garden, by a good sod wall, from the s. w. and s. e. winds, as much as possible.

The soil is in general boggy, barren, and rocky; but affords good pasturage in the vallies, and level spots for sheep and goats, and would for cattle, which might be out all winter; for that season is more remarkable for its mildness than in the same degree of northern latitude. The summer is as remarkably cold, and both proceeds from the prevailing winds; in the winter the n. and n. e. winds are frequent, which brings warm, mild, moist weather. In the summer, the s. and s. w. and s. e. prevails, which are cold, sharp, and blighting; but in general, throughout the year, there is very little difference in the weather, but mostly cold. The thermometer scarcely ever exceeds 64° in the warmest days, and very seldom in winter is below the freezing point, though Mr. C. observed it 20° below freezing; but that did not continue long, nor does the snow continue in the plains or vallies a week together, or frost last so long; but the weather in winter is perpetually changing; on the hills the snow lies for 9 months.

There is a great plenty, and some variety of moss on all the islands, and most of it when wet with water dyes of a brick-dust red. He tried it with other liquids, but found it still the same. The coasts abounds with whales of the spermaceti kind; the islands with innumerable seals and sea-lions, from which a valuable fishery might be carried on. The passage out is 12 weeks: the same home. Ships might be loaded with oil ready made in 6 or 8 weeks, and the price of that article greatly reduced.

VI. Short and Easy Theorems for finding, in all cases, the Differences between the Values of Annuities payable Yearly, and of the same Annuities payable Half Yearly, Quarterly, or Momently. By the Rev. Richard Price, D. D., F. R. S. p. 109.

The values of annuities, as given in all the common tables, suppose them

paid yearly. But it is well known, that generally they are paid half-yearly, and sometimes quarterly; and that this is a circumstance which always adds to their value. The difference between the values of annuities, according as they are paid in these different ways, being nowhere stated with accuracy, Dr. P. has thought that an attempt to do this may be of some use. But, for particulars, see the last edition of Dr. P.'s Treatise on Reversionary Payments.

From the rules and examples there given it may be gathered, that the difference between the values of annuities on lives payable yearly, half-yearly, quarterly, and momentarily, increases continually with the ages; but, if not secured by land, this difference can never be so great as a quarter of a year's purchase in the case of annuities payable yearly and half-yearly; $\frac{1}{8}$ of a year's purchase in the case of annuities payable yearly and quarterly; and half a year's purchase in the case of annuities payable yearly and momentarily.

Mr. Simpson, in his Treatise on the Doctrine of life-annuities, p. 78, and in his Select Exercises, p. 283, has given a quarter of a year's purchase as the addition always to be made to the value of a life-annuity payable yearly, in order to obtain its value payable half yearly; and $\frac{3}{8}$ of a year's purchase, if its value payable quarterly is required. But it appears that these are too large additions; and, whatever be the rate of interest, or the number of lives, a 5th of a year's purchase will be generally more than a sufficient addition, if the value of the annuity is desired payable half-yearly; and $\frac{3}{10}$ of a year's purchase, if the value of the annuity is desired payable quarterly. Mr. De Moivre's rules, in p. 85 of his book on life-annuities, for finding the values of life-annuities payable half-yearly and quarterly from their values payable yearly, are still less correct; for they suppose the difference between these values the same, whether the annuities are life-annuities, or annuities certain.

Mr. Dodson, in the 1st question in the 3d volume of his Mathematical Repository, has given a rule for finding the value of an annuity secured by land, and payable yearly, which coincides with that here given; and Mr. De Moivre, in p. 338 of his Treatise on the Doctrine of Chances, has given a theorem for this purpose, which also brings out nearly the same answers. But Mr. Simpson, in prob. 1, p. 323, of his Select Exercises, makes the excess of the value of such an annuity above the value of an annuity payable yearly, but not secured by land, double to the same excess derived from Mr. Dodson's and Mr. De Moivre's rules. The truth is, that Mr. Dodson's rule gives the exact value; and that Mr. Simpson's problem gives the value, not of an annuity secured by land and payable yearly, but of an annuity secured by land and payable momentarily; and also, that his method of solution implies a rate of interest somewhat less when the annuity is payable momentarily, than when it is payable yearly.

VII. An Account of the Romansh Language. By Joseph Planta, F.R.S.*
p. 129.

The Romansh language, is that now spoken in the most mountainous parts of the country of the Grisons, near the sources of the Rhine and the En. It consists of 2 main dialects; which, though partaking both of the above general name, yet differ so widely as to constitute in a manner 2 distinct languages. Books are printed in both of them; and each, though it be universally understood in its respective district, is yet subdivided into almost as many secondary dialects as there are villages in which it is spoken: which differ however but little, except in the pronunciation. One of the main dialects, which is spoken in the Engadine, a valley extending from the source of the En to the frontiers of the Tyrolese, is by the inhabitants called Ladin. It admits of some variation, even in the books, according as they are printed either in the upper or the lower part of this province. The other dialect, which is the language of the Grey, or Upper League, is distinguished from the former by the name of Cialover: and in the very centre, and most inaccessible parts of this latter district, there are some villages situated in narrow vallies, called Rheinwald, Cepina, &c. in which a 3d language is spoken, more similar to the German than to either of the above idioms, though they are neither contiguous, nor have any great intercourse with the parts where the German is used.

It being impossible to form any idea of the origin and progress of a language, without attending to the revolutions that may have contributed to its formation and subsequent variations; and this being particularly the case in the present instance, where no series of documents is extant to guide our researches; Mr. P. briefly recapitulates the principal events which may have affected the language of the Grisons, as they are related by authors of approved veracity.

Ambigatus, the first king of the Celtic Gaul on record, who about 400 years before Christ, governed all the country situated between the Alps and the Pyrenæan mountains, sent out 2 formidable armies under the command of 2 of his nephews; one of whom, named Segovisius, forced his way into the heart of Germany: and the other, Bellovisius, having passed the Alps, penetrated into Italy as far as the settlements of the Tuscans, which at that time extended over the greatest part of the country now called Lombardy. These, and several other swarms of invaders whom the successes of the former soon after attracted, having totally subdued that country, built Milan, Verona, Brescia, and several other considerable towns, and governed with such tyrannic sway, especially over the nobility, whose riches they coveted and sought by every means to extort from them, that most of the principal families, joining under the conduct of Rhætus,

* Now chief librarian of the British Museum.

one of the most distinguished personages among them, retired with the best part of their effects and attendants among the steepest mountains of the Alps, near the sources of the Rhine, into the district now called the Grey League. The motive of their flight, their civil deportment, and perhaps more so the wealth they brought with them, procured them a favourable reception from the original inhabitants of that inhospitable region, who are mentioned by authors as being a Celtic nation, fabulously conjectured from their name (*Λειποντίοι*, *relicti*) to have been left there by Hercules in his expedition into Spain.

The new adventurers had no sooner climbed over the highest precipices, but thinking themselves secure from the pursuits of their rapacious enemies, they fixed in a valley which, from its great fertility in comparison of the country they had just passed, they called *Domestica*. They intermixed with the old inhabitants, and built some towns and many castles, whose present names manifestly bespeak their origin. They soon after spread all over the country, which took the name of *Rhætia* from that of their leader; and introduced a form of government similar to their own, of which there are evident traces at this day, especially in the administration of justice; in which a *Laertes*, or President, now called *Landamman*, or *Ministral*, together with twelve *Lucumones*, or jurors, determine all causes, both civil and criminal: and *Livy*, though he erroneously pretends that they retained none of their ancient customs, yet allows that they continued the use of their language, though somewhat adulterated by a mixture with that of the *Aborigines*.

Several Roman families, dreading the fury of the Carthaginians under *Hannibal*; and perhaps since, during the rage of the civil wars, and the subsequent oppressive reigns, interior commotions and foreign invasions, forsook the *Latium* and *Campania*, and resorted for a peaceful enjoyment of their liberty, some into the islands where *Venice* now stands; and many into the mountains of the *Grisons*, where they chiefly fixed their residence in the *Engadine*, as appears not only from the testimonies of authors, but also from the names of several places and families which are evidently of Roman derivation. The inhabitants these emigrants found in that place of refuge, could not but be a mixture of the *Tuscans* and original *Lepontii*: and the 2 languages which met on this occasion, must at the very first have had some affinity; as the *Tuscan*, which derived immediately from the *Greek*, is known to have had a great share in the formation of the *Roman*. But as it is generally observed, that the more polished people introduce their native tongue wherever they go to reside in any considerable numbers, the arrival of these successive colonies must gradually have produced a considerable change in the language of the country in which they settled; and this change gave rise to the dialect since called *Ladin*, probably from the name of the mother country of its principal authors.*

Though the name of Romansh, which the whole language bears, seem to be a badge of Roman servitude, yet the conquest of that nation, if ever effected, could not have produced a great alteration in a language which must already have been so similar to their own; and its general name may as well be attributed to the pacific as to the hostile Romans. But when we consider that a coalition of the 2 main dialects, which differ so far as not to be reciprocally understood, must have been the inevitable consequence of a total reduction; and that such a coalition is known never to have taken place, we may lay the greater stress on the many passages of ancient authors, in which it is implied that the boasted victories of the Romans over the Rhæti, for which public honours had been decreed to L. Múnatus, M. Anthony, Drusus, and Augustus, amounted to no more than frequent repulses of those hardy people into their mountains; out of which their want of sufficient room and sustenance, (which in our days drives considerable numbers of them into the services of foreign powers) compelled them at times to make desperate excursions in quest of necessities. And we may also from these collected authorities be induced to give the greater credit to the commentator of Lucan, and to the modern historians, who positively assert, that the people living near the sources of the Rhine and the En were never totally subdued by the Roman arms; but only repelled in their attempts to harass their neighbours.

This whole country however, from its central situation, could not but be annumerated to one of the provinces of the empire, and accordingly we find that Rhætia itself (which by the accounts of ancient geographers appears to have extended its limits beyond the lake of Constance, Augsburg, and Trent, towards Germany, and to Como and Verona towards Italy) was formed into a Roman province, governed by a pro-consul or procurator, who resided at Augsburg; and that when, in the year 119, the emperor Adrian divided it into Rhætia prima and secunda, the governor of the former, in which the country now spoken of must have been comprized, took up his residence in 2 castles situated where Coire now stands, while the other continued his seat at Augsburg. But notwithstanding these appearances, no trace or monument of Roman servitude is to be met with in this district, except the ambiguous name of one mountain, situated on the skirts of these highlands, and generally thought to have been the non plus ultra of the Roman arms on the Italian side.

From the difficulty those persevering veterans experienced in keeping this stubborn people in awe, Mr. P. infers that such strenuous assertors of their in-

* A parallel instance of the formation of a language by Roman colonies is the idiom of Moldavia; which, according to Prince Cantemir's account of that country, has still many traces of its Latin origin, and which, though engrafted on the Dacian, and since on the Slavonian dialects of the Celtic, may still be considered as a sister language to that here treating of.—Orig.

dependence, whom the flattering pens of Ovid and Horace represent as formidable even to Augustus, and preferring death to the loss of their liberties, favoured by the natural strength and indigence of their country, were not very likely to be so far subdued by any foreign power inferior to the Roman, as to suffer any considerable revolution in their customs and language: for as to the irruptions of the Goths, Vandals, and Lombards, in the 5th and 6th centuries, besides a profound silence in history concerning any successful attempt of those barbarians on this spot, it is hardly credible, that any of them should have either wished or endeavoured to settle in a country, perhaps far less hospitable than that they had just forsaken, especially after they had opened to themselves a way into the fertile plains of Lombardy.

Some stress must be laid on this inference, as the history of what befel this country, after the decline of the Roman empire, is so intimately blended with that of Suabia, the Tyrolese, and the lower parts of the Grisons, which are known to have fallen to the share of the rising power of the Franks, that nothing positive can be drawn from authors as to the interior state of this small tract. The victory gained in the year 496 near Cologne, by Clovis I. king of the Franks, over the Almain, who had wrested from the Romans all their dominions on the northern side of the Alps; and the defeat of both Romans and Goths in Italy, in the year 549, by the treacherous arms of Theodebert king of Austrasia, whose dominions soon after devolved to the crown of France, necessarily gave the aspiring Merovingian race a great ascendancy over all the countries surrounding the Grisons; and accordingly we find that this district also was soon after, without any military effort, considered as part of the dominions of the reviving western empire. But it does not appear that those monarchs ever made any other use of their supremacy in these parts than, agreeable to the feudal system which they introduced, to constitute dukes, earls, presidents and bailiffs, over Rhætia; to grant out tenures on the usual feudal terms; and consequently to levy forces in most of their military expeditions. It must however be observed, that these feudal substitutes were seldom, if ever, strangers: those who are on record to the latter end of the 8th century having all been chosen from among the nobility of the country. And that no foreign garrisons were ever maintained for any continuance of time in these parts, appears from a circumstance related by their annalists; who say, that an inroad of the Huns in 670, when external forces would probably have been very acceptable to the natives, was repulsed merely by a concourse of the inhabitants.

History continues to furnish us with proofs of the little connection this people had with other nations in their domestic affairs, notwithstanding their dependence on a foreign power. In the year 780, the bishop of Coire, who by the constitution of that see can only be a native, obtained from Charlemain,

besides many considerable honours and privileges in the empire, a grant of the supreme authority in this country, by the investiture of the office of hereditary president or bailiff over all Rhætia. His successors not only enjoyed this prerogative to the extinction of the Carlovingian race of emperors in 911, but received accumulated favours from other succeeding monarchs, as the bigotted devotion of those times or motives of interest prompted them. And so far did their munificence gradually extend, that the sole property of one of the 3 leagues was at one time vested in the hands of the bishop. This prelate and the nobles, the greatest part of whom became his retainers, availed themselves, like all the German princes, of the confusion, divisions, and interreigns which frequently distracted the empire in the succeeding centuries, in order to establish a firm and unlimited authority of their own. Henceforth the annals of this country furnish us with little more than catalogues of the bishops and dukes, who were still, at times, nominated by the emperors; and of the domains granted out by them to different indigene families; with accounts of the atrocious cruelties exercised by these lords over their vassals; and with anecdotes of the prowess of the natives in several expeditions into Italy and Palestine, in which they still voluntarily accompanied the emperors. The repeated acts of tyranny exercised by those arbitrary despots, who had now shaken off all manner of restraint, at length exasperated the people into a general revolt, and brought on the confederacy; in which the bishop and most of the nobles were glad to join, in order to screen themselves from the fury of the insurgents. The first step towards this happy revolution, was made by some venerable old men dressed in the coarse grey cloth of the country, who in the year 1424 met privately in a wood near a place called Truns, in the Upper League; where, impressed with a sense of their former liberties, they determined to remonstrate against, and oppose the violent proceedings of their oppressors. The abbot of Dissentis was the first who countenanced their measures; their joint influence gradually prevailed over several of the most moderate among the nobles; and hence arose the League which, from the colour of its first promoters, was ever after called the Grey League: which, from its being the first in the bold attempt to shake off the yoke of wanton tyranny, has ever since retained the pre-eminence in rank before the 2 other leagues; and which has even given its name to the whole country, whose inhabitants, from the circumstances of their deliverance, pride themselves in the appellation of Grisons, or the grey ones. From this period nothing has ever affected their freedom and absolute independence; which they now enjoy in the most unlimited sense, in spite of the repeated efforts of the house of Austria to recover some degree of ascendancy over them.

From this concise view of the history of the Grisons, it appears, that as no foreign nation ever gained any permanent footing in the most mountainous parts

of this country, since the establishment of the Tuscans and Romans, the language now spoken could never have suffered any considerable alteration from extraneous mixtures of modern languages. And to those who may object, that languages, like all other human institutions will, though left to themselves, be inevitably affected by the common revolutions of time, it may be observed, that a language, in which no books are written, but which is only spoken by a people chiefly devoted to arms and agriculture, and consequently not cultivated by the criticisms of men of taste and learning, is by no means exposed to the vicissitudes of those that are polished by refined nations; and that, however paradoxical it may appear, it is nevertheless true, that the degeneracy of a language is more frequently to be attributed to an extravagant refinement, than to the neglect of an illiterate people, unless indeed external causes interfere. May we not hence conclude, that as the Romansh has never been used in any regular composition in writing, till the 16th century, nor affected by any foreign invasion or intimate connection, it is not likely to have received any material change before the period of its being written? And we have the authority of the books since printed to prove that it is at present the identical language that was spoken 200 years ago. These arguments will receive additional weight from the proofs hereafter given of the great affinity there is between the language as it is now spoken, and the Romance that was used in France 9 centuries ago.

When we further consider the facts above briefly related, the wonder will cease, that in a cluster of mountains, situated in the centre of Europe, a distinct language (not a dialect or jargon of those spoken by the contiguous nations, as hath been generally imagined) should have maintained itself through a series of ages, in spite of the many revolutions which frequently changed the whole face of the adjacent countries. And indeed, so obstinately tenacious are these people of their independence, laws, customs, and consequently of their very language, that, as hath been already observed, their form of government, especially in judicial matters, still bears evident marks of the ancient Tuscan constitution; and that, though they be frequently exposed to inconveniences from their stubbornness in this respect, they have not yet been prevailed on to adopt the Gregorian reformation of the calendar.

As to the nature of this language, it may now be advanced, with some degree of confidence, that the Cialover owes its origin to a mixture of the Tuscan and of the dialect of the Celtic spoken by the Lepontii; and that the introduction of the vulgar Roman affected it in some degree, but particularly gave rise to the Ladin; the vocabulary of which has a great affinity with that of the Latin tongue. But these assertions rest merely on historical evidence; for as to the Cialover, all that it may have retained of the Tuscan or Roman, is so much disfigured by an uncouth pronunciation, and a vague orthography, that all etymo-

logical inquiries are thus rendered intricate and unsatisfactory. And as to the Ladin, though its derivation be more manifest, yet we are equally at a loss from what period or branch of the Latin tongue to trace its real origin : for even the vocabulary, in which the resemblance is most evident, differs equally from the classical purity of Tully, Cæsar, and Sallust, as it does from the primitive Latin of the 12 tables of Ennius, and the *columna rostralis* of Duillius, which has generally been thought the parent of the Gallic Romance ; as also from the trivial language of Varro, Vegetius, and Columella. May we not from this circumstance infer, that, as is the case in all vernacular tongues, the vulgar dialect of the Romans, the *sermo usualis, rusticus, pedestris*, of which there are no monuments extant, differed very widely, both in pronunciation and construction, from that which has at any time been used, either in writing or in the senate ?

The grammatical variations, the syntax, and the genius of the language, must in this, as well as in several other modern European tongues, have been derived from the Celtic ; it being well known, that the frequent use of articles, the distinction of cases by prepositions, the application of 2 auxiliaries in the conjugation, do by no means agree with the Latin turn of expression ; though a late French academician, who hath taken great pains to prove that the Gallic Romance was solely derived from the Roman, quotes several instances in which even the most classical writers have in this respect offended the purity of that refined language. It cannot here be denied, * that as new ideas always require new signs to express them, some foreign words, and perhaps phrases, must necessarily, from time to time, have insinuated themselves into the Romansh by the military and some commercial intercourse of the Grisons with other nations ; and this accounts for several modern German words which are now incorporated into the language of the Engadine.*

The little connection there is in mountainous countries between the inhabitants of the different valleys, and the absolute independence of each jurisdiction in this district, which still lessens the frequency of their intercourse, also accounts, in a great measure, for the variety of secondary dialects subsisting in almost every different community or even village.

The oldest specimens of writing in this language, are some dramatical performances in verse on scriptural subjects, which are extant only in manuscript. The histories of Susanna, of the Prodigal Son, of Judith and Holofernes, and of Esther, are among the first ; and are said to have been composed about the year 1560. The books that have since been printed are chiefly on religious subjects ; and among those that are not so, the only ones Mr. P. ever heard of, are

* Tapferdà, Tapferkeit, Bravery ; Nardà, Narheit, Folly ; Elinot, Kleinod, a Jewel ; Graf, Graf, a Count ; Baur, Baur, a Peasant, &c.—Orig.

a small code of the laws of the country in the Cialover dialect, and an epitome of Sprecher's Chronicle, by Da Porta, in the Ladin.

The language spoken in Gaul, from the 5th to the 12th centuries, being evidently a mixture of the same Roman and Celtic ingredients, and partaking of the same name with those of the Grisons, Mr. P. enters into a few particulars concerning it, as it seems to have been an essential part, or rather the trunk, of the language under consideration. One of the many instances of how little the laboured researches of philologists, into the origin of languages, are to be depended on, is the variety of opinions entertained by French authors concerning the formation of the Gallic Romance. A learned Benedictine first starts the conjecture, and then maintains it against the attacks of an anonymous writer, that the vulgar Latin became the universal language of Gaul immediately after Cæsar's conquest, and that its corruption, with very little mixture of the original language of the country, gradually produced the Romance towards the 8th century. Bonamy, on the other hand, is of opinion, that soon after that conquest, a corruption of vulgar Latin by the Celtic formed the Romance, which he takes to be the language always meant by authors when they speak of the *Lingua Romana* used in Gaul. The author of the Celtic Dictionary tells us, that the Romance is derived from the Latin, the Celtic, which he more frequently calls Gallic, and the Teutonic; in admitting of which latter he deviates from most other authors, who deny that the Teutonic had any share in the composition of the Romance, since the Franks found it already established when they entered Gaul, and were long before they could prevail on their new subjects to adopt any part of their own mother tongue, which however appears to have been afterwards instrumental in the formation of the modern French. Duclos, guided perhaps by Du Cange, whose opinion appears to be the most sober and best authenticated, maintains that the vulgar Latin was undoubtedly the foundation of the Romance; but that much of the Celtic gradually insinuated itself in spite of the policy of the Romans, who never failed to use all their endeavours to establish their language wherever they spread their arms. Among this variety of conjectures and acute controversies, it is however agreed on all hands, that the vocabulary of the Roman, and the idiom of the Celtic have chiefly contributed to the formation of the Gallic Romance, which is sufficient to prove that it partakes of a common origin with that of the Grisons.

There are incontestible proofs that this language was once universal all over France; and that this, and not immediately the Latin, has been the parent of the Provençal, and afterwards of the modern French, the Italian, and the Spanish. The oath taken by Lewis the Germanic, in the year 842, in confirmation of an alliance between him and Charles the Bald, his brother, is a decisive proof of the general use of the Romance by the whole French nation at that

time, and of their little knowledge of the Teutonic, which being the native tongue of Lewis, would certainly have been used by him in this oath, had it been understood by the French to whom he addressed himself. But Nithardus, a cotemporary writer, and near relation to the contracting parties, informs us, that Lewis took the oath in the Romance language, that it might be understood by the French nobility, who were the subjects of Charles; and that they, in their turn, entered into reciprocal engagements in their own language, which the same author again declares to have been the Romance, and not the Teutonic; though one would imagine that, had they at all understood this latter tongue, they could not but have used it on this occasion, in return for the condescension of Lewis.

While the Grisons neglected to improve their language, and rejected, or indeed were out of the reach of every refinement it might have derived from polished strangers, the taste and fertile genius of the Troubadours, fostered by the countenance and elegance of the brilliant courts and splendid nobility of Provence, did not long leave theirs in the rough state in which we find it in the 9th century. But the change having been gradual and almost imperceptible, the French historians have fixed no epocha for the transition of the Romance into the Provençal. That the former language had not received any considerable alteration in the 12th century, may be gathered from a comparison of the two: and that it still bore the same name, appears from the titles of several books which are said to have been written in, or translated into the Romance. But though mention is made of that name even after this era, yet on examining impartially what is given us for that language in this period, it will be found so different from the Romance of the 9th century, that to trace it any further would be both a vain and an extravagant pursuit.

Admitting however the universal use of the Romance all over France down to the 12th century, which no French author has yet doubted or denied; and allowing that what the writers of those times say of the Gallic is to be understood of those of the Romance, as appears from chronological proofs, and the expressions of several authors prior to the 5th century; who, by distinguishing the Gallic, both from the Latin and the Celtic, plainly indicate that they thereby mean the Romance, those being the only 3 languages which, before the invasion of the Franks, could possibly have been spoken, or even understood in Gaul: admitting these premises then, it necessarily follows, that the language introduced into England under Alfred, and afterwards more universally established by Edward the Confessor, and William the Conqueror, must have been an emanation of the Romance, very near akin to that of the abovementioned oath, and consequently to that which is now spoken in the Alps.

The intercourse between Britain and Gaul is known to have been of a very

early date; for even in the first century we find, that the British lawyers derived the greatest part of their knowledge from those of the continent; while, on the other hand, the Gallic Druids are known to have resorted to Britain for instruction in their mysterious rites. The Britons therefore could not be totally ignorant of the Gallic language. And hence it will appear that Grimbald, John, and the other doctors, introduced by Alfred, could find no great difficulty in propagating their native tongue in this island; which tongue, at that interval of time, could only be the true Romance, since they were cotemporaries with Lewis the Germanic. That the Romance was almost universally understood in this kingdom under Edward the Confessor, it being not only used at court, but frequently at the bar, and even sometimes in the pulpit, is a fact too well known and attested, to need further authenticating it with superfluous arguments and testimonies. Duclos, in his history of the Gallic Romance, gives the above-mentioned oath of Lewis as the first monument of that language. The 2d he mentions is the code of laws of William the Conqueror, whom the least proficient in the English history knows to have rendered his language almost universal in this kingdom. If we may credit Du Cange, who grounds his assertion upon various instruments of the kings of Scotland during the 12th century, the Romance had also penetrated into that kingdom before that period.

The same corruption, or coalescence, which gave rise to the Gallic Romance, and to that of the Grisons, must also have produced in Italy a language, if not perfectly similar, at least greatly approaching to those 2 idioms. Nor did it want its Northern nations to contribute what the 2 other branches derived from that source. But be the origin what it will, certain it is, that a jargon very different from either the Latin or the Italian was spoken in Italy from the time of the irruptions of the barbarians to the successful labours of Dante and Petrarca; that this jargon was usually called the vulgar idiom; but that Speroni, the father of Italian literature, and others, frequently call it the common Italian Romance. And if Fontanini's authorities be sufficient, it appears that even the Gallic Romance, by the residence of the Papal Court at Avignon, and from other causes, made its way into Italy before it was polished into the Provençal.

As to Naples and Sicily, the expulsion of the Saracens by the Normans, under Robert Guiscard in 1059, must have produced in that country nearly the same effect, that a similar event soon after brought about in England. And in fact we have the authority of William of Apulia to prove, that the conquerors used all their efforts to propagate their language and manners among the natives, that they might ever after be considered only as one people. And Hugo Falcland relates, that in the year 1150, Count Henry refused to take upon him the management of public affairs, under pretence of not knowing the language of the French; which, he adds, was absolutely necessary at court.

That the language of the Romans penetrated very early into Spain, appears evidently from a passage in Strabo, who asserts, that the Turditani, inhabiting the banks of the Boetis, now the Guadalquivir, forgot their original tongue, and adopted that of the conquerors. That the Romance was used there in the 14th century appears from a correspondence between St. Vincent of Ferrieres and Don Martin, son of Peter the 1vth of Arragon; and that this language must once have been common in that kingdom appears manifestly from the present name of the Spanish, which is still usually called Romance.

The universality of the Romance in the French dominions, during the 11th century, also accounts for its introduction in Palestine and many other parts of the Levant by Godfrey de Bouillon, and the multitude of adventurers who engaged under him in the Crusade. The assizes or laws of Jerusalem, and those of Cyprus, are standing monuments of the footing that language had obtained in those parts; and if we may trust a Spanish historian of some reputation, who resided in Greece in the 13th century, the Athenians and the inhabitants of Morea spoke at that time the same language that was used in France. And there is great reason to imagine, that the affinity the *Lingua Franca* bears to the French and Italian is entirely to be derived from the Romance, which was once commonly used in the ports of the Levant. The heroic atchievements and gallantry of the Knights of the Cross also gave rise to the swarm of fabulous narratives; which, though not an invention of those days, were yet, from the name of the language in which they were written, ever after distinguished by the appellation of Romances.

Mr. P. concludes this letter by observing, that far from presuming that the Romance hath been preserved so near its primitive state only in the country of the Grisons, there is great reason to suppose that it still exists in several other remote and unfrequented parts. When Fontanini informs us that the ancient Romance is now spoken in the country of the Grisons, he adds, that it is also the common dialect of the Friulense, and of some districts in Savoy bordering on the Dauphiné. And Rivet seriously undertakes to prove, that the Patois of several parts of the Limousin, Quercy, and Auvergne (which in fact agrees singularly with the Romansh of the Grisons) is the very Romance of 8 centuries ago. And no doubt but some inquisitive traveller might still meet with manifest traces of it in many parts of the Pyrenæans and other mountainous regions of Spain, where the Moors and other invaders have never penetrated.

VIII. A Supplement to a Paper, entitled, Observations on the Population of Manchester. By Thomas Percival, M.D., F.R. and A.S. p. 160.

Reprinted in this author's collected works.

IX. Violent Asthmatic Fits, occasioned by the Effluvia of Ipecacuanha. By William Scott, M. D., of Stamfordham, Northumberland. p. 168.

Mrs. S. of Stamfordham, Northumberland, married a person of the medical faculty in the year 1759, being then about 26 years of age. She had been always remarkably healthy before that period, and quite free from all nervous or other complaints, except a trifling nervous head-ach that used to affect her temples and forehead, sometimes for a night or so, about the time of her menstruation. The first year or 2 after marriage she enjoyed her usual good health and spirits in general; but at times she was afflicted with a very troublesome shortness of breathing, attended with a remarkable stricture about her throat and breast, and with a particular kind of wheezing noise. These fits came on very suddenly, and without any previous cause that at first could be assigned; and were often so violent as to threaten immediate suffocation. Their duration was uncertain, sometimes longer and sometimes shorter; but in general they went off in 2 or 3 days, and commonly with a spitting of a tough phlegm, which, she said, had a disagreeable metallic taste. When these fits were off, she enjoyed her usual good health and spirits: had children; and suffered as little as any woman could do, either in breeding or lying-in; and it was not observed, that she was more subject to these fits when with child than at other times. She was blooded, and took some common pectoral medicines for them; but without any benefit. About $1\frac{1}{2}$ year, or 2 years, after her marriage, she told her husband, that she observed these fits always attacked her when any ipecacuanha was powdered in his shop; and that she was certain the effluvia of that medicine immediately brought them on. This was looked on at first as a fancy, and little regard paid to it for some time. However, frequently after this, when any of that medicine was powdering or putting up, she used immediately to call out, perhaps from a different room, that she found the ipecacuanha, and that they would see her immediately affected by it. This Dr. S. and several others, saw frequently happen as she had said; so that we were at last convinced, to a demonstration, that the effluvia of the medicine, some how or other, so affected her nerves, as to bring on a very great and remarkable degree of spasm all about her throat and breast. Having thus had several repeated proofs of the effects the medicine had on her, great precaution was therefore taken for several years never to pound any of it, but to purchase it powdered; and also care was taken, when weighing or putting any of it up, to send her out of the way, or to some distant part of the house. By these means she was kept pretty clear of it for 7 or 8 years together, during which time she enjoyed perfect good health.

Between 9 and 10 o'clock in the evening, June 3, 1775, her husband happening to have got home a quantity of the pulv. ipecacuanhæ, inconsiderately opened it out, and put it into a bottle; his wife not being far off at the time, and then

in perfect good health, immediately, almost even before it was got quite into the bottle, called out that she felt the ipecacuanha affect her throat; on which she was immediately seized with the stricture on her breast and difficulty of breathing. She was advised to walk out into the air, to try if that would remove it; but it had little or no effect. She went to bed some little time afterwards; was exceedingly ill all night; and between 2 and 3 o'clock next morning, June 4, Dr. S. saw her when she was gasping for breath at a window, was as pale as death, her pulse hardly to be felt, and in short she seemed evidently to be in the utmost danger of suffocation. Seven or 8 oz. of blood were taken from her arm, her feet put into warm water, an anodyne draught with 7 or 8 drops of laudanum given her, and she took frequently a table spoonful of oil of almonds. None of these seemed to have the least effect: and she continued much in the same way, with few or no intervals of ease, till about 9 o'clock that morning; when, being in a manner almost exhausted, she fell into a kind of disturbed sleep, the difficulty of breathing with a wheezing noise still continuing with little abatement. She slept some little time, and got out of bed again about 11 o'clock that forenoon; her breathing still very difficult, and her eyes looked red and a little inflamed. After she got up, she became easier towards the afternoon, and it was then supposed it would go off. Dr. Brown, an eminent physician of Newcastle, happening to be in the neighbourhood, called on Mrs. S.; and being told what had happened, said he had known a case, pretty much similar, from the same cause; and hoped, as she then seemed better, it would soon go off; recommended to her riding out as soon as she was able, and to be kept open. Towards bed-time the same evening, June 4, the difficulty of breathing returned, and she was again exceedingly ill all night; had flannel cloths wrung out of warm water applied to her feet, breast, and throat, with little or no advantage; was blooded again about 4 o'clock next morning, June 5, and had also a blister applied to the back part of her neck, still continuing to take now and then a spoonful of the oil of almonds. She again fell into some sleep about 9 in the morning, and continued in bed till between 11 and 12: got up, and was again a little easier during the day; but at night was as bad as ever. And the same scene was continued for 8 days and nights successively; that is, she was generally a little easier from about 11 o'clock of the forenoon, though still far from well, till towards 10 or 11 at night, when the shortness of breathing always returned very violently. However, after 8 days she began to get better rest at nights; the asthmatic fits were neither so long nor so violent; and in about 14 days from the accident were almost entirely gone off; and at the date of this letter, Aug. 1, 1775, though she was in very good health, she had not then quite recovered her usual flesh, strength, and colour. Besides the abovementioned medicines, she took at times, during the first 8 days, small quantities of an emulsion of spermaceti, lac. ammon. and

succ. liquorit.; had a dose of cooling physic; rode and walked out a little sometimes; had a few anodyne draughts with 7 or 8 drops of laudanum; but it could not be observed that she got any benefit from them, except that she sometimes thought the oil of almonds gave her a little ease. She had a slight appearance of the menses about 4 or 5 days after the accident happened, though it was then only about the middle of the usual period; coughed up at times some small quantities of blood, and had also some mixed with her stools and urine. The reason why the laudanum, the most effectual and universal anti-spasmodic, was used in such small quantities was, that it was known before that she could never bear above 8 or 9 drops of it, as the common dose used to affect her with violent sickness at stomach, giddiness and pain in her head, &c. to so great a degree, that for some years past, she neither would take, nor durst her husband administer, a larger dose to her.

The above effects of ipecacuanha, Dr. S. believed, very seldom happened, and no doubt arose from some peculiarity of constitution. Medical writers, as far as he could recollect, seem to have taken little or no notice of its ever producing such an effect as the above. Quincey however, if he remembered right, mentions its producing asthmas; but then he seems to mean, that it has that effect sometimes when taken internally, but not by means of its effluvia. Mr. Leighton, a reputable surgeon and apothecary in Newcastle, told Mr. S. that the effluvia of ipecac. had the very same effect on his wife, as it is above described to have had on Mrs. S.; and that he had once, in particular, very near lost her from having some of it powdered in his shop. The ipecac. which had the above effects on Mrs. S. was the common officinal ash-coloured or grey kind. Oct. 20, 1775, Mrs. S. had quite recovered her flesh, strength, colour, &c. Dr. S. sometimes thought since, that musk in pretty large doses might have been of service to her.

X. On the Success of some Attempts to Freeze Quicksilver, at Albany Fort, in Hudson's Bay, in the Year 1775: with Observations on the Dipping Needle. By Thomas Hutchins, Esq. p. 174.

Mr. H. made his first attempt to freeze the quicksilver on the 19th of January, 1775. The thermometer at 8 o'clock in the morning was at 37° below 0; but between 10 and 11 it stood at 28° . Mr. H. took the same thermometer and the best spare tube he had, which admitted only of 250° below 0, and immersed them both together in a large tea-cup filled with snow, and poured on sp. nitri fumans glauberi till the snow was dissolved; but finding it did not cover the bulbs, he added more snow and spirit till the bulbs were entirely covered in the mixture, which was now liquified: the quicksilver subsided very gradually to 130° , and then stopped. He had another cup at hand, and mixed some snow and spirit in

it so as to liquify the mixture, and removed both the thermometers into it; but found the standard thermometer, viz. the instrument graduated by Messrs. Nairne and Blunt, London, had risen in the removal to 110° below 0. As the mixture in this 2d cup did not cover the bulbs, he added more as before, and also poured some out of the first cup. The spare tube, graduated by himself, stood in this cup at 130° ; but the standard fell deliberately to 262 , where it stood again. He therefore prepared a 3d cup as before; the quicksilver did not ascend in the removal, but when immersed it fell very swiftly: that in the spare tube sunk into the bulb, and that in the standard descended much quicker than before, till it came to 400° ; after which it fell gently to 430° , and did not go beyond this point. As this was a greater degree of cold than that which professor Braum said quicksilver would freeze at, he determined to break his spare tube, which was easily done by a stroke with a pair of scissars; the quicksilver in a fall of about 6 inches was flattened, and some globuli appeared at the bottom of a tea-cup in which it was received. Mr. H. repeatedly struck the cake with a hammer, and heard it give a deadish sound like lead, as M. Braum justly expresses it. The quicksilver liquified in about 6 or 10 seconds. The surface, when frozen, was finely polished. Mr. H. imagines the internal part of the globe was not frozen, and that the force of the fall having flattened it, might crack the external coat or shell of congealed quicksilver, and permit the globuli which he saw to escape into the cup. On taking the standard thermometer out of the mixture, it fell 10° lower than when the bulb was immersed; but it soon began to ascend, and being taken into his room, it rose to 40 above 0.

Having succeeded thus far in his first attempt to freeze quicksilver, he was anxious for another opportunity; but sometimes business, and want of a sufficient degree of cold in the air at other times, obliged him to defer it till the 11th of February, which was very clear, and the thermometer stood at 36° below 0. Mr. H. began the operation at 45 minutes past 8: the instrument, being at 28° , was put into a large tea-cup with the mixture as above, together with a spare tube, graduated by himself; the quicksilver in the latter subsided into the bulb, which was only 200° below 0; in the standard thermometer it sunk to 447° at 59 minutes after 8 o'clock. Finding it did not go any lower, he removed it into a 2d cup, prepared as before; but the quicksilver showed no alteration in it. After waiting a considerable time, he removed it into a 3d; but in the removal, the quicksilver rose to 380° below 0. Mr. H. imagined he had put in too much spirit in proportion to the snow, and therefore added more of the latter, by which means it subsided to 408° ; and after standing at this point for some time, it rose to 406° ; and soon after, at 10 minutes after 9 o'clock, it rose with great celerity and full of bubbles, till it came to 160 above 0, and in a

minute after it reached the point of boiling water. On examining the instrument, he found the bulb cracked, and the quicksilver fluid.

Mr. H. imagines it is extremely difficult to ascertain the exact degree at which quicksilver begins to freeze, because no particular alteration or circumstance points out the moment of congelation, or even afterwards; for the quicksilver in the tube still continues to fall, and has the same appearance as before, contrary to what we observe in water. He thinks therefore it can only be determined by breaking the glasses at different altitudes; but this would be both tedious and expensive. However, were spare tubes filled by the maker, and graduated by the operator, to be made use of, the expence would be less; but then, if those tubes will not admit of being graduated to a considerable distance, suppose 1000° , below 0, the operator is obliged to put a thermometer, with a scale graduated by the instrument-maker, together with the other tube, into the mixture, to learn the degree of cold after the quicksilver in the spare tube, designed chiefly for the experiment, has subsided into the bulb. Professor Braum made it subside even to 1500° , which shows the fineness of the tubes he made use of.

By a great number of observations on the dipping needle, at Albany-Fort, longitude $83^{\circ} 30'$ west, latitude $52^{\circ} 24'$ north, February 3, 1775, the mean of all was $79^{\circ} 17\frac{5}{8}'$. When the observations were finished, he turned the index south, when the needle pointed at $89^{\circ} 56'$, or very nearly perpendicular. He cannot account for the differences, more especially as he took so much pains to render these observations correct.

Similar observations on March 13, 1775, gave for the mean $79^{\circ} 25\frac{1}{4}'$.

And like observations on May 6, 1775, give a medium of $79^{\circ} 28\frac{3}{4}'$.

XI. Astronomical Observations made in the Austrian Netherlands in 1772 and 1773. By Nathaniel Pigott, Esq., F. R. S. p. 182.

At Namur, 1772, by a mean of 8 meridian altitudes of the fixed stars taken in September, Mr. P. determined the latitude of his observatory, in the Rue St. Nicholas, near the Recollets Church, $50^{\circ} 28' 32''$ north. And by an observation of the first satellite of Jupiter, compared with one at Paris, he determined the longitude of Namur, $9^m 39^s$, or $2^{\circ} 24' 45''$, east of Paris observatory.

In like manner, for Luxembourg, he determined the latitude to be $49^{\circ} 37' 6''$, and the longitude $15^m 27^s$ of time, east of Paris observatory, by a mean from a lunar eclipse and many eclipses of Jupiter's first satellite.

At Luxembourg, 1772, Oct. 22, at 3 hours, p. m. a magnetic needle of 4 inches, made by Dollond, gave the declination west $18^{\circ} 42\frac{1}{4}'$. And Oct. 23, at 10 hours, a. m. the declination was $18^{\circ} 50'$.

At La Heese, near Hoogstraeten, by a mean of 22 meridian altitudes of the sun and fixed stars taken in November 1772, one of which only gives the latitude different from the mean of the whole $10''.2$, he determined the latitude of his observatory $51^{\circ} 23' 2'' + N$. And by a mean of the observations of Jupiter's satellites, La Heese is east of the Royal Observatory at Paris $9^m 49^s$ in time, or $2^{\circ} 27' 15''$.

At Ostende, by a mean of 24 meridian altitudes of the sun and stars taken in December, one of which only gives the latitude $11''.7$ different from the mean of the whole, he determined the latitude of his observatory, in the Rue de la Poste, $51^{\circ} 15' 10''$ north. And by an emersion of Jupiter's first satellite, the longitude was $2^m 38^s$, or $38' 15''$, east of Paris observatory. The magnetic declination was $20^{\circ} 35'$.

At Tournai, 1773, by a mean of 14 meridian altitudes of the sun and stars taken in January, one of which only gives the latitude $22''.8$ different from the mean of the whole, he determined the latitude of his observatory, in the Rue des Jesuites, $50^{\circ} 36' 57'' +$ north. The weather would not permit to observe either Jupiter's satellites, or an occultation of a star by the moon, for the longitude of Tournai.

XII. On some Attempts to Imitate the Effects of the Torpedo by Electricity.
By the Hon. Henry Cavendish, F. R. S. p. 196.

Though the proofs brought by Mr. Walsh, (see page, 469, of vol. XIII.) that the phenomena of the torpedo are produced by electricity, are such as leave little room for doubt; yet it must be confessed that there are some circumstances, which, at first, seem scarcely to be reconciled with this supposition. I propose therefore to examine, whether these circumstances are really incompatible with such an opinion; and to give an account of some attempts to imitate the effects of this animal by electricity.

It appears from Mr. Walsh's experiments, that the torpedo is not constantly electrical, but has a power of throwing at pleasure a great quantity of electric fluid from one surface of those parts which he calls the electrical organs, to the other; that is, from the upper surface to the lower, or from the lower to the upper, the experiments do not determine which; by which means a shock is produced in the body of a person who makes any part of the circuit which the fluid takes in its motion to restore the equilibrium.

One of the principal difficulties attending the supposition, that these phenomena are produced by electricity, is, that a shock may be perceived when the fish is held under water; and in other circumstances, where the electric fluid has a much readier passage than through the person's body. To explain this, it must be considered, that when a jar is electrified, and any number of

different circuits are made between its positive and negative side, some electricity will necessarily pass along each; but a greater quantity will pass through those in which it meets with less resistance, than those in which it meets with more. For instance, let a person take some yards of very fine wire, holding one end in each hand, and let him discharge the jar by touching the outside with one end of the wire, and the inside with the other; he will feel a shock, provided the jar is charged high enough; but less than if he had discharged it without holding the wire, in his hands; which shows, that part of the electricity passes through his body, and part through the wire. Some electricians indeed seem to have supposed that the electric fluid passes only along the shortest and readiest circuit; but besides that such a supposition would be quite contrary to what is observed in all other fluids, it does not agree with experience. What seems to have led to this mistake is, that in discharging a jar by a wire held in both hands, as in the above-mentioned experiment, the person will feel no shock, unless either the wire is very long and slender, or the jar is very large and highly charged. The reason of which is, that metals conduct surprisingly better than the human body, or any other substance I am acquainted with; and consequently, unless the wire is very long and slender, the quantity of electricity which will pass through the person's body, will bear so small a proportion to the whole, as not to give any sensible shock, unless the jar is very large and highly charged.

It appears from some experiments, of which I propose shortly to lay an account before this society, that iron wire conducts about 400 million times better than rain or distilled water; that is, the electricity meets with no more resistance in passing through a piece of iron wire 400,000,000 inches long, than through a column of water of the same diameter only one inch long. Sea water, or a solution of one part of salt in 30 of water, conducts 100 times, and a saturated solution of sea salt about 720 times better than rain water.

To apply what has been here said to the torpedo; suppose the fish by any means to convey in an instant a quantity of electricity through its electric organs, from the lower surface to the upper, so as to make the upper surface contain more than its natural quantity, and the lower less; this fluid will immediately flow back in all directions, part over the moist surface, and part through the substance of its body, supposing it to conduct electricity, as in all probability it does, till the equilibrium is restored: and if any person has at the time one hand on the lower surface of the electric organs, and the other on the upper, part of the fluid will pass through his body. And, if he has one hand on one surface of an electric organ, and another on any other part of its body, for instance the tail, still some part of the fluid will pass through him, though much less than in the former case; for as part of the fluid, in its way from the upper surface of the organ to the lower, will go through the tail, some of that part

will pass through the person's body. Some fluid also will pass through him, even though he does not touch either electric organ, but has his hands on any two parts of the fishes body whatever, provided one of those parts is nearer to the upper surface of the electric organs than the other. On the same principle, if the torpedo is immersed in water, the fluid will pass through the water in all directions, and that even to great distances from its body; but it must be observed, that the nearer any part of the water is to the fishes body, the greater quantity of fluid will pass through it. Also, if any person touches the fish in this situation, either with one hand on the upper surface of an electric organ, and the other on the lower, or in any other of those manners in which I supposed it to be touched when out of the water, some fluid will pass through his body; but evidently less than when the animal is held in the air, as a great proportion of the fluid will pass through the water: and even some fluid will pass through him, though he does not touch the fish at all; but only holds his hands in the water, provided one hand is nearer to the upper surface of the electric organs than the other.

The second difficulty is, that no one has ever perceived the shock to be accompanied with any spark or light, or with the least degree of attraction or repulsion. With regard to this, it must be observed, that when a person receives a shock from the torpedo, he must have formed the circuit between its upper and lower surface before it begins to throw the electricity from one side to the other; for otherwise the fluid would be discharged over the surface of the fish's body before the circuit was completed, and consequently the person would receive no shock. The only way therefore by which any light or spark could be perceived, must be by making some interruption in the circuit. Now Mr. Walsh found, that the shock would never pass through the least sensible space of air, or even through a small brass chain. This circumstance therefore does not seem inconsistent with the supposition that the phenomena of the torpedo are owing to electricity; for a large battery will give a considerable shock, though so weakly charged that the electricity will hardly pass through any sensible space of air; and the larger the battery is, the less will this space be. The principle on which this depends will appear from the following experiments.

I took several jars of different sizes, and connected them to the same prime conductor, and electrified them in a given degree, as shown by a very exact electrometer; and then found how near the knobs of an instrument, in the nature of Mr. Lane's electrometer must be approached, before the jars would discharge themselves. I then electrified the same jars again in the same degree as before, and separated all of them from the conductor except one. It was found, that the distance to which the knobs must be approached to discharge this single jar, was not sensibly less than the former. It was also found, that the

divergence of the electrometer was the same after the removal of the jars, as before, provided it was placed at a considerable distance from them: from which last circumstance, I think we may conclude, that the force with which the fluid endeavours to escape from the single jar, is the same as from all the jars together.

It appears therefore, that the distance to which the spark will fly, is not sensibly affected by the number or size of the jars, but depends only on the force with which they are electrified; that is, on the force with which the fluid endeavours to escape from them: consequently, a large jar, or a great number of jars, will give a greater shock than a small one, or a small number, electrified to such a degree, that the spark shall fly to the same distance; for it is well known, that a large jar, or a great number of jars, will give a greater shock than a small one, or a small number electrified with the same force.

In trying this experiment, the jars were charged very weakly, insomuch that the distance to which the spark would fly, was not more than the 20th of an inch. The electrometer I used, consisted of two straws, 10 inches long, hanging parallel to each other, and turning at one end on steel pins as centres, with cork balls, about $\frac{1}{4}$ of an inch in diameter, fixed on the other end. The way by which I estimated the divergence of these balls, was by seeing whether they appeared to coincide with parallel lines placed behind them at about 10 inches distance; taking care to hold my eye always at the same distance from the balls, and not less than 30 inches off. To make the straws conduct the better, they were gilded, which causes them to be much more regular in their effect. This electrometer is very accurate; but can be used only when the electricity is very weak. It would be easy however to make one on the same principle, which should be fit for measuring pretty strong electricity. The instrument by which I found to what distance the spark would fly, differs from Mr. Lane's electrometer no otherwise, than in not being fixed to a jar, but made so as to be held in the hand.

I next took 4 jars, all of the same size; electrified one of them to a given degree, as shown by the electrometer; and tried the strength of the shock which it gave; and found also to what distance the spark would fly. I then took 2 of the jars, electrified them in the same degree as before, and communicated their electricity to the 2 remaining. The shock of these 4 jars united, was rather greater than that of the single jar; but the distance to which the spark would fly was only half as great. Hence it appears, that the spark from 4 jars, all of the same size, will not dart to quite half so great a distance as that from one of those jars, electrified in such a degree as to give a shock of equal violence; and consequently the distance to which the spark will fly, is inversely in a rather greater proportion than the square root of the number of jars, supposing them

to be electrified in such a degree that the shock shall be of a given strength. It must be observed, that in the last mentioned experiment, the quantity of electric fluid which passed through my body, was twice as great in taking the shock of the 4 jars, as in taking that of the single one; but the force with which it was impelled was evidently less, and I think we may conclude, was only half as great. If so, it appears that a given quantity of electricity, impelled through our body with a given force, produces a rather less shock than twice that quantity, impelled with half that force; and consequently, the strength of the shock depends rather more on the quantity of fluid which passes through our body, than on the force with which it is impelled.

That no one could ever perceive the shock to be accompanied with any attraction or repulsion, does not seem extraordinary; for as the electricity of the torpedo is dissipated by escaping through or over the surface of its body, the instant it is produced, a pair of pith balls suspended from any thing in contact with the animal, will not have time to separate, nor will a fine thread hung near its body have time to move towards it, before the electricity is dissipated. Accordingly I have been informed by Dr. Priestley, that in discharging a battery, he never could find a pair of pith balls suspended from the discharging rod to separate. But besides, there are scarcely any pith balls so fine, as to separate when suspended from a battery so weakly electrified, that its shock will not pass through a chain, as is the case with that of the torpedo.

In order to examine more accurately, how far the phenomena of the torpedo would agree with electricity, I endeavoured to imitate them by means of a particular apparatus, made of wood, to represent a torpedo, connected with glass tubes and wires, and covered with a piece of sheep's-skin leather. In making experiments with this instrument, or artificial torpedo as I shall call it, after having kept it in water of about the same saltness as that of the sea, till thoroughly soaked, I fastened the end of one of the wires to the negative side of a large battery, and when it was sufficiently charged, touched the positive side with the end of the wire; by which means the battery was discharged through the torpedo.

The battery was composed of 49 jars, of extremely thin glass, disposed in 7 rows, and so contrived that I could use any number of rows I chose. The outsides of the jars were coated with tin foil; but as it would have been very difficult to have coated the insides in that manner, they were filled with salt water. In a battery to answer the purpose for which this was intended, it is evidently necessary that the metals serving to make the communications between the different jars should be joined quite close: accordingly care was taken that the contacts should be made as perfect as possible. I find, by trial, that each row of the battery contains about $15\frac{3}{4}$ times as much electricity, when both are

connected to the same prime conductor, as a plate of crown glass, the area of whose coating is 100 square inches, and whose thickness is $\frac{5.5}{100}$ of an inch; that is, such that one square foot of it shall weigh 10 oz. 12 dwts.; and consequently, the whole battery contains about 110 times as much electricity as this plate.*

I found, on trial, that though a shock might be procured from this artificial torpedo, while held under water, yet there was too great a disproportion between its strength, when received this way, and in air; for if I placed one hand on the upper, and the other on the lower surface of the electric organs, and gave such a charge to the battery, that the shock, when received in air, was as strong as, I believe, that of the real torpedo commonly is; it was but just perceptible when received under water. By increasing the charge indeed it became considerable: but then this charge would have given a much greater shock out of water than the torpedo commonly does. The water used in this experiment was of about the same degree of saltiness as that of the sea; that being the natural element of the torpedo, and what Mr. Walsh made his experiments with. It was composed of one part of common salt dissolved in 30 of water, which is the proportion of salt usually said to be contained in sea water. It appeared also, on examination, to conduct electricity not sensibly better or worse than some sea water procured from a mineral water warehouse. It is remarkable, that if I used fresh water instead of salt, the shock seemed very little weaker, when received under water than out; which not only confirms what was before said, that salt water conducts much better than fresh; but I think shows, that the human body is also a much better conductor than fresh water: for otherwise the shock must have been much weaker when received under fresh water than in air.

As there appeared to be too great a disproportion between the strength of the shock in water and in air, I made another torpedo, exactly like the former, except that the body part instead of wood was made of several pieces of thick leather, such as is used for the soles of shoes, fastened one over the other, and cut into the proper shape; the pieces of pewter being fixed on the surface of this, as they were on the wood, and the whole covered with sheep skin like the other. As the leather, when thoroughly soaked with salt water, would suffer the electricity to pass through it very freely, I was in hopes that I should find less difference between the strength of the shock in water and out of it, with this than with the other. The event answered expectation; for it required about 3 times as great a charge of the battery, to give the same shock in air, with this new

* I find, by experiment, that the quantity of electricity which coated glass of different shapes and sizes will receive with the same degree of electrification, is directly as the area of the coating, and inversely as the thickness of the glass; whence the proportion which the quantity of electricity in this battery bears to that in a glass or jar of any other size, may easily be computed.—Orig.

torpedo, as with the former ; and the difference between its strength when received under water and out of it, was much less than before, and perhaps not greater than in the real torpedo. There is however a considerable difference between the feel of it under water and in air. In air it is felt chiefly in the elbows ; whereas under water it is felt chiefly in the hands ; and the sensation is sharper and more disagreeable. The same kind of shock, only weaker, was felt if, instead of touching the sides, I held my hands under water at 2 or 3 inches distance from it.

It is remarkable, that I felt a shock of the same kind, and nearly of the same strength, if I touched the torpedo under water with only one hand, as with both. Some gentlemen who repeated the experiment with me thought it was rather stronger. This shows, that the shock under water is produced chiefly by the electricity running through the hand from one part to the other ; and that but a small part passes through our body from one hand to the other. The truth of this will appear with more certainty from the following circumstance ; namely, that if I held a piece of metal, a large spoon for instance, in each hand, and touched the torpedo with them instead of my hands, it gave me not the least shock when immersed in water ; though when held in air, it affected me as strongly if I touched it with the spoons as with my hands. On increasing the charge indeed its effect became sensible : and the battery required to be charged about 12 times as high to give the same shock when the torpedo was touched with the spoons under water, as out of it. It must be observed, that in trying this experiment, as my hands were out of water, I could be affected only by that part of the fluid which passed through my body from one hand to the other.

The following experiments were made with the torpedo in air. If I stood on an electric stool, and touched either surface of the electric organs with one hand only, I felt a shock in that hand ; but scarcely so strong as when touching it in the same manner under water. If I laid a hand on one surface of the electric organs, and with the other touched the tail, I felt a shock ; but much weaker than when touching it in the usual manner ; that is, with one hand on the upper surface of those organs, and the other on the lower. If I laid a thumb on either surface of an electric organ, and a finger of the same hand on any part of the body, except on or very near the same surface of the organs, I felt a small shock. In all the foregoing experiments, the battery was charged to the same degree, except where the contrary is expressed : they all seem to agree very well with Mr. Walsh's experiments.

Mr. Walsh found, that if he inclosed a torpedo in a flat basket, open at the top, and immersed it in water to the depth of 3 inches, and while the animal was in that situation, touched its upper surface with an iron bolt held in one hand, while the other hand was dipped into the water at some distance, he felt a

shock in both of them. I accordingly tried the same experiment with the artificial torpedo; and if the battery was charged about 6 times as high as usual, received a small shock in each hand.* No sensible difference could be perceived in the strength, whether the torpedo was inclosed in the basket or not. The trough in which this experiment was tried was 36 inches long, $14\frac{1}{2}$ broad, and 16 deep; and the distance of that hand which was immersed in the water from the electric organs of the torpedo, was about 14 inches. As it was found necessary to charge the battery so much higher than usual, in order to receive a shock, it follows, that unless the fish with which Mr. Walsh tried this experiment was remarkably vigorous, there is still too great a disproportion between the strength of the shock of the artificial torpedo when received under water and out of it. If this is the case, the fault might evidently be remedied by making it of some substance which conducts electricity better than leather.

When the torpedo happens to be left on shore by the retreat of the tide, it loosens the sands by flapping its fins, till its whole body, except the spiracles is buried; and it is said to happen sometimes, that a person accidentally treading on it in that situation, with naked feet, is thrown down by it. I therefore filled a box, 32 inches long and 22 broad, with sand, thoroughly soaked with salt water, to the depth of 4 inches, and placed the torpedo in it, entirely covered with the sand, except the upper part of its convex surface, and laid one hand on its electrical organs, and the other on the wet sand about 16 inches from it. I felt a shock, but rather weak; and about as strong as if the battery had been charged half as high, and the shock received in the usual way.

I next took 2 thick pieces of that sort of leather which is used for the soles of shoes, about the size of the palm of my hand; and having previously prepared them by steeping in salt water for a week, and then pressing out as much of the water as would drain off easily, repeated the experiment with these leathers placed under my hands. The shock was weaker than before, and about as strong as if received in the usual way with the battery charged one-third part as high. As it would have been troublesome to have trod on the torpedo and sand, I chose this way of trying the experiment. The pieces of leather were intended to represent shoes, and in all probability the shoes of persons who walk much on the wet sand will conduct electricity as well as these leathers. I think it likely, therefore, that a person treading in this manner on a torpedo, even with shoes on, but more so without, may be thrown down, without any extraordinary exertion of the animal's force, considering how much the effect of the shock would be aided by the surprize.

* As well as I could judge, the battery required to be charged about 16 or 20 times as high, to give a shock of the same strength when received this way as when received in the usual manner with the torpedo out of water.—Orig.

One of the fishermen employed by Mr. Walsh assured him, that he always knew when he had a torpedo in his net, by the shocks he received while the fish was at several feet distance; in particular, he said, that in drawing his nets with one of the largest in them, he received a shock when the fish was at 12 feet distance, and 2 or 3 more before he got it into his boat. His boat was afloat in the water, and he drew in the nets with both hands. It is likely that the fisherman might magnify the distance; but probably he may so far be believed, as that he felt the shock before the torpedo was drawn out of water. This is the most extraordinary instance I know of the power of the torpedo; but I think seems not incompatible with the supposition of its being owing to electricity; for there can be little doubt, but that some electricity would pass through the net to the man's hands, and thence through his body and the bottom of the boat, which in all probability was thoroughly soaked with water and perhaps leaky, to the water under the boat: the quantity of electric fluid however, taking this circuit, would most likely bear so small a proportion to the whole, that this effect can not be accounted for, without supposing the fish to exert at that time a surprizingly greater force than what it usually does.

Hitherto the effects of this artificial torpedo appear to agree very well with those of the natural one. I now proceed to consider the circumstance of the shock's not being able to pass through any sensible space of air. In all my experiments on this head, I used the first torpedo, or that made of wood; for as it is not necessary to charge the battery more than one-third part as high to give the same shock with this as with the other, the experiments were more likely to succeed, and the conclusions to be drawn from them would be scarcely less convincing: for I find, that 5 or 6 rows of my battery will give as great a shock with the leathern torpedo, as one row electrified to the same degree will with the wooden one; consequently, if with the wooden torpedo and my whole battery, I can give a shock of a sufficient strength, which yet will not pass through a chain of a given number of links, there can be no doubt, but that if my battery was 5 or 6 times as large, I should be able to do the same thing with the leathern torpedo.

I covered a piece of sealing wax on one side with a slip of tin foil, and holding it in one hand, touched an electrical organ of the torpedo with the end of it, while my other hand was applied to the opposite surface of the same organ. The shock passed freely, being conducted by the tin foil; but if I made, with a penknife, as small a separation in the tin foil as possible, so as to be sure that it was actually separated, the shock would not pass; conformably to what Mr. Walsh observed of the torpedo. I tried the experiment in the same manner with the Lane's electrometer described before, and found that the shock would

not pass, unless the knobs were brought so near together as to require the assistance of a magnifying glass to be sure that they did not touch.

I took a chain of small brass wire, and holding it in one hand, let the lowest link lie on the upper surface of an electric organ, while my other hand was applied to the opposite surface. The event was, that if the link held in my hand, was the 5th or 6th from the bottom, and consequently that the electricity had only 4 or 5 links to pass through besides that in my hand, I received a shock; so that the electricity was able to force its way through 4 or 5 intervals of the links, but not more. If, instead of this chain, I used one composed of thicker wire, the shock would pass through a great number of links; but I did not count how many. It must be observed, that the principal resistance to the passage of the electrical fluid is formed by the intervals of the lower links of the chain; for as the upper are stretched by a greater weight, and therefore pressed closer together, they make less resistance. Consequently the force required to make the shock pass through any number of intervals, is not twice as great as would be necessary to make it pass through half the number. For the same reason it passes easier through a chain consisting of heavy links than of light ones. Whenever the electricity passed through the chain, a small light was visible, provided the room was quite dark. This however affords no argument for supposing that the phenomena of the torpedo are not owing to electricity; for its shock has never been known to pass through a chain or any other interruption in the circuit; and consequently it is impossible that any light should have been seen.

In all these experiments, the battery was charged to the same degree; namely, such that the shock was nearly of the same strength as that of the leathern torpedo, and which I am inclined to think, from my conversation with Mr. Walsh, may be considered as about the medium strength of those of a real one of the same size as this.

As it appeared, that a shock of this strength would pass through a few intervals of the links of the chain, I tried what a smaller would do. If the battery was charged only to a 4th or 5th part of its usual height, the shock would not pass through a single interval: but then it was very weak, even when received through a piece of brass wire, without any link in it. This chain was quite clean and very little tarnished; the lowest link was larger than the rest, and weighed about 8 grains. If I used a chain of the same kind, the wire of which, though pretty clean, was got brown by being exposed to the air, the shock would not pass through a single interval, with the battery charged to about one-third or one-half its usual strength.

It appears, that in this respect the artificial torpedo does not completely imi-

tate the effects of the real one, though it approaches near to it; for the shock of the former, when not stronger than that of the latter frequently is, will pass through 4 or 5 intervals of the links of a chain; whereas the real torpedo was never known to force his through a single interval. But I think this by no means shows that the phenomena of the torpedo are not produced by electricity; but only that the battery I used is not large enough. For we may safely conclude, from the experiments mentioned before, that the greater the battery is, the less space of air, or the fewer links of a chain, will a shock of a given strength pass across. For greater certainty however, I tried whether, if the whole battery and a single row of it were successively charged to such a degree, that the shock of each should be of the same strength when received through the torpedo in the usual manner, that of the whole battery would be unable to pass through so many links of a chain as that of a single row. From which it appears, that if the whole battery, and a single row of it, are both charged in such a degree as to give a shock of the same strength, the shock with the whole battery will pass through fewer loops of the chain than that with the single row; so that I think there can be no doubt, but that if the battery had been large enough, I should have been able to give a shock of the usual strength, which yet would not have passed through a single interval of the links of a chain.

On the whole, there seems nothing in the phenomena of the torpedo at all incompatible with electricity; but to make a complete imitation of them, would require a battery much larger than mine. It may be asked, where can such a battery be placed within the torpedo? I answer, perhaps it is not necessary that there should be any thing analogous to a battery within it. The case is this; it appears that the quantity of electric fluid, transferred from one side of the torpedo to the other, must be extremely great; for otherwise it could not give a shock, considering that the force with which it is impelled is so small as not to make it pass through any sensible space of air. Now if such a quantity of fluid was to be transferred at once from one side to the other, the force with which it would endeavour to escape would be extremely great, and sufficient to make it dart through the air to a great distance, unless there was something within it analogous to a very large battery. But if we suppose that the fluid is gradually transferred through the electrical organs, from one side to the other, at the same time that it is returning back over the surface, and through the substance, of the rest of the body; so that the quantity of fluid on either side is, during the whole time, very little greater or less than what is naturally contained in it; then it is possible, that a very great quantity of fluid may be transferred from one side to the other, and yet the force with which it is impelled be not sufficient to force it through a single interval of the links of a chain. There seems however to be room in the fish for a battery of a sufficient size; for Mr. Hunter

has shown, that each of the prismatical columns of which the electrical organ is composed, is divided into a great number of partitions by fine membranes, the thickness of each partition being about the 150th part of an inch; but the thickness of the membranes which form them is, he says, much less. The bulk of the 2 organs together, in a fish $10\frac{1}{3}$ inches broad, that is of the same size as the artificial torpedos, seems to be about $24\frac{1}{2}$ cubic inches; and therefore the sum of the areas of all the partitions is about 3700 square inches. Now 3700 square inches of coated glass, $\frac{1}{150}$ of an inch thick, will receive as much electricity as 30500 square inches .055 of an inch thick; that is, 305 times as much as the plate of crown glass beforementioned, or about $2\frac{3}{4}$ times as much as my battery, supposing both to be electrified by the same conductor; and if the glass is 5 times as thin, which perhaps is not thinner than the membranes which forms the partitions, it will contain 5 times as much electricity, or near 14 times as my battery.

It was found, both by Dr. Williamson and by a committee appointed by the Philosophical Society of Pennsylvania, that the shock of the *Gymnotus* would sometimes pass through a chain, though they never perceived any light. I therefore took the same chain which I used in the foregoing experiments, consisting of 25 links, and suspended it by its extremities from the extreme hooks of a machine, and applying the end of the machine to the negative side of the battery, touched the positive side with a piece of metal held in the other hand, so as to receive the shock through the chain without its passing through the torpedo; the battery being charged to such a degree that the shock was considerably stronger than what I usually felt in the foregoing experiments. I found that if the chain was not stretched by any additional weight, the shock did not pass at all: if it was stretched by hanging a weight of 7 pennyweights to the middle link, it passed, and a light was visible between some of the links; but if 14 pennyweights were hung on, the shock passed without my being able to perceive the least light, though the room was quite dark; the experiment being tried at night, and the candle removed before the battery was discharged. It appears therefore, that if in the experiments made by these gentlemen, the shock never passed, except when the chain was somewhat tense, which in all probability was the case, the circumstance of their not having perceived any light, is by no means repugnant to the supposition that the shock is produced by electricity.

XIII. Observations on Respiration, and the Use of the Blood. By Joseph Priestley, LL. D., F. R. S. p. 226.

Reprinted in this author's works on Air.

XIV. Experiments on Water obtained from the Melted Ice of Sea-Water, to ascertain whether it be fresh or not; and to determine its Specific Gravity with respect to other Water. Also Experiments to find the Degree of Cold in which Sea Water begins to freeze. By Mr. Edw. Nairne. p. 249.

The sea water used in the following experiments was furnished by Mr. Owen, of the Mineral Water Warehouse, at Temple Bar; who assured Mr. N. that it was taken up off the North Foreland. On Jan. 27, 1766, at 10 in the evening, Mr. N. filled a jar, $3\frac{1}{4}$ inches in diameter and $6\frac{1}{2}$ inches deep, with seawater, and exposed it to the open air, the thermometer standing at 15° . At noon the next day, on taking it in, he found it frozen very hard, except a very little at the bottom, which remained quite fluid. The ice, when taken in from the open air, was $\frac{1}{4}$ of an inch above the edge of the jar. He now set it by a stove in a heat of 56° to thaw. When the jar had continued in this degree of heat for 8 hours, he took out the ice, which was then $3\frac{1}{2}$ inches long and 2 inches in diameter; about $\frac{2}{3}$ of the water appeared to remain. In order to clear the ice from any brine that might adhere to it, he washed it in a pail of pump water, in which it was suffered to remain about a quarter of an hour, and then set it in a sieve to drain off the water in which it had been washed. On Jan. 29, he set the beforementioned ice in a basin in a heat of about 46° , in which it continued 9 hours before the whole was dissolved. The bulb of a thermometer rested on the ice during the time of the solution, and continued without variation at 32° . The water thus obtained was, to his palate, perfectly free from any taste of salt.

To ascertain the comparative gravity of this water, Mr. N. filled a bottle with it to a certain mark in its neck, which was very narrow, and weighed the bottle so filled very carefully. He weighed the same bottle, filled to the same mark in its neck with seawater, and other waters successively, which were all brought to the same degree of heat by a thermometer. The results were as follow; videlicet,

	Grains.
Water obtained from the melted ice of the seawater	1614
Distilled rainwater	1612
Water taken out of a water tub, being a mixture of rain and snow water.....	1615
The seawater	1653
The residuum of the seawater from which the ice beforementioned had been taken..	1659

To find the degree of cold in which seawater begins to freeze, Mr. N. made the following experiments. He exposed to the open air a decanter filled with the seawater, in which a thermometer was suspended, the bulb of which reached to the middle of the widest part of the decanter; a jelly-glass filled with the same seawater, in which also a thermometer was put, resting on the bottom, was placed in the same exposure. The result will be seen in the following table:

January 29, 1776.

Vessel.	Time.	Im. Ther.	Therm. in the open air.	Effects, &c.
Decanter, Jelly Glass,	12 ^h 0 ^m	33 25 to 28.5	19	A number of beautiful feathered crystals appeared in the jelly glass; they began to shoot from the top, which was covered with ice, toward the bottom; when they reached it, the thermometer rose immediately from 25 to 28.5. Ice began to form in the decanter though hardly perceptible at the edge of the water.
Decanter, Jelly Glass,	12 15	31 28.5	19	
Decanter, Jelly Glass,	12 20	30 28.5	19	Crystals of a laminated appearance began to shoot downwards obliquely from the ice at the surface, which at the edge of the water was barely two-tenths of an inch thick; no appearance of ice in the middle of the surface.
Decanter, Jelly Glass,	12 30	29 28.5		
Decanter,	1 0 p.m.	27.5	19	Crystals began to shoot round the neck of the decanter close to the glass.
Decanter,	1 15	28.5	19	The inside became covered with finely feathered crystals, which made it impossible to observe the height of the thermometer, without raising it till the quicksilver in the tube appeared above the ice.
Decanter,	4 0	28.5	19	

January 29, at 8 in the evening, Mr. N. exposed to the open air 2 similar jars each $5\frac{1}{2}$ inches deep and $1\frac{7}{10}$ inch in diameter; one of which, for the sake of distinction, he called A, the other B. A was filled with the seawater; B with water taken out of a water tub, which was a mixture of rain and snow water. In A 2 thermometers were placed; one rested on the bottom; the upper part of the ball of the other was a quarter of an inch only below the surface of the water; one thermometer was also placed in B, resting on the bottom. The following table shows the result.

Vessel.	Time.	Therm. at the Top.	Therm. at the Bottom.	Therm. in the open air.	
A	8 ^h 0 ^m p. m.	60	60	19.5	
B			60		
A	8 15	40	33		
B			38		
A	8 20	35	29.5		The surface of the water in B covered with ice.
B			37.5		
A	8 25	31	26.5		Surface as before.
B			34		
A	8 30	29	25		No appearance of ice.
B			32		
A	8 32	28.5	24.5		The ice on the surface increased.
B			32		
A	8 36	28.2	28.5		Ice began to appear on the surface.
B			32		
					Quite frozen.
					Crystals over every part of the glass.
				20	As before.

During the time in which these observations were made, the thermometer in the open air rose half a division.

The following table shows the result of some further observations on the effects of cold on the seawater in the jar A of the last table, which had been

thawed in order to be now exposed again to the open air. The thermometer in the jar continued in the same situation as before.

January 30, 1776, A.M.

Time.	Therm. at the Top.	Therm. at the Bottom.	Therm. in the open air.	Effects, &c.
10 ^h 32 ^m	34.5	35.5	16.5	The water fluid.
10 39	29	32		Ice began to be formed about the glass at the edge of the water.
10 42	28.5	30.5		Still continued to have ice only about the edge of the water.
10 48	28	28		The surface of the water rendered stagnant by the ice.
11 1	27	24.5	18.5	The crystals had almost reached the bottom.
11 1½ } 2 }	27 +	28.5		During the half minute employed in this observation, the crystals reached the bottom of the jar; the lower thermometer rose almost instantaneously from 24.5 to 28.5, and was immediately rendered obscure by the ice.
11 45	26.5	28.5	19	The jar was taken in from the open air, and the lower thermometer lifted out of the ice to a sufficient height for the observation.

From these observations it seems that the freezing point of seawater should be fixed in Fahrenheit's scale at 28.5. As the water, when it began to freeze in 2 experiments, exhibited phenomena different from any Mr. N. had observed before, it may not be improper to subjoin an account of them. At 14^m after 8 in the morning of Jan. 31, he put the jar B of the 2d table, containing the same water; viz. a mixture of rain and snow water, in a window; having the evening before placed a 2d thermometer in it, the bulb of which was just below the surface of the water. This, as well as the thermometer at the bottom, stood at 27.5, and the water was perfectly fluid: the thermometer placed near the jar within the window, was at 23.5. At 27^m after 8 it began to freeze at the bottom of the jar, the thermometers at the top and bottom standing alike at 27. The instant the crystals began to encompass the ball of the thermometer below, which they very soon did after it began to freeze, the quicksilver rose in it to 32°, the upper one continuing at 27°. The crystals continued to shoot upward, and in less than half a minute reached the bulb of the thermometer at the surface, which immediately rose to 32°. After 10^m before 6 in the evening of the same day, Mr. N. put the jar A of the 2d table into the open air, its contents the same; viz. seawater. The thermometers in it were likewise the same, not having been moved; they both stood at 34°; that in the open air at 19.5. At 6 o'clock the thermometer above was at 31°, that below at 28.25. At this time he discovered some ice on the surface of the water; but as it was by candle-light, he could not discern its first appearance. At 10^m after 6, the thermometer above was at 29°; that below at 26.5. At 15^m after 6, the upper thermometer at 28.5; that below at 25°. At 17^m after 6, both the thermometers stood at 28.5, crystals having risen from the bottom covered the ball of that below, on which it rose instantly from 25° to 28.5. The thermometer in the open air continued as at first, viz. at 19.5.—The scale of all the thermometers, used in these experiments, was Fahrenheit's.

XV. Easy Methods of Measuring the Diminution of Bulk, taking place on the Mixture of Common Air and Nitrous Air; with Experiments on Platina. By John Ingenhousz, M. D., F. R. S. p. 257.

Dr. I. having received from Abbé Fontana a copy of a pamphlet, on the use of some experiments for measuring the salubrity of air, he imitated some of them, and found them very useful for the intended purpose of measuring the quantity of air absorbed or diminished by mixing the nitrous with the common air; by which criterion the degree of the salubrity of common air may be ascertained according to the discovery of Dr. Priestley. Fontana first produces nitrous air in a separate vessel, and then forces it into the glass, or other vessel, in which it is to remain, till a communication be opened between this vessel and the other which contains common air. Dr. I. found it a difficult matter to force always just the same quantity of nitrous air into the vessel; because he could never be sure that the nitrous air had dislodged all the common air out of it, or had dislodged always the same quantity of common air. If this quantity is not always just the same, some variety must happen in every experiment; and thus an exact valuation of the quantity of air absorbed cannot well be made. To obviate in some measure this difficulty, and to abridge the experiment by mixing suddenly the two airs together, he contrived a particular instrument. It is a strong glass vessel, nearly $2\frac{1}{2}$ inches in diameter, and about as much in height: a conical figure would perhaps be better. A brass cover, which embraces the glass about half an inch downwards, is cemented to it, and has a hole in its middle, corresponding with the hole in the glass vessel. This hole of the brass cover has a female screw fitted to receive the male screw of a brass tube, about 7 inches long and about an inch in diameter, terminating at one end in a male screw, and at the other, in a neck adapted to enter the mouth of an elastic gum bottle, otherwise called boradchio or caout-chouc, to be tied to it with a strong ribbon. This brass tube has towards each extremity an air tight cock, by which the communication between one extremity and the other may be opened or shut. Between these two cocks, about the middle of the tube, is a short lateral tube, communicating with the canal of the other tube. This lateral tube has also an air tight cock, which opens or shuts up the communication with the long tube, and has a female screw to receive the male screw of another short tube, which serves to receive a glass tube bent at right angles, and of 2 feet or more in length; the diameter somewhat more than that of a large quill. This glass tube is to be divided into any number of equal parts.

The instrument is used in the following manner. The elastic gum bottle being well tied to the brass tube, all the cocks shut, and the glass tube fixed to its place, he pours a certain quantity of aquafortis (v. g. $\frac{3}{4}$ lb) into the glass vessel, taking care that none of it touches the brass cover: he then puts into it

a certain quantity of iron filings (v. g. 3j) wrapt up in a bit of paper to prevent its being immediately corroded. This done, he screws the glass vessel to the brass tube, so that no air can get out. When the red fumes begin to rise, he opens the 2 cocks of the brass tube, which open the communication between the glass vessel and the elastic gum bottle. By squeezing the elastic gum bottle, he forces the two airs to mix together. The diminution of the air is soon perceived by the elastic gum bottle becoming flaccid. When he judges the air is as much diminished as it can be, he puts the extremity of the glass tube into a vessel with water, and opens the cock of the side tube: the water immediately rises in the glass tube to a height proportioned to the diminution of the two airs. By repeating several times the experiment in the same place, he found the rise of the water nearly the same, though not so exactly as he could have wished: the variation he ascribed partly to the elastic bottle not being always of the same firmness or elasticity, which it loses more or less by squeezing.

Dr. I. contrived another method, more simple, and perhaps more accurate, which is the following: he took a glass tube about $2\frac{1}{2}$ feet long, and not quite a 12th of an inch in diameter; so that a column of quicksilver might slide through the whole without dispersing itself, filling always the whole cavity. He cemented to each extremity a brass ring, that he might be able to shut the opening with his finger without hurting himself. This tube being divided into 100 equal parts, he used it in two different ways; viz. having poured some aquafortis into a little phial, and put to it some filings, he thrust the extremity of the glass tube into the neck of the phial. A column of quicksilver of about an inch in length occupied that end of the glass tube which was in the neck of the phial. The whole was kept in such a posture that the tube was nearly in an horizontal line, the end which is put into the phial being rather the highest. Care was taken that the tube should not touch the aquafortis. The phial being filled with red fumes, and the extremity of the tube surrounded with them, he opens and shuts alternately the opposite extremity of the tubes, so as to allow the quicksilver to advance slowly towards the middle; as soon as the column of quicksilver is arrived at the middle, he takes the tube out of the bottle, and shuts each extremity with the fore-finger: thus moving the tube upwards and downwards as briskly as can be done with a certainty of keeping both extremities all the while exactly shut. The two airs being thoroughly mixed, he puts one extremity into a vessel filled with quicksilver, and withdrawing the finger from the opening, the quicksilver rises immediately within the tube, and shows by its height the exact quantity of air diminished.

The other method is this: he ties to the end of the same tube the neck of a small elastic gum bottle, the bottom of which is cut away: having put some iron filings into a little phial, filled with aquafortis, he puts the end of the tube

within the mouth of the phial, clapping his hand fast to the orifice of the phial, the loose part of the elastic bottle, so that the nitrous air, rising from the phial, must take its course through the tube. When the whole tube is filled with red fumes, he takes it out, and shuts the two extremities with his two fore-fingers. He then puts one end of the tube in a vessel with quicksilver, and withdraws both fingers for an instant to make the column of quicksilver rise within the tube. He applies immediately both fingers; and holding the tube nearly in a horizontal direction, so that the extremity where the quicksilver is may be rather the highest, he opens and shuts at the same time both extremities, so that the column of quicksilver gradually advances towards the middle. The quicksilver advancing towards the middle, as much common air follows the quicksilver as it forces out nitrous air from the other extremity. As soon as the column of quicksilver is in the middle, he keeps both extremities well shut with his fingers, and moving the tube in various ways, he forces the two airs to come into mutual contact, and to mix intimately together. He then puts one extremity into a vessel filled with quicksilver, withdraws the finger from within the quicksilver, and observes to what height the quicksilver rises. It requires some practice to perform this experiment with dexterity.

Some time ago Dr. I. got some ounces of fine platina from Spain, with which he made some experiments. Most writers assert, that a considerable part of the platina is attracted by the magnet, but not the whole of it: but by a nice inquiry he found, that every one of the particles obeyed the magnet more or less, except some transparent stony particles; and that even these were all magnets in themselves; or that each particle had 2 poles, which he could change at pleasure by the application of magnetical bars. Though their magnetical virtue is always much less than that of particles of iron, yet every one had more or less of it; but some so little as not to be perceived but by applying a strong magnet to them when floating on water. Besides the flat, smooth, and shining bright particles, which are alone the true platina, he found 2 other kind of particles among them; viz. some very small black particles, most of which are of an irregular figure, resembling the iron sand found in some parts of North America; at Teneriffe; near some lakes in Italy; in some rivers in Transylvania, among the gold dust which is taken out of them; and in many other places. Some of these black particles, though few in comparison with the number of the irregular particles, are of a very regular figure, and when seen through a good magnifier, somewhat resemble the figure of a melon. These black particles of both sorts are attracted by the loadstone, and have each of them 2 poles, though those of an irregular figure have them more manifestly. The other particles are of a gold colour; having, in general, more or less of a paleness approaching to the colour of platina. Some of these gold particles

have the figure of the rest of the platina, differing only from them in colour, and in not being so bright, or as it were polished. Others are irregular masses of indeterminate figure, having generally a spongy appearance. The most part of these gold particles were evidently attracted by the magnet, and showed on the surface of the water their 2 distinct poles. These gold particles being put on a piece of charcoal, and the flame of a candle directed on them by the blow-pipe of the chemical pocket laboratory, described by Gustave Von Engestrom, published in the English translation of Crownsted's Mineralogie, ran easily into round balls, which have all the appearance and quality of real gold, except their being in general magnetical or having 2 distinct poles. I make no doubt but this magnetical quality is owing to some platina mixed with the gold.

Dr. I. could never melt a single particle of true shining platina by blowing strongly on it with the blow pipe; the only change they underwent by this operation, was to lose their brightness and the greatest part of their magnetical virtue. Having filled a small glass tube with that platina, each end of it attracted both poles of a compass indiscriminately; but being put to a set of magnetical bars, it became a real magnet, having 2 distinct poles, which he could change at pleasure. He filled another small tube with platina, the hollow of the tube being only of such a size, as to allow the particles of platina to go in freely. He stuck a pin in each end, and fixed the pins with sealing-wax. He directed 5 or 6 electrical explosions from 3 very large jars through the tube; after which the platina had acquired no polarity. By looking with a microscope at the outside of the tube, the platina was much changed, so as to appear one uninterrupted cylinder of metal, all the interstices between each particle being quite, in appearance at least, obliterated and filled with bright metal. The places which were not bright, were become of a black hue, and appeared to be parts of the platina not melted; which he found afterwards to be the case. He attempted to shake the particles out of the tube, but he could not succeed. He could only force out some few at the opening with a pin. He separated a little bit of the tube with a file, to push out the cylinder of platina; but could not succeed without employing a great force: he therefore beat some part of the tube to pieces with a hammer, and found each particle had undergone a remarkable alteration. All of them appeared in several places to have been melted, and some little ones seemed to have been entirely in a fluid state; they all adhered in lumps together so strongly, that many of them could absolutely not be rubbed asunder between the fingers. The inside of the tube exhibited marks of having received impressions of the melted metal. By comparing the separated particles of this platina with particles not exposed to an electrical explosion, they were scarcely to be known for the same substance. He had put some iron filings in a tube of the same size, and directed the same explosion

through it, in order to compare the effect of electricity on it with what happened to the platina. He found, by looking at the outside, somewhat of the same appearance, of being melted. By cutting this tube in small bits, he could easily push out the filings with a pin, which he could not do in the other case but with great force. The filings stuck together, as the particles of platina had done; but with less force.

By this experiment it should seem as if platina, which hitherto could never be melted by common fire by itself, but only in the focus of a very strong burning glass, were equally fusible, if not more so than iron, by electrical fire. The particles of platina taken out of the aforesaid tube, had got a remarkably stronger magnetical force, being attracted by a loadstone at a greater distance, and turning their poles more briskly on the water than before, though the whole cylinder of these particles, still inclosed in the tube, gave no signs of having acquired polarity. Thus it appears, that common fire diminishes the magnetical virtue of platina, and that electrical fire increases it. Platina mixed with lead was put on an ordinary cupel in a docimastic furnace strongly heated. When the metal came to a solid state, it was a flat rough lump, much heavier than the crude platina. He put fresh lead to it, and cupelled it again as before. He repeated it 10 times, when he obtained a large lump, somewhat less flat, pretty smooth, but not bright; of about the same weight as after the first cupellation. This lump did not give the least sign of magnetism, and even would not receive any by being applied to strong magnetical bars; and the substance was very brittle, nearly of the same colour as platina, and took a fine polish.

Though a piece of soft iron attracts the 2 poles of a compass indiscriminately, and is incapable of acquiring polarity itself, yet Dr. I. has never been able to separate a single particle of the softest iron, even when he separated it carefully with a flint, or other body containing no steel or iron, without its giving evident signs of 2 distinct poles when floating on water, nay even on paper. Dr. I. could also never find iron filings of ever so soft a substance, but each particle separately had evidently 2 poles. Such iron filings mixed with bees wax, as much as is sufficient to keep them together, got a strong polarity by being touched with magnetical bars, and had all the qualities of a magnet: the mass is easily cut with a warm knife, and is very convenient for magnetical experiments, such as Dr. Knight made with similar loadstones made of pounded magnets. He found also, that each part of those granulated iron ores of Sweden, which are placed among the *mineræ ferri retractoriæ*, separated iron from stone, and had 2 distant poles, and that a piece of the ore itself became a tolerable magnet by being touched with the bars.

XVI. An Account of Three Journeys from the Cape Town into the Southern Parts of Africa; undertaken for the Discovery of New Plants, towards the Improvement of the Royal Botanical Gardens at Kew. By Mr. Francis Masson, one of his Majesty's Gardeners. p. 268.

In this journey Mr. M. discovered many new plants, of which, however, no description is here given. Respecting the country and inhabitants, his observations contain nothing worthy of notice.

XVII. Meteorological Journal kept at the House of the Royal Society, by order of the President and Council. p. 319.

These observations of the thermometers, barometers, rain, winds, weather, &c. were made twice in every day of the year, viz. at 8 and at 2 o'clock, beginning with March 1775, and ending with February 1776. The mediums of the whole, for each month, and for the whole year, are here taken, and registered as in the following table.

1775.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
March	58.0	28.5	43.9	53.0	37.0	46.1	30.61	29.07	29.67	1.854
April	83.5	36.5	52.8	69.0	40.0	52.7	30.36	29.50	30.026	1.068
May	74.5	43.0	57.8	67.5	50.0	58.5	30.43	29.68	30.12	0.552
June	81.5	52.0	66.3	73.5	60.5	67.3	30.30	29.61	29.91	1.258
July	82.0	58.0	66.1	74.0	61.5	67.3	30.18	29.59	29.88	4.364
August	75.5	52.5	64.3	70.5	58.0	64.8	30.07	29.50	29.86	2.203
September	75.0	47.0	61.1	68.5	56.0	62.6	30.15	29.51	29.756	5.192
October ..	66.0	32.0	50.5	68.0	41.5	52.4	20.26	29.16	29.86	2.427
November	57.0	28.0	42.5	53.0	37.0	43.7	30.36	29.16	29.76	2.941
December	58.0	27.0	41.2	56.0	34.0	43.0	30.57	28.60	30.06	0.576
1776.										
January ..	44.5	13.5	29.3	43.5	20.5	31.8	30.14	29.21	29.687	1.167
February ..	49.5	14.5	42.6	46.0	19.0	42.4	29.97	28.84	29.408	3.510
Whole year, beginning with March 1775. }			51.5			52.7			29.833	27.114

The mean variation of the needle was $21^{\circ} 43'$. And the mean dip $72^{\circ} 30'$.

XVIII. An Abridged State of the Weather at London for one Year, commencing with March 1775, collected from the Meteorological Journal of the Royal Society. By S. Horsley, LL.D. Sec. R. S. p. 354.

TABLE I.

An Abridged View of the Winds at London, for one Year, beginning with March 1775.

Months.	N.	S.	E.	W.	N.W.	S.E.	N.E.	S.W.	Sums.	Rain.	Qrly. rain.	Hf. yr. rain.
March	1	1½	0	3	6½	1	1	17	31	1.854	3.474	11.298
April	1½	3½	0½	1½	3½	3	6½	10	30	1.068		
May	6	0	0	2½	6½	2	6½	7½	31	0.552		
June	1	1	0½	1½	1	4½	14	6½	30	1.258	7.824	15.813
July	1	2½	0	2	7½	2	2	14	31	4.364		
August	0½	3	0½	2	3½	3½	1½	16½	31	2.203		
September	0½	3½	0½	1½	1½	2½	5	15	30	5.192	10.560	15.813
October ..	1½	1½	0½	4	3½	0	2½	17½	31	2.427		
November	4	0½	2	0	3	5½	9½	5½	30	2.941		
December	2½	2½	1	0½	1	3	6	14	30½	0.576	5.253	
January	2½	1	4½	0	1½	3½	15½	2½	31	1.167		
February ..	0½	1	1	0	0½	2	2	22	29	3.510		
Sums	22½	21½	11	18½	39½	32½	72	148		27.112		

It appears that the winds from the s. w. were again the most frequent of any, and next to these the winds from the n. e. Of the winds from the 4 cardinal points, the n. was the most frequent, and the e. the most rare. The autumn was the wettest quarter, and the spring the driest. The rain of the 3 summer months was almost half as much again as that of the 3 winter months; but the rain of the winter half-year exceeded that of the summer half-year by about $\frac{1}{8}$ of the rain of the whole year. September gave the greatest quantity of rain, and May the least of any single month in the whole year.

TABLE II.

Sub-division of the s. w.

	W. S. W.	S. W.	S. S. W.	Sums.
March	5	5	7	17
April	3	4	3	10
May	3½	0½	3½	7½
June	3	3	0½	6½
July	4	7	3	14
August	3	2½	11	16½
September	6	3½	5½	15
October ..	5	7½	5	17½
November	0½	2½	2½	5½
December	3	6	5	14
January ..	1	0½	1	2½
February ..	8	6½	7½	22
Sums	45	48½	54½	148

TABLE III.

Sub-division of the n. e.

	E. N. E.	N. E.	N. N. E.	Sums.
March	0	1	0	1
April	1	3	2½	6½
May	2	1½	3	6½
June	6	4	4	14
July	0½	1	0½	2
August	0½	0½	0½	1½
September	1½	1	2½	5
October ..	1	1	0½	2½
November	4	3½	2	9½
December	1½	2	2½	6
January ..	6½	6½	2½	15½
February ..	0½	1½	0	2
Sums	25	26½	20½	72

TABLE IV.

Sub-division of the s. e.

	E. S. E.	S. E.	S. S. E.	Sums.
March	0	$0\frac{1}{2}$	$0\frac{1}{2}$	1
April	1	1	1	3
May	0	$1\frac{1}{2}$	$0\frac{1}{2}$	2
June	$1\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$	$4\frac{1}{2}$
July	0	$0\frac{1}{2}$	$1\frac{1}{2}$	2
August	$0\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$	$3\frac{1}{2}$
September	1	1	$0\frac{1}{2}$	$2\frac{1}{2}$
October ..	0	0	0	0
November	$1\frac{1}{2}$	$2\frac{1}{2}$	$1\frac{1}{2}$	$5\frac{1}{2}$
December	$0\frac{1}{2}$	1	$1\frac{1}{2}$	3
January ..	$0\frac{1}{2}$	2	1	$3\frac{1}{2}$
February ..	$0\frac{1}{2}$	0	$1\frac{1}{2}$	2
Sums	7	13	$12\frac{1}{2}$	$32\frac{1}{2}$

TABLE V.

Sub-division of the n. w.

	W. N. W.	N. W.	N. N. W.	Sums.
March	$0\frac{1}{2}$	$3\frac{1}{2}$	$2\frac{1}{2}$	$6\frac{1}{2}$
April	$1\frac{1}{2}$	1	1	$3\frac{1}{2}$
May	$1\frac{1}{2}$	4	1	$6\frac{1}{2}$
June	0	1	0	1
July	$1\frac{1}{2}$	5	1	$7\frac{1}{2}$
August	0	$2\frac{1}{2}$	1	$3\frac{1}{2}$
September	$0\frac{1}{2}$	1	0	$1\frac{1}{2}$
October ..	$0\frac{1}{2}$	2	1	$3\frac{1}{2}$
November	1	$1\frac{1}{2}$	$0\frac{1}{2}$	3
December	0	0	1	1
January	0	$0\frac{1}{2}$	1	$1\frac{1}{2}$
February ..	0	$0\frac{1}{2}$	0	$0\frac{1}{2}$
Sums	7	$22\frac{1}{2}$	10	$39\frac{1}{2}$

Of the winds between the s. and w. those from the s. s. w. were this year the most frequent. Here follows a general state of the winds, according to the degrees in which they prevailed respectively, collected from the 5 preceding tables.

E. S. E.	W. N. W.	N. N. W.	E.	S. S. E.	S. E.	W.	N. N. E.	S.	N. W.	N.	E. N. E.	N. E.	W. S. W.	S. W.	S. S. W.	Sum.
7	7	10	11	$12\frac{1}{2}$	13	$18\frac{1}{2}$	$20\frac{1}{2}$	$21\frac{1}{2}$	$22\frac{1}{2}$	$22\frac{1}{2}$	25	$26\frac{1}{2}$	45	$48\frac{1}{2}$	$54\frac{1}{2}$	$365\frac{1}{2}$
Missed in the journal																$\frac{1}{2}$

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TABLE VI.

Showing the number of fair and frosty days in each half month and in the whole year.

	Fair		Fair days in whole months	Frosty days		Frosty da. in whole months
	1st half	Latt. half		1st half	Latt. half	
March	6	7	13		4	4
April	15	9	24			
May	13	13	26			
June	12	11	23			
July	1	9	10			
August	5	7	12			
September	1	11	12			
October ..	11	6	17		1	1
November	8	5	13	1	1	2
December	15	9	24	2	6	8
January ..	5	11	16	10	15	25
February ..	7	2	9	1		1
Total fair days	199			Total frosty days, 41		

There were 11 snowy days in this year, all in January, with the wind between the n. and e. The first snow fell on the 7th, and introduced the great frost, which set in in the day-time: for on the 7th, at $8\frac{1}{2}$ in the morning, it rained with the thermometer at 33° , wind E. N. E.; but, at 2 in the afternoon of the same day, the rain was turned into snow, and the thermometer was sunk to 31° . There was a short remission of the frost on the 18th, the thermometer at 8 in the morning of that day being at 33° ; but it was sunk again to 30° at 2 in the afternoon. On the 31st, at 8 in the morning, it was at 13.5 , and only 1° higher the next morning, February 1. The frost broke in the night between the 1st and 2d of February, the wind yet continuing N. E., from which quarter it had set almost all the time the frost lasted. It changed to the s. e.

on the 2d, and on the 3d got into the s. w., where it remained almost all the rest of the month.

The following table shows the quantity of rain that fell with each wind in each month in the whole year. It appears, that the s. w. gave more than $\frac{2}{3}$ of the rain of the whole year, which seems not to have been altogether owing to the wet quality of that wind, but in great measure to the greater length of time it blew than any other. The numbers at the bottom of the table show the proportional wetness of each wind on the whole. They are made from the numbers in the last horizontal row but one of this table, compared with the numbers in the last horizontal row of tab. 1. For the wetness of each wind is in proportion as the quantity of rain it gave in the whole year directly, and the number of days it blew inversely. The former is shown by the numbers in the last row but one of tab. 7, and the latter by those in the last row of tab. 1. It appears that the s. wind was the driest of all, the s. w. the wettest, and the w. the next wettest.

TABLE VII.

Showing the quantities of rain which fell severally with each wind in every month and in the whole year.

	N.	S.	E.	W.	N.W.	S.E.	N.E.	S.W.	Sums.
March		0.132	0.114	0.074	0.095	1.439	1.854
April	0.016	.	0.386	0.143	.	.	0.523	1.068
May		0.058	.	.	0.052	0.442	0.552
June		0.768	.	0.495	0.098	0.028	1.389
July		0.155	0.528	.	0.214	3.335	4.232
August	0.039	0.038	.	.	0.043	.	2.083	2.203
September . .	.	0.107	.	0.378	0.184	.	0.542	3.981	5.192
October . . .	0.037	.	0.039	.	0.378	.	.	2.465	2.919
November . .	0.211	.	.	.	0.969	0.208	0.409	0.652	2.449
December	0.030	.	.	.	0.546	0.576
January . . .	0.079	.	0.091	.	0.075	0.031	0.447	0.444	1.167
February . .	.	0.089	.	.	.	0.093	0.291	3.037	3.510
Sums	0.327	0.251	0.168	1.907	2.391	0.944	2.148	18.975	27.111
	11 +	9 +	12 -	82 +	48	22 -	23 -	100	

An 8th table was added by Dr. H. for trial of the moon's influence, in which is shown the state of the weather on the several quarters of the moon, in all the months of the year; from which the following inferences are drawn. From this table it appears that, of 92 changes of weather in the whole year, 46 fell on the days of the moon's pretended influence. And rejecting, of these changes, all that were reversed within 24 hours, of 53 that remain in all, 27 fell on the days of lunar influence. And if from these again we reject the octantal days, confining the moon's influence to the days of syzygy and quadrature, there still remain 14 of the 53 for these days. Of the new moons, 4 only were attended with a change of weather, and of the full moons 3; namely, the new moons of

the months of March, July, December, and February: and the full moons of October, November, January. Both the setting in and the breaking of the great frost happened on days exempt from lunar influence. On the whole, the trial turns out more in favour of the moon this year than it did the last. But still the changes were many more on the days confessedly exempt from her influence, than on those which have been supposed to be the most subject to it.

The greatest monthly height of the barometer was only 4 times in this year accompanied with a N. E. wind, namely, in the months of April, June, November, and February. It was 5 times attended with a S. W. namely, in March, May, August, September, and December; and the greatest height observed in the whole year was one of these, namely, in the month of March. Once it was accompanied with the E. wind, namely, in January; and twice with a N. W. namely, in July and October. The least monthly height was once accompanied with a N. E. namely, in January; 6 times with a S. W. namely, in April, May, August, October, December, and February; twice with the S. wind, namely, in March and September; once with the N., namely in July; once with a S. E. in June; and once with a N. W. in November.

*XIX. Extract of a Meteorological Journal for the Year 1775, kept at Bristol.
By Samuel Farr, M. D. p. 367.*

Months.	Barometer.			Rain.
	Highest.	Lowest.	Mean.	
January ..	30.13	29.08	29.70	4.529
February ..	30.29	28.74	29.72	4.145
March	30.38	28.75	29.69	2.834
April	30.22	29.42	29.67	0.616
May	30.20	29.63	29.93	0.332
June	30.09	29.40	29.72	4.288
July	29.96	29.50	29.68	5.414
August	29.89	29.30	29.61	6.947
September	29.93	29.30	29.53	2.936
October ..	30.13	28.90	29.70	3.358
November	30.22	28.90	29.50	2.044
December	30.36	28.49	29.85	1.154
			29.69	38.597

XX. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1775. By Thomas Barker, Esq. p. 370.

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the House.			Abroad.			
					High.	Low.	Mean.	High.	Low.	Mean.	
Jan.	Morn.	29.91	28.72	29.33	47	30	40½	50	20	36½	1.973
	Aftern.				48	31	41½	52½	26	41	
Feb.	Morn.	29.91	28.35	29.24	48	39½	44	49	31½	39	2.522
	Aftern.				49	41	45	51½	36	46	
Mar.	Morn.	30.09	28.61	29.32	48	38	44	46½	28	36½	1.728
	Aftern.				49½	39½	45	54	34	46½	
Apr.	Morn.	29.97	29.05	29.60	64½	40½	49	55	36	44	1.035
	Aftern.				67	42½	51	80	47	55½	
May	Morn.	29.94	29.31	29.67	62	49½	55½	58½	36	49	0.900
	Aftern.				64	50½	57	73	53	61	
June	Morn.	29.87	29.17	29.49	66½	58	62	62	50	56	0.887
	Aftern.				68	59	64	78	59	69½	
July	Morn.	29.71	29.16	29.41	66½	58½	63	63	52	58	4.078
	Aftern.				68	60	64½	78	58½	70	
Aug.	Morn.	29.60	28.98	29.37	65	58½	62	61	48½	54½	4.760
	Aftern.				66	60	63	72	53	65	
Sept.	Morn.	29.67	29.02	29.31	64½	55½	60	60	45½	52½	5.670
	Aftern.				65½	56½	61	71	53	63	
Oct.	Morn.	29.80	28.50	29.38	59½	43½	51½	57½	30	43	3.480
	Aftern.				59½	45	52½	65	39	52	
Nov.	Morn.	29.96	28.50	29.34	48	39	42½	52	26½	36	3.570
	Aftern.				50	39	43	56	34	41½	
Dec.	Morn.	30.06	28.15	29.54	51	35½	42	52	24½	35	1.096
	Aftern.				51½	35½	42	55½	32	40	
Mean of all				29.42	51⅞			49⅝			31.699

In 4 years, 1740, 41, 42, and 43, there came but 66.361 inches of rain. In the last 4 years, 1772, 73, 74, and 75, there was 124.957 inches, which is nearly twice as much.

XXI. An Experiment of parting Fresh Water from Salt by Freezing. By the same. p. 373.

In the severe frost last January, some salt water, being set abroad, froze into an ice, which was not solid but porous, the hollows being filled with the saltiest part of the water, for the ice, when drained, was quite fresh. The salt water, being again set abroad, froze as before; what remained still unfrozen was now become exceedingly salt, but the ice, drained and dissolved, was little if at all brackish. This agrees with what Captain Cook mentions in his late voyage, that in 61° 35' south latitude they filled their water casks with fresh water, melted out of ice found floating in the sea. By this experiment, if another time more fully repeated, it may be found to what degree the saltness of water may be increased, by continuing to freeze away the fresh water. May not the

knowledge of this be of use to the salt makers, especially in cold countries? The sun is strong enough of itself between the tropics to dry away the seawater into salt; and at the salt works near Lymington, they increase the saltiness of the seawater by drying it away in the sun before they boil it into salt. And this seems to be another means of parting fresh water from the salt, which would save expence in boiling it away, and may be of use in the cold countries, and in winter.

XXI. An Account of the Meteorological Instruments used at the Royal Society's House. By the Hon. Henry Cavendish, F. R. S. p. 375.

Of the Thermometers, with reflections concerning some precautions necessary to be used in making experiments with those instruments, and in adjusting their fixed points.—The thermometers are both adjusted to Fahrenheit's scale: that without doors is placed out of a two-pair-of-stairs window, looking to the north, and stands about 2 or 3 inches from the wall, that it may be the more exposed to the air, and the less affected by the heat and cold of the house. The situation is tolerably airy, as neither the buildings opposite to it, nor those on each side, are elevated above it in an angle of more than 12° ; but as the opposite building is only 25 feet distant, perhaps the heat may be a little increased at the time of the afternoon observation by the reflection from it. In the middle of summer the sun shines on the wall of the house, against which the thermometer is fixed, for an hour or two before the morning observation, but never shines on the thermometer itself, or that part of the wall close to it, except in the afternoon, long after the time of observing. On the whole, the situation is not altogether such as could be wished, but is the best the house afforded. The thermometer within doors is intended chiefly for correcting the heights of the barometer, and is therefore placed close to it. The room in which it is kept looks to the north, and has sometimes a fire in it, but not often.

It has been too common a custom, both in making experiments with thermometers and in adjusting their fixed points, to pay no regard to the heat of that part of the quicksilver which is contained in the tube, though this is a circumstance which ought by no means to be disregarded; for a thermometer, dipped into a liquor of the heat of boiling water, will stand at least 2° higher, if it is immersed to such a depth that the quicksilver in the tube is heated to the same degree as that in the ball, then if it is immersed no lower than the freezing point, and the rest of the tube is not much warmer than the air. The only accurate method is, to take care that all parts of the quicksilver should be heated equally. For this reason, in trying the heat of liquor much hotter or colder than the air, the thermometer ought, if possible, to be immersed almost as far as to

the top of the column of quicksilver in the tube. As this however would frequently be attended with great inconvenience, the observer will often be obliged to content himself with immersing it to a much less depth; but then, as the quicksilver in a great part of the tube will be of a different heat from that in the ball, it will be necessary to apply a correction on that account to the heat shown by the thermometer; to facilitate which the following table is given, in which the upper horizontal line is the length of the column of quicksilver contained in that part of the tube which is not immersed in the liquor expressed in degrees; the first perpendicular column is the supposed difference of heat of the quicksilver in that part of the tube and in the ball; and the corresponding numbers in the table show how much higher or lower the thermometer stands than it ought to do. The foundation on which the table is computed is, that quicksilver expands one 11500th part of its bulk by each degree of heat.

Diff. of Heat	Degrees not immersed in the liquors.														
	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750
50	.2	.4	.7	.9	1.1	1.3	1.5	1.7	2.0	2.2	2.4	2.6	2.8	3.1	3.3
100	.4	.9	1.3	1.8	2.2	2.6	3.0	3.5	3.9	4.4	4.8	5.2	5.7	6.1	6.6
150	.7	1.3	2.0	2.6	3.3	3.8	4.6	5.2	5.9	6.5	7.2	7.9	8.4	9.2	9.8
200	.9	1.8	2.6	3.5	4.4	5.1	6.1	7.0	7.8	8.7	9.6	10	11	12	13
250	1.1	2.2	3.3	4.4	5.5	6.4	7.6	8.7	9.8	11	12	13	14	15	16
300	1.3	2.6	3.8	5.1	6.4	7.7	9.1	10	12	13	14	16	17	18	20
350	1.5	3.0	4.6	6.1	7.6	9.1	11	12	14	15	17	18	20	21	23
400	1.7	3.5	5.2	7.0	8.7	10	12	14	16	17	19	21	23	24	26
450	2.0	3.9	5.9	7.8	9.8	12	14	16	18	20	22	24	25	27	29
500	2.2	4.4	6.5	8.7	11	13	15	17	20	22	24	26	28	31	33
550	2.4	4.8	7.2	9.6	12	14	17	19	22	24	26	29	31	34	36

But as the generality of observers will be apt to neglect this correction, it would be proper to form two sets of divisions on such thermometers as are intended for trying the heat of liquors; one of which should be used when the tube is immersed almost to the top of the column of quicksilver; and the other when not much more than the ball is immersed; in which last case the observer should be careful, that the tube should be as little heated by the steam of the liquor as possible. It must be observed however, that the heat of the liquor may be estimated with much more accuracy by the first set of divisions, with the help of the correction, than it can by the second set, as the latter method is just only in one particular heat of the atmosphere, namely, that to which the divisions are adapted; but if they are adapted to the mean heat of the climate for which the thermometer is intended, the error can never be very great, and, when the liquor is much hotter or colder than the air of that climate ever is, will be much less than if the first set of divisions were used without any correction; but, when the liquor is within the limits of the heat of the atmosphere, greater accuracy will sometimes be obtained by using the first set of divisions

than the second, for which reason the latter set should not be continued within those limits. I would willingly have given rules for the construction of this second set of divisions, but am obliged to omit it, as it cannot be done properly without first determining, by experiment, how much the quicksilver in the tube is heated by immersing the ball in hot liquors.

In a spirit thermometer, the error proceeding from the fluid in the tube being not of the same heat as that in the ball, is much greater; as spirits of wine expand much more by heat than quicksilver: for which reason spirit thermometers are not so proper for trying the heat of liquors, as those of quicksilver. Another circumstance which ought to be attended to, in adjusting the boiling point of a thermometer is, that the ball should not be immersed deep in the water; for if it is, the fluid which surrounds it will be compressed by considerably more than the weight of the atmosphere, and will therefore acquire a sensibly greater heat than it would otherwise do. Mr. C. here describes a vessel to inclose the thermometer in, to adjust that point, with a chimney to carry off the steam. If such a vessel as this is used, the thermometer will be found to stand not sensibly higher when the water boils vehemently, than when it boils gently; and if the mouth of the chimney is covered by any light body, in such manner as to leave no more passage for the steam than what is necessary to prevent the body from being blown off by the pressure of the included vapour, the thermometer will stand only half or three quarters of a degree higher, if the ball is immersed a little way in the water, than if it is exposed only to the steam. But if the covering of the chimney is removed, the thermometer will immediately sink several degrees, when the ball is exposed only to the steam, at least if the cover does not fit close; whereas when the ball is immersed in the water, the removal of the covering has scarcely any effect on it. Whence it appears, that the steam of water boiling in a vessel, from which the air is perfectly excluded, is a little, but not much, cooler than the water itself, but is considerably so if the air has the least admission to the vessel. Perhaps a still more convenient method of adjusting the boiling point would be, not to immerse the ball in the water at all, but to expose it only to the steam, as thus the trouble of keeping the water in the vessel to the right depth would be avoided; and besides, several thermometers might be adjusted at the same time, which cannot be done with proper accuracy when they are immersed in the water, unless the distance of the boiling point from the ball is nearly the same in all of them. At present there is so little uniformity observed in the manner of adjusting thermometers, that the boiling point, in instruments by our best artists, differ from each other by not less than $2\frac{1}{4}^{\circ}$; owing partly to a difference in the height of the barometer at which they were adjusted, and partly to the quicksilver in the tube being more heated in the method used by some persons, than in that used by

others. It is very much to be wished therefore, that some means were used to establish a uniform method of proceeding: and there are none which seem more proper, or more likely to be effectual, than that the R. S. should take it into consideration, and recommend that method of proceeding which shall appear to them to be most expedient.

Of the Barometer, Rain-gage, Wind, and Hygrometer.

The barometer is of the cistern kind, and the height of the quicksilver is estimated by the top of its convex surface, and not by the edge where it touches the glass, the index being properly adapted for that purpose. This manner of observing appears more accurate than the other; because, if the quicksilver should adhere less to the tube, or be less convex at one time than another, the edge will, in all probability, be more affected by this inequality than the surface. I prefer the cistern to the syphon barometer, because both the trouble of observing and error of observation are less; as in the latter we are liable to an error in observing both legs. Moreover, the quicksilver can hardly fail of settling truer in the former than in the latter; for the error in the settling of the quicksilver can proceed only from the adhesion of its edge to the sides of the tube; now the latter is affected by the adhesion in 2 legs, and the former by that in only 1: and besides, as the air has necessarily access to the lower leg of the syphon barometer, the adhesion of the quicksilver in it to the tube will most likely be different, according to the degree of dryness or cleanness of the glass. It is true, as Mr. De Luc observes, that the cistern barometer does not give the true pressure of the atmosphere; the quicksilver in it being a little depressed on the same principle as in capillary tubes. But this does not appear a sufficient reason for rejecting the use of them. It is better, I think, where so much nicety is required, to determine, by experiment, how much the quicksilver is depressed in tubes of a given bore, and to allow accordingly. By some experiments which have been made on this subject by my father, Lord Charles Cavendish, the depression appears to be as in the following table:

Inside diameter of tube. Inch.	Grains of quicksilver in one inch of tube.	Depress. of surface of quicksilv.	Inside diameter. Inch.	Grains of quicksilv.	Depress. of surface.	Inside diameter. Inch.	Grains of quicksilv.	Depress. of surface.
.6	972	.005	.35	331	.025	.20	108	.067
.5	675	.007	.30	243	.036	.15	61	.092
.4	432	.015	.25	169	.050	.10	27	.140

In this barometer, the inside diameter of the tube is about .25 of an inch, and consequently the depression is .05; the area of the cistern is near 120 times as great as that of the bore of the tube; so that as the quantity of quicksilver was adjusted when the barometer stood at $29\frac{3}{4}$, the error arising from the alteration of the height of the quicksilver in the cistern can scarcely ever amount

to so much as $\frac{1}{100}$ th of an inch. As the tube appeared to be well filled, it was thought unnecessary to have the quicksilver boiled in it; but that is certainly the surest way of filling a barometer well.

The principal reason of setting down the mean heat of the thermometer within doors, during each month, in the journal of the weather, is this: suppose that any one desires to find the mean height of the barometer in any month, corrected on account of the heat of the quicksilver in the tube; that is, to find what would have been the mean height, if the quicksilver in the tube had been constantly of a certain given heat. To do this, it is sufficient to take the mean height of the barometer, and correct that according to the mean heat of the thermometer; the result will be exactly the same as if each observation had been corrected separately, and a mean of the corrected observations taken. For example, suppose it is desired to find what would have been the mean height of the barometer in the month of August 1775, if the quicksilver during that time had been always at 50 degrees of heat: the mean of the observed heights is 29.86 inches, and the mean heat of the thermometer is 65° or $50 + 15$. The alteration of the height of the barometer by 15° of heat, according to M. De Luc's rule, is .047 inches; consequently the corrected mean height is 29.813.

The vessel which receives the rain is a conical funnel, strengthened at the top by a brass ring, 12 inches in diameter. The sides of the funnel and inner lip of the brass ring are inclined to the horizon, in an angle of above 65° ; and the outer lip in an angle of above 50° ; which are such degrees of steepness, that there seems no probability either that any rain which falls within the funnel, or on the inner lip of the ring, should dash out, or that any which falls on the outer lip should dash into the funnel. This vessel is placed on some flat leads on the top of the Society's house. It can hardly be screened from any rain by the chimnies, as none of them are elevated above it in an angle of more than 25° ; and as it is raised $3\frac{1}{2}$ feet above the roof, there seems no danger of any rain dashing into it by rebounding from the lead.

The strength of the wind is divided in the journal into 3 degrees; namely, gentle, brisk, and violent or stormy, which are distinguished by the figures 1, 2, and 3. When there is no sensible wind, it is distinguished by a cypher.

In the future journals of the weather will be given observations of the hygrometer. The instrument intended to be used is of Mr. Smeaton's construction, and is described in Phil. Trans. vol. 61, p. 198. It is kept in a wooden case, made so as to exclude the rain, but to leave a free passage for the wind, and placed in the open air, where the sun scarcely ever shines on it. The instrument and case are both a present to the Society from Mr. Smeaton. The

hygrometer was last adjusted in Dec. 1775, and as the string has now been in use upwards of 5 years, it is not likely to want re-adjusting soon.

Of the Variation Compass.—In this instrument, the box which holds the needle is not fixed, but turns horizontally on a centre, and has an index fastened to it, pointing to a divided arch on the brass frame on which it turns; and the method of observing is, to move the box till a line drawn on it points exactly to the end of the needle; which being done, the angle that the needle makes with the side of the frame is shown by the index. It was by this method that the error of the instrument, at the time of the observations in 1774, was found to be 10'. For example, by a mean of the observations made on Sept. 5, the variation with the needle, in its upright position, was 21.36 by the south end, and 21.27 by the north; with the needle inverted it was 21.19 by the south end and 21.29 by the north. The mean of all four is 21.28, which is the true variation at that time and place, and is 8' less than that shown in the upright position of the needle by the south end, which is the end always used in observing; so that by this day's experiment the error of the instrument appeared to be 8'; but by a mean of the observations of this and two other days it came out 10'. Since that time the needle has been altered; and, at the time of the observations in 1775, the error was so small as to be scarcely sensible.

Great care was taken that the metal, of which this variation compass is composed, should be perfectly free from magnetism. There is a contrivance in it for lifting the needle from off the point, and letting it down gently, to prevent injury in carrying from one room to another. The instrument is constructed nearly on the same plan as some made by the late Dr. Knight. The principal difference is, that in his the pin which carried the needle was not fixed to the lower frame as in this, but to the box; the consequence of which was, that when the needle had settled, and the box was moved to make the index on the needle point to the proper mark, it was again put into vibration, which caused great trouble to the observer. This inconvenience is entirely removed by the present construction. There is no other material difference, except that of the needle being made to invert, and the addition of the telescope. The contrivance of fixing the pin which carries the needle to the lower frame, is taken from an instrument of Lord Charles Cavendish; that of making the needle invert I have seen in some compasses made by Sisson.

There is a very common fault in the agate-caps usually made for needles, which is, that they are not hollowed to a regular concave, but have a little projecting part in the centre of the hollow; the consequence of which is, that the point of the pin will not always bear against the same part of the agate, and consequently the needle will not always stand horizontal; but sometimes one end

will stand higher, and sometimes the other, which causes a difficulty in observing. Another inconvenience also attends it when the indices of the needle are on a level with the point of the pin, which is of more consequence; namely, that it causes the two indices not to agree, and consequently makes a sensible error, when only one index is made use of, at least in nice observations: but when the lines, serving by way of index, are drawn on the needle itself, and therefore are nearly on a level with its centre of gravity, it can cause very little error. The agate cap, which was first made for this instrument, was of this kind; and was so faulty, that if no better could have been procured, it would have been necessary either to have drawn the lines serving by way of index on the needle itself, or to have observed by both ends, either of which would have been attended with a considerable increase of trouble to the observer; but Mr. Nairne, the artist who made the instrument, has since ground some himself, which are perfectly free from this fault, the concave surface being of an extremely regular shape and well polished, and also of a very small radius of curvature; which is a matter of considerable consequence, as otherwise the point of the pin will not easily slip sufficiently near to the bottom of the hollow.

Care was taken to place the variation compass in a part of the house where it is as little likely to be affected by the attraction of the iron work, as in any that could be found. As it seemed however to be not entirely out of the reach of the influence of that metal, I took the following method to examine how much it was influenced by it. The instrument was removed into a large garden belonging to a house in Marlborough-street, distant from the Society's house about one mile and a quarter towards the west, where there seemed no danger of its being affected by any iron-work. Here it was placed exactly in the meridian, and compared for a few days with a very exact compass, placed in an adjoining room, and kept fixed constantly in the same situation. It was then removed back to the Society's house, and compared again with the same compass. By a mean of the observations, the variation shown by the compass in the room, is $1'.3$ greater than by the Society's instrument in the garden, and $14'.1$ less than by the same instrument placed in its proper situation; so that the variation appears to be $15'.4$ greater in that part of the Society's house where the compass is placed, than in the abovementioned garden; and therefore, as there is no likelihood of its being affected by any iron in the latter place, the needle seems to be drawn aside $15\frac{1}{2}'$ towards the N. W. by the iron work of the house and adjacent buildings.

On comparing the observations of the last two years together, the variation appears, after allowing for the error of the instrument, to have been $27'$ greater in 1775 than in 1774; though I have been informed by Dr. Heberden, who has

made observations of this kind for several years past, that the annual alteration of the variation has, in general, been not more than $10'$; and in particular, that the alteration in the last year appears to be only $11\frac{1}{2}'$; so that the great difference observed at the Society's house seems to be owing, not solely to the real alteration in the variation, but partly to some other cause; though what that should be I cannot conceive, unless some change was made in the iron work either of this or the adjoining houses between the two periods; but I do not find that any such change has been made. During the last year indeed there have been two large magnets in the house, each consisting of several great bars joined together, being what the late Dr. Knight used for making artificial magnets, and at the time of the observations in 1774 there was only one; but their distance from the compass is above 50 feet: and I am well assured, that in the situation in which they are actually placed, they cannot draw the needle aside more than $3'$, and not more than $15'$, when the line joining their poles is placed in such a direction as to act with most force.* The single magnet in the year 1774 was placed nearly in the same situation and direction that the two were in 1775, so that the difference of their effect in these 2 years can hardly have been so much as $3'$; and therefore the great apparent alteration of the variation, between the two periods, cannot have been owing to them. Neither can it have been owing to the fault of the agate cap used in the year 1774, as the error proceeding from thence could hardly be more than 2 or $3'$. It is intended that, for the future, the abovementioned magnets shall be kept always in the same situation and direction that they are in at present, and in which they were in 1775.

Of the Dipping Needle.—In this instrument the ends of the axis of the needle roll on horizontal agate planes, a contrivance being applied, by which the needle is at pleasure lifted off from the planes, and let down on them again, in such manner as to be supported always by the same points of the axis resting on the same parts of the agate planes; and the motion with which it is let down is very gradual and without shake. The general form of the instrument, the size and shape of the needle, and the cross used for balancing it, are the same as in the dipping needle described in Phil. Trans., vol. 62, p. 476. It is also made by the same artist, Mr. Nairne. It may be seen in the Meteorological Journal, that the dip was observed first with the front of the instrument to the west, and then to the east; after which the poles of the needle were reversed, and the dip ob-

* The principle by which this was determined is, that if a magnet is placed near a variation compass, with its poles equi-distant from it, and situated so that each shall act equally oblique to the length of the needle, it can have no tendency to alter the variation; and that the situation in which it alters it most, except when placed nearly north or south of the compass, is when the line joining its poles points almost directly towards the needle. This experiment I tried purposely on the occasion, and found it answer; but I believe any one skilled in magnetism would have granted the truth of the position without that precaution.—Orig.

served both ways as before. The reason of this is, that the mean of the observed dips, in these 4 situations, differs very little from the truth, though the needle is not well balanced, and even though a great many other errors are committed in the construction of the instrument; provided the needle is made equally magnetical after the poles are reversed as before;* and that the difference of the observed dip, in these 4 situations, is not very great.

The error which is most difficult to be avoided, is that which proceeds from the ends of the axis being not truly cylindrical. I before said, that the parts of them which rest on the agate planes are always exactly the same. The instrument is so contrived however that we may on occasion, by giving the axis a little liberty in the notches by which it is lifted up and down, make those planes bear against a part of the axis distant about $\frac{1}{100}$ or $\frac{1}{50}$ th of an inch from their usual point of bearing. Now I find that when the axis is confined, so as to have none of this liberty, and when care is taken, by previously making the needle stand at nearly the right dip, that it shall vibrate in very small arches when let down on the planes; that then, if the needle is lifted up and down any number of times, it will commonly settle exactly at the same point each time, at least the difference is so small as to be scarcely sensible; but if it is not so confined, there will often be a difference of 20' in the dip, according as different parts of the axis rest on the planes, and that though care is taken to free the axis and planes from dust as perfectly as possible, which can be owing only to some irregularity in the axis. Also, if the needle vibrates in arches of 5 or more degrees, when let down on the planes, there will frequently be as great an error in the dip. It is true that the part of the agate planes which the axis rests on, when the vibrations are stopped, will be a little different according to the point which the needle stood at before it was let down; which will make a small difference in the dip as shown by the divided circles, when only one end of the needle is observed, though the real dip or inclination of the needle to the horizon is not altered: but this difference is by much too small to be perceived; so that the abovementioned error cannot be owing to this cause. Neither does it seem owing to any irregularity in the surface of the agate planes, for they were ground and polished with great accuracy; but it most likely proceeds from the axis slipping in the large vibrations, so as to make the agate planes bear against a different part of it from what they would otherwise do. I have great reason to think that this irregularity is not owing either to want of care or skill in the execution, but to the unavoidable imperfection of this kind of work. I imagine too that this instrument is at least as exact, if not more so, than any which has been yet made. By a mean of all the experiments with several needles, the true dip at

* It is easy to see whether the needle is made equally magnetical after the poles are reversed as before, by counting the number of vibrations which it makes in a minute.—Orig.

London, at this time, comes out $72^{\circ} 30'$, the different needles all agreeing within $14'$, which is a difference considerably less than I should have expected. It appears also, that the dipping needle, in the situation in which it is placed at the Society's house, is not much affected by any iron work, as the dip shown by it in the garden differs only $7'$ from that set down in the journal of the weather.

According to Norman, the inventor of the dipping needle, the dip at London in the year 1576 was $71^{\circ} 50'$; * in 1676 it was $73^{\circ} 47'$, according to Mr. Bond; † Mr. Whiston in 1720 made it $75^{\circ} 10'$; ‡ Mr. Graham in 1723 made it between $73\frac{1}{2}$ and 75° , § his different trials varying so much; and at present it appears to be $72^{\circ} 30'$. I do not know how much Mr. Bond's determination is to be depended on, as he does not say by what means he arrived at it; but I believe Mr. Whiston's is pretty accurate, for he observed the dip in many parts of the kingdom, and the observations agree well together; so that it is reasonable to suppose, that his instrument was a good one, and that he observed in places where the needle was not much influenced by iron work. The dip therefore seems to have been considerably greater about the year 1720, than it was in Norman's time, or is at present: it appears however to alter very slowly in comparison of the variation.

XXII.¶ *The Method taken for Preserving the Health of the Crew of his Majesty's Ship the Resolution during her late Voyage round the World. By Captain James Cook, F. R. S. p. 402.*

As many gentlemen have expressed some surprize at the uncommon good state of health which the crew of the Resolution, under my command, experienced during her late voyage; I take the liberty to communicate the methods that were taken to obtain that end. Much was owing to the extraordinary attention given by the Admiralty, in causing such articles to be put on board, as either by experience or conjecture were judged to tend most to preserve the health of seamen. I shall not trespass on your time in mentioning all those articles, but confine myself to such as were found the most useful.

We had on board a large quantity of malt, of which was made sweet wort, and given (not only to those men who had manifest symptoms of the scurvy, but to such also as were, from circumstances, judged to be most liable to that disorder) from 1 to 2 or 3 pints in the day to each man, or in such proportion as the surgeon thought necessary; which sometimes amounted to 3 quarts in the

* New Attractive, c. 4.—Orig.

† Longitude found, p. 65.—Orig.

‡ Longitude and Latitude found by dipping needle, p. 7, 49, and 94.—Orig.

§ Phil. Trans. No. 389, p. 332.—Orig.

¶ For this paper, Capt. Cook was honoured with the gold medal, which is annually presented by the R. S.

24 hours. This is doubtless one of the best antiscorbutic sea medicines yet found out; and if given in time will, with proper attention to other things, I am persuaded, prevent the scurvy from making any great progress for a considerable time: but I am not altogether of opinion that it will cure it in an advanced state at sea.

Sour kroust, of which we had also a large provision, is not only a wholesome vegetable food, but, in my judgment, highly antiscorbutic, and spoils not by keeping. A pound of it was served to each man, when at sea, twice a week, or oftener when it was thought necessary. Portable soup or broth, was another essential article, of which we had likewise a liberal supply. An ounce of this to each man, or such other proportion as was thought necessary, was boiled with their pease 3 days in the week; and when we were in places where fresh vegetables could be procured, it was boiled with them and with wheat or oatmeal, every morning for breakfast, and also with dried pease and fresh vegetables for dinner. It enabled us to make several nourishing and wholesome messes, and was the means of making the people eat a greater quantity of 'greens than they would otherwise have done. We were also provided with rob of lemons and oranges; which the surgeon found useful in several cases.

Among other articles of victualling we were furnished with sugar instead of oil, and with wheat instead of much oatmeal, and were certainly gainers by the exchange. Sugar, I imagine, is a very good antiscorbutic; whereas oil, such at least as is usually given to the navy, I apprehend has the contrary effect. But the introduction of the most salutary articles, either as provision or medicines, will generally prove unsuccessful, unless supported by certain rules of living.

On this principle, many years experience, together with some hints I had from Sir Hugh Palliser, the Captains Campbell, Wallis, and other intelligent officers, enabled me to lay down a plan by which all was to be conducted. The crew were at 3 watches, except on some extraordinary occasions. By this means they were not so much exposed to the weather as if they had been at watch and watch: and they had generally dry clothes to shift themselves when they happened to get wet. Care was also taken to expose them as little as possible. Proper methods were employed to keep their persons, hammocks, bedding, clothes, &c. constantly clean and dry. Equal pains were taken to keep the ship clean and dry between decks. Once or twice a week she was aired with fires; and when this could not be done, she was smoked with gunpowder moistened with vinegar or water. I had also frequently a fire made in an iron pot at the bottom of the well, which greatly purified the air in the lower parts of the ship. To this and cleanliness, as well in the ship as among the people, too great attention cannot be paid; the least neglect occasions a putrid, offensive smell below, which nothing but fires will remove: and if these be not used in time,

those smells will be attended with bad consequences. Proper care was taken of the ship's coppers, so that they were kept constantly clean. The fat, which boiled out of the salt beef and pork, I never suffered to be given to the people, as is customary: being of opinion that it promotes the scurvy. I never failed to take in water wherever it was to be procured, even when we did not seem to want it; because I look upon fresh water from the shore to be much more wholesome than that which has been kept some time on board. Of this essential article we were never at an allowance, but had always abundance for every necessary purpose. I am convinced, that with plenty of fresh water, and a close attention to cleanliness, a ship's company will seldom be much afflicted with the scurvy, though they should not be provided with any of the antiscorbutics before-mentioned. We came to few places where either the art of man or nature did not afford some sort of refreshment or other, either of the animal or vegetable kind. It was my first care to procure what could be met with of either by every means in my power, and to oblige our people to make use of them, both by my example and authority; but the benefits arising from such refreshments soon became so obvious, that I had little occasion to employ either the one or the other.

These were the methods, under the care of Providence, by which the Resolution performed a voyage of 3 years and 18 days, through all the climates from 52° north to 71° south, with the loss of one man only by disease, and who died of a complicated and lingering illness, without any mixture of scurvy. Two others were unfortunately drowned, and one killed by a fall; so that, of the whole number with which I set out from England, I lost only 4.

Extract of a Letter from Captain Cook to Sir John Pringle, dated Plymouth Sound, July 7, 1776.

I entirely agree with you, Sir, that the dearness of the rob of lemons and of oranges will hinder them from being furnished in large quantities, but I do not think this so necessary; for though they may assist other things, I have no great opinion of them alone. Nor have I a higher opinion of vinegar: my people had it very sparingly during the late voyage; and towards the latter part none at all; and yet we experienced no ill effects from the want of it. The custom of washing the inside of the ship with vinegar I seldom observed, thinking that fire and smoke answered the purpose much better.

XXIII. Extraordinary Electricity of the Atmosphere observed at Islington in October, 1775. By Mr. Tiberius Cavallo. p. 407.

Before I enter on the particular narration of the observation made with an electrical kite on the 18th of last October, it will be necessary (says Mr. C.) to give an idea of the scale of my quadrant electrometer used on the occasion,

which, being constructed in some measure different from what are commonly sold in shops, will no doubt give an unsettled idea of my narration, by expressing the same intensity of electricity under different degrees from the others. In order to this, therefore, it must be observed, that when the kite is raised, I generally connect with the end of its string a small cylindrical conductor, 9 inches long and 1 inch diameter, made of pasteboard covered with tin foil; with this I connect the quadrant electrometer, which shows me exactly the state, increase, and decrease of electricity. The apparatus being thus disposed, I have observed, that when the electrometer is at 10° , a little bran presented to the conductor will be attracted by it at the distance of about six-tenths of an inch; when the electrometer is at 20° , the bran will be attracted at the distance of $1\frac{1}{4}$ inch; when at 30° , it will be attracted at the distance of $2\frac{1}{5}$ inches; and when at 35° , it will be attracted at the distance of 3 inches. The experiment is as follows:

Oct. the 18th, after having rained a great deal in the morning and night before, the weather got a little clear in the afternoon, the clouds appearing separated and pretty well defined; the wind was west, and rather strong, and the atmosphere in a temperate degree of heat. In these circumstances, at 3 o'clock in the afternoon, Mr. C. raised a small electrical kite, which measured 3 feet 9 inches in length, and 3 feet in breadth, giving to it 360 feet of wired string. The angle that the string, or rather the cord of the incurvated string, generally made with the horizon, was near 60° , and in consequence the kite's perpendicular height was about 310 feet. After the end of the string had been insulated with a silk lace, and a leathern ball covered with tin foil had been hanged to it, he tried the power and quality of the electricity, and found it positive and pretty strong; in a little time a small cloud passing over, the electricity increased a little; but the cloud being gone, it decreased again to its former degree. The string of the kite now was fastened by the silk lace to a post in the yard of the house where he lived which is situated near Islington; and he was amusing himself and some other persons with charging 2 coated phials, and giving several shocks with them. While so doing, the electricity, still positive, began to decrease; and in 2 or 3 minutes time it was so weak, that it could be hardly perceived with a very sensible electrometer, made with 2 cork balls after Mr. Canton's manner. Seeing at the same time, that a large and black cloud was approaching the zenith, which no doubt caused the decrease of electricity, indicating imminent rain, he introduced the end of a string through a window in a first-floor room, in which he fastened it by the silk lace to an old chair; the quadrant electrometer was fixed on the same window, and was connected by a wire with the string of the kite. Being now $\frac{3}{4}$ of an hour after 3 o'clock, the electricity was absolutely unperceivable: however, in 2 or 3 minutes time it began

again to appear, but now, on trial, was found to be negative; so that it was plain, that its stopping was no more than a change from positive to negative, which was evidently occasioned by the approach of the cloud; part of which by this time had reached the zenith of the kite, and the rain also had begun to fall in large drops: The cloud came further on, the rain increased, and the electricity keeping pace with it, the electrometer soon arrived to 15° . Seeing now that the electricity was pretty strong, Mr. C. took again the 2 coated phials, and began again to charge them, and to give shocks to several by-standers; but the phials were not charged above 3 or 4 times, before he perceived that the electrometer was arrived to 35° , and was still increasing. The shocks now being very smart, he desisted from charging the phials any longer; and considering the rapid advances of the electrometer, thought to take off the insulation of the string, in case that, if the electricity should increase further, it might be silently conducted to the earth, without causing any bad accident by being accumulated in the insulated string. To effect this, as Mr. C. had no proper apparatus at hand, he thought to take away the silk lace, and fasten the string itself to the chair; accordingly he disengaged the wire that connected with the electrometer, laid hold of the string, untied it from the silk lace, and fastened it to the chair. But while he effected this, which took up less than half a minute, he received about 12 or 15 very hard shocks, which he felt all along his arms, his breast and legs, shaking him in such a manner, that he had hardly power enough to effect his purpose, and to warn the people in the room to keep their distance. As soon as he took his hand off of the string, the electrical fluid, in consequence of the chair being a bad conductor, began to snap between the string and the shutter of the window, which was the nearest body to it. The snappings, which were audible at a good distance out of the room, seemed at first isochronous with the shock he had received; but in about a minute's time they became more frequent, so that the people of the house compared their sound to the rattling noise of a jack going when the fly is off. The cloud was now just over the kite; it was black and pretty well defined, of almost a circular form, its diameter appearing to be about 40° . The rain was copious, but not remarkably heavy. As the cloud was going off, the electrical snappings began to weaken, and in a short time became inaudible. Mr. C. then went near the string, and finding the electricity weak, but still negative, he insulated it again, thinking to keep the kite up some time longer; but as another larger and denser cloud was approaching apace towards the zenith, and he had then no proper apparatus to prevent bad accidents, he resolved to pull the kite in. Accordingly a gentleman, who was by, began pulling it in, while Mr. C. was winding up the string. The other cloud was now very nearly over the kite; and the gentleman who was pulling in the string said that he had received 1 or 2

slight shocks in his arms; and that if he were to receive one more, he would certainly let the string go. On which Mr. C. laid hold of the string, and pulled the kite in as fast as he could, without any further observation. When the kite was pulled in, it was 10 minutes after 4 o'clock; so that all the time that this experiment took up was 1 hour and 10 minutes. There was neither thunder nor lightning perceived that day, nor indeed for some days before or afterwards.

XXIV. Proposals for the Recovery of People Apparently Drowned. By John Hunter, Esq., F. R. S. p. 412.

Reprinted in Mr. J. H.'s *Observations on the Animal Economy*, 4to., 1786.

XXV. An Extraordinary Cure of Wounded Intestines. By Charles Nourse, Surgeon, Oxford. p. 426.

In the evening of the 26th of Sept., 1775, Mr. N. was called in great haste to James Langford, a young man in his 21st year, who had been maliciously stabbed, with a knife, in the left side of his belly. The wound was between 2 and 3 inches in length, running from the left os ilium obliquely upwards towards the navel. Mr. N. found him lying on the floor, weltering in his blood, with a large portion of his intestines forced through the wound; and he learnt, from the unhappy youth himself, that as soon as the wound was inflicted, the bowels began to appear; and by the time he got to him, which could not exceed 10 minutes, he verily believes, that the full half of the intestinal tube was protruded through the opening. This he attributed in some measure to the fulness of the stomach; for, immediately before the accident happened, he had eaten a very hearty supper. The wound at first bled freely; but the hæmorrhage was soon restrained by the pressure of the prolapsed intestines, which were, to a great degree, distended with air; and from this circumstance he was flattered with the best hopes that they had escaped the assassin's knife; but, to his great disappointment, it proved otherwise. Examining the man's pulse, he found it was exceedingly low, quick, and interrupted; his skin was all over cold and clammy, and he laboured under great languor, anxiety, and pain about the præcordia. He also complained of a disagreeable tingling and numbness of the whole thigh, leg, and foot, of the side wounded; and said, that he dropped on the floor in consequence of the inability of the limb to support him, and not from any faintness, as might have been reasonably expected from the loss of blood, or through fear, to which indeed he seemed an utter stranger. Mr. N. ordered him to be conveyed to his bed in a horizontal posture, lest the raising of the body might encourage a farther descent of the parts which still remained in the abdomen; and a fomentation of port wine with warm water to be got ready immediately, out of which a double flannel should be wrung, and applied directly to the pro-

lapsed intestines, and renewed occasionally, to prevent them from getting too dry, as well as to preserve as much as possible their natural heat. The reduction of the displaced bowels was begun with laying the patient's legs over an assistant's shoulders, who was desired to kneel on the bed for that purpose, with his back towards him, and then the legs were brought forward as far as to the hams. By this means the lower parts of the body were elevated, and in consequence the weight of the bowels falling back towards the chest, counteracted their further protrusion. While the patient continued in this position, Mr. N. endeavoured, with his hands, to force the guts back into their proper place; but soon found, from the quantity of them protruded, with their great inflation, that a larger or more extended pressure than his own hands could afford him was necessary; and not thinking it prudent to employ any of the by-standers in so hazardous a task, lest by their inexperience they might handle the bowels too roughly, he sent for 2 of his fellow-labourers in the care of the Radcliffe Infirmary, to his assistance. As soon as they came, the reduction was again attempted; one of them directing that portion of the bowel which was last protruded, while the 2 others made a gentle, regular, and circumscribed pressure from all sides towards the opening. But this endeavour not succeeding, convinced them, that it would be much safer to enlarge the wound, to facilitate the return of the prolapsed parts, than hazard the necessity of handling them too much, or exposing them too long to the external air, either of which would, in all probability, have proved fatal. This being done accordingly, by continuing the wound in the same direction upwards about 2 inches, the exposed bowels were easily and soon returned into the abdomen. They then brought the edges of the wound together, and kept them by the suture called gastroraphia, leaving a proper opening in the most depending part of it for the discharge of the blood or matter which might be collected in the cavity; and afterwards it was dressed in the usual way, lightly and almost superficially, with an anodyne poultice over all. The regimen enjoined him, with respect to diet, was only gruel, panado, and sage-tea, with barley water or thin gruel to drink; with an oily laxative draught and emollient anodyne clyster.

27th. Visiting him early the next morning, Mr. N. found the night had been spent in great restlessness and inquietude, notwithstanding the clyster had been thrown up according to the direction. He was exceedingly low; his skin felt still cold and clammy; his pulse weak and fluttering; he complained of frequent chills, and an oppressive tightness of his belly, though the wound had discharged considerably a thin, serous humour, which had wetted the bandage quite through. Nor was the tension of the abdomen, at this time, sufficient to account for this oppressive pain he complained of; from which Mr. N. concluded it to be spasmodic. The dressings were removed; and Mr. N. desired his apprentice to foment the part with an infusion of the emollient flowers for a full hour, and to

take particular care that the stupes were applied of a very moderate warmth; often having observed, that this manner of applying them, when an inflammation was either to be resolved or prevented, was more effectual than when the heat has been greater. This observation, on a little reflection, will be found agreeable to reason; for as great heat proves an astringent, on the contrary, a moderate and kindly warmth relaxes, and, by promoting a free perspiration of the parts to which it is applied, sooner effects the end proposed. The wound was dressed as before, with the addition of 2 oz. of the species pro cataplasmate de cymino to the poultice; and as the draughts he had taken had not produced any motion of the bowels, it was thought proper to inject a laxative clyster, as soon as it could be prepared: which in about half an hour occasioned a very copious discharge of fæces, together with a good deal of blood; some of it congealed into lumps, the rest fluid. This circumstance did not fail to alarm his apprehension of the imminent danger of the lad's situation, as it was no longer to be doubted, but that the bowels were wounded in some part of them; but what part still remained a matter of conjecture. When the clyster had done operating, he took an oily anodyne draught. In the evening the fomentation and dressings were again renewed, and directions given, that he should take one of the oily laxative draughts, as first prescribed, at 3 o'clock in the morning; and to repeat them regularly every 4th hour, till they had had their desired effect.

28th. He had got but little sleep in the night, though he had lain something quieter, with short, but interrupted slumbers intervening. His pulse, and all the other symptoms, were much in the same state as the day before, except a general soreness of the abdomen; of which, at this time, he made great complaint, and more particularly about the wounded part. The whole belly was full and tense; and when Mr. N. struck it with his finger, it returned an emphysematose sound. The discharge from the wound was increased; it had stained the bandage of a deep reddish-brown colour, and was of a disagreeable smell. The draughts he had taken had not yet moved him; therefore, Mr. N. desired they might be continued, according to the general direction; and that in case any stools should come off, to put them by separately for his inspection. By the time he made his evening visit, he had had 2 motions: in the first, there was a good deal of fluid blood; with the last but little, no more than just to give it a tinge. He was evidently relieved by the evacuation; was calmer and more composed; his pulse was rather more up, and his skin warmer. He said he found himself lithesomer, that he was not so tight, and thought he breathed with more freedom. When Mr. N. came to loosen the bandage, he was greatly surprised to find it daubed all over with the discharge; but as soon as the dressing was removed, there was no evidence wanting to assure him, that this discharge was in part fecal, not only from the colour and smell of it, but also from the

sharp pain it had occasioned in passing through the wound. Mr. N.'s hopes of his recovery now began to fail him; however he resolved to persevere, and act as though he was sure of success. After dressing, he was ordered to take the anodyne draught, and to begin again the manna draughts with oil early in the morning.

29th. Before Mr. N. came to visit him, he had had another motion; and the nurse informed him, that his night had been better than any of the preceding ones, he having slept, at different times, full 3 hours. His pulse was stronger, but remitting, and his skin inclining to perspire. The tongue was foul, and the water clear and pretty high coloured. In the stool which had come off this morning Mr. N. did not find any blood, or in any he had afterwards during the time of his confinement. The wound had discharged a great deal, and was more inflamed; and the edges looked thick and ill-natured, and were ready to separate from each other. The tension of the belly still kept up, though he did not perceive that it had at all increased. The opening draughts were continued, once in 6 hours only, through the course of this day, which kept him sufficiently open; and the anodyne was repeated at 10 o'clock this night.

30th. This morning things wore but a melancholy aspect. His night had been restless, and his head confused, and he talked sometimes incoherently; his pulse was increased, though exceedingly irregular, and the skin felt hot and dry; he was thirsty, and complained of a great tightness, particularly about the region of the stomach; his countenance was hollow, the eyes being sunk, with a deadness in them not easily to be expressed. The wound had discharged very much, and it was extremely offensive. The edges of it were inverted, much swollen, and separated from each other considerably more than the preceding day. He likewise complained of a sharp, burning pain, deep in the wound, but could not express precisely where. As soon as the wound was dressed, the anodyne clyster was administered; and Mr. N. desired he might have a small basin of the infusion of mint, with a knob of fine sugar, got ready for him as soon as possible, and that he would sip it down as warm as he could. At 2 o'clock this afternoon he was seized suddenly with a most violent vomiting, and brought up a large quantity of bile. This Mr. N. the more wondered at, as he had never made the least complaint of sickness, or nausea, from the time of his accident; for every thing he had taken had sat easy and well on his stomach. What he had brought up was of so dark a colour, that he imagined it was mixed with blood; but, on a careful examination of it, found he was mistaken. When the vomiting was over, the nurse gave him a little more of the mint infusion; and soon after he fell into a sound sleep, which continued more than an hour. In the evening he was hot and uneasy, complaining of thirst, and a pain in his head; his pulse was increased, and his skin felt dry. The wound had made a prodigious discharge,

which Mr. N. observed always to increase, in proportion as the bowels were more or less loosened by the medicines he was taking; and, from the violent efforts of the abdominal muscles in the time of his vomiting, most of the stitches in the wound were broken, so that one might plainly see into the cavity of the abdomen. After dressing the wound, 12 oz. of blood were taken from the arm, and the anodyne draught was given to him soon after.

Oct. 1st. Mr. N. learnt from the people about him, that for a few hours after he had taken the opiate, he lay composed; but soon after midnight he awoke in great hurry and confusion, complaining of his stomach and bowels, accompanied with convulsive twitchings of the tendons; and that about 5 o'clock this morning he brought up another large quantity of bile, which gave him great relief; for afterwards he lay perfectly easy, and got between 2 and 3 hours sleep. At 9 o'clock, when Mr. N. made his morning visit, he found him much refreshed, and without any kind of complaint. His pulse was full, but much steadier than it had been any time before, and his skin was open. The water he had made was turbid, though still high coloured. The wound indeed made but an indifferent appearance; the edges of it were very sloughy, particularly the tendons of the oblique muscle, and so far receded from each other as to make it necessary to divide the remaining stitches. The lower part of the wound, or that next to the ilium, was beginning to digest, and the inflammation and tension of the belly to abate. The opening draughts, made a little warmer, were continued, which kept the bowels constantly and gently open. In the evening his pulse was rather increased; and Mr. N. found that, some time in the afternoon, he had brought up a little more bile, though without any previous complaining. After dressing, he directed more blood to be drawn, and the opiate to be repeated.

2d. The nurse acquainted Mr. N. this morning, that her patient had had a very quiet night, and had slept many hours without intermission; that he had taken a sufficient quantity of nourishment, and that it had sat very well on his stomach. He found him cheerful, without any complaint, except that of hunger. His pulse was steady, his skin soft and open, his tongue getting cleaner, and his water beginning to break. The discharge this morning from the sore was exceedingly offensive; and when he had taken off the dressing, he was really astonished at the horrid appearance! The wound was burst open, in such a manner as to assume a circular form, and was rather more than 3 inches in its least diameter. In the base of this dreadful opening, there was nothing to be seen but the circumvolutions of the small guts; and how this amazing breach was to be restored, he could not easily conceive. Had any one taken a view of the wound at this time, who was unacquainted with the real progress of it, he must naturally have concluded, there had been a great loss of substance. The patient was dressed with thin pledgets of very fine unformed lint, moistened with the oil of

the flowers of the hypericum luke-warm, laid first on the exposed bowels; afterwards the cavity was filled up lightly with the same sort of lint dry; the edges were covered with a moderately warm digestive, and the whole secured with the uniting bandage; which bandage had been used from the very beginning, to prevent, as much as art could prevent, the impending mischief.

3d. Appearances this morning were very favourable; he had slept well most part of the night; his pulse was perfectly quiet, and his skin moderately open. The water was become better coloured, and had made a fair separation; so that from this time all signs of fever, inflammation, and pain its concomitant, entirely ceased; nor did there even arise any alarming, or even disagreeable symptom afterwards; but every thing went on in an easy, regular way. The wound digested kindly, and was constantly dressed twice a day, as the quantity, and indeed the quality of the discharge from it required. The opening medicines were repeated occasionally, and his nights secured by a few drops of the Theban tincture.

In a few days, the sloughs from the edges of the abdominal muscles separated, and left the sore so largely open, that he could easily discover whence the fæces made their exit, which was from the middle of that part of the colon that lies between the left kidney, to which it is attached, and the upper part of the sacrum, where it empties itself into, and forms the rectum. It was exceedingly satisfactory and pleasing to observe, from day to day, the progress nature made in renovating this formidable breach, and her means of accomplishing it; for, after a little time, the surface of the intestines looked florid, and began to pululate, throwing up small grains of flesh from every point. These granules, daily increasing, united with each other, and after filling up the intervals between the circumvolutions of the bowels, became one uniform surface: which surface meeting with that of the raw edges of the integuments, they both adhered together, and became one continued sore. As the wound incarnated, the fecal discharge lessened daily, and about the 22d or 23d day entirely ceased. Mr. N. now allowed him chicken broth, milk porridge, calves-feet jelly, &c. The wound was dressed once a day with dry lint only, and in 7 weeks it was completely healed.

XXIII. Observations on the Island of Minorca. By Mr. Alexander Small, Surgeon to the Train of Artillery at Minorca. p. 439.

The following conjectures, says Mr. S., may be a kind of addition to Dr. Cleghorn's account of this island. The new as well as the old town are built in a very dry situation, on a solid rock, on which there is not a drop of stagnating water, nor is there any near the surface of the earth; for the water the inhabitants have for use, is either rain water kept in cisterns, or water drawn out of

wells, from 20 to 60 or more feet in depth; nor are there any marshes near either town, or indeed in this part of the island. The castle of St. Philip stands, or rather is cut out of the solid rock, on a promontory, $\frac{2}{3}$ of which are washed by the sea, and is open to the sea winds from $\frac{2}{3}$ of the compass. As there is no tide, there is no slimy shore, which might send forth putrid vapours at low water; and if there were a tide, our shore is one continued rock, on which there is not any putrescent substance. Indeed the rocks are so free from filth, that after a strong wind has raised the seawater, and carried it into cavities hollowed in the rock by storms, it dries there into pure white salt.

During the hot weather in July, August, and September, our unhealthy season, the air is daily ventilated, either by general winds, which pass freely over the island, or by sea breezes. The air over the land being rarefied by the reflected rays of the sun, and by being in contact with the heated earth, necessarily makes room for the cooler and denser air in contact with the cooler seawater. Whence, in such a situation, shall we seek for the causes of tertianas, so called here, and so much dreaded during the hot months? Two causes seem to offer themselves; one very obvious, the other rather more remote. The southerly winds are much complained of here, as occasioning a general lassitude, and as bringing with them noxious effluvia from Africa; but whoever considers the distance between this island and Africa, will scarcely believe, that the air can carry with it so far any other quality than the warmth attending the season of the year. Gibraltar, nearer Africa, and more southerly than we are, is not subject to tertians, nor are some places even in this island. The causes therefore must be sought for on the spot. In a situation, such as above described, shade and a plentiful supply of fresh succulent culinary plants must be very desirable. On so dry a rock, an artificial supply of moisture must become necessary, especially in a country where there is seldom rain from May to October. It is not an easy matter to keep a due mean in the use of whatever experience shows to be necessary. If a little does good, we are apt to conclude that a great deal will do more good: thus, I think, it fares with us in regard to the use of water in our gardens. In order to have a garden, it is necessary here to have a draw-well. The drawing of water is the labour of an ass; and, as the labour is not hard, the beast is kept at it pretty constantly, and thus plenty of water is drawn up. As the water is hard, and is much colder than the temperature of the air, it is kept in cisterns for some time, exposed to the sun, till it acquires the temperature of the air, and thus becomes more friendly to vegetation than if used immediately on being drawn up. Having thus obtained plenty of water, they bestow it most copiously on their gardens. Suppose yourself landed at St. Philip's in this season of the year, on a dry, parched rock, and that you were told, that the rock was uniformly the same all the way to Mahon, a distance of 2 miles, and that you were

under a necessity of going to Mahon in the evening; would you expect to be serenaded on this rock with the croaking of frogs all the way you went? This is literally the case. The gardens on each side the road are so much watered, that the frogs, bred in the cisterns which contain the water, spread and enjoy themselves around, and frequently take up their abode in trees. This shows that even the trees abound much in watery juices, as the exhalations arising from them yield an atmosphere agreeable to the frogs. Where land is thus abundantly watered and closely planted with succulent vegetables, many parts of these vegetables, as well as the insects which feed on them, will be liable to putrify; and a putrid vapour may be thence exhaled in the evening especially, and during the night, when there seldom is wind to carry them off. Wherever the inhabitants can find a proper depth of mould, within a convenient distance of a market, so many sources of putrid exhalations are formed.

The 2d general cause of tertians was pointed out by Dr. Munro, physician of this island, an ingenious gentleman, and very observant of every thing relating to his profession. The rocks of this island consist chiefly of 2 kinds of stone; one so hard that scarcely any tool can touch it; and the other so soft, that it is easily cut into any form. It much resembles the Bath stone, and is called Cantoan stone. The first is impervious to any fluid; but the other sucks up or is penetrated by moisture, like filtering stones. When houses are built on the hard rock, all within the walls is levelled; and on that floor the poorer inhabitants live. As this stone takes a greater degree of cold than substances less solid, and does not so soon come to the temperature of the air; it consequently cools, and attracts to it the moisture in the air, and retains it long on its surface. In order to avoid the damp cold feel, if the inhabitants can afford to buy a mat, they cover the floor with it; under which the wet remaining, induces a degree of putrefaction, which renders the houses more unhealthy, and reduces the inhabitants to a state ready to be affected by any distemper, especially by the tertian, which spreads by contagion. As the moisture remaining on this stone is but temporary, provided there are drains to carry the water off, its bad effects are easily prevented by keeping a fire burning, or by laying the ground floor with terrace, or with deal boards.

When houses are built on the soft Cantoan stone, the rain that falls without soaks through it; and if there are no means of carrying it off, it remains in the stone, becomes putrid, gradually exhales, and thus becomes highly prejudicial to health. Several instances might be given of families dying in consequence of such putrid moisture; but one may suffice, which became an object of general observation. At a little distance northward of the line-wall, a lofty building was erected for a house of entertainment. The people who inhabited it became very unhealthy; and in a few years so much so, that 2 or 3 whole families died in it.

This house it seems is built on Cantoan stone, the hollows filled with Cantoan rubbish, and is surrounded by gardens continually watered, some of which are higher than the floor of this building; by which means the stone became the receptacle of the waste water. In order to remedy this inconveniency, the floor was taken up, and a stench arose which the workmen could scarcely bear, and changed the colour of every metallic substance about them. People were impressed with so strong a prejudice against the house, that it remains uninhabited and a useless building. The same has happened in other dwelling-houses; in which the same stench, and other indications of putrefaction, were met with, as in the former case.

XXVI. Of the Tides in the South Seas. By Captain James Cook, F.R.S.
p. 447.

The following are observations on the tides in Endeavour river, on the east coast of New Holland, in latitude $15^{\circ} 26'$ s. About 11 o'clock in the evening of the 10th of June, 1770, when standing off shore, the ship suddenly struck, and stuck fast on a reef of coral rocks, about 6 leagues from the land. At this time, it was judged about high water, and that the tides were taking off, or decreasing, as it was 3 days past the full moon; 2 circumstances by no means favourable. As the efforts to heave her off, before the tide fell, proved ineffectual, they began to lighten her, by throwing overboard the guns, ballast, &c. in hopes of floating her the next high water; but to their great surprise, the tide did not rise high enough to accomplish this by near 2 feet. They had now no hopes but from the tide at midnight; and these only founded on a notion, very general indeed among seamen, but not confirmed by any thing which had yet fallen under Captain C.'s observation, that the night-tide rises higher than the day-tide. They prepared however for the event, which exceeded their most sanguine expectations; for about 20 minutes after 10 o'clock in the evening, which was a full hour before high water, the ship floated. At this time the heads of rocks, which on the preceding tide were at least a foot above water, were wholly covered. Captain C. was fully satisfied with the truth of the remark, after getting into the river, where they remained from the 17th of June till the 4th of August, repairing the damage the ship had received. As this was to be done with the assistance of the tides, it led him to make the following observations, which on any other less important occasion might have escaped notice.

The times of high water on the full and change days are about a quarter after 9; the evening-tide, at the height of the spring, rises 9 feet perpendicular, the morning-tide scarcely 7: and the low water preceding the highest or evening-tide, falls or recedes considerably lower than the one preceding the morning-

tide. This difference in the rise and fall of the tide was uniformly the same on each of the 3 springs which happened while they lay in the place, and was apparent for about 6 or 7 days; that is, for about 3 days before and after the full or change of the moon. During the neap, the tide was very inconsiderable, and if there was any difference between the rise of the tide in the day and in the night, it was not observed; but to the best of Captain C.'s recollection none was perceptible. Excepting 2 or 3 mornings, when they had a land-breeze for a few hours, they had the winds from no other direction than s. e., which is the same as this part of the coast, and from which quarter he judged the flood-tide came. The wind, for the most part, blew a brisk gale, and rather stronger during the day than the night. How far this last circumstance might affect the evening-tide, he pretends not to determine; nor can he assign any other cause for this difference in the rise and fall of the tide, and therefore must leave it to those who are better versed in this subject.

XXVII. An Experimental Examination of the Quantity and Proportion of Mechanic Power necessary to be employed in giving Different Degrees of Velocity to Heavy Bodies from a State of Rest. By Mr. John Smeaton, F. R. S. p. 450.

About the year 1686 Sir Isaac Newton first published his Principia, and conformably to the language of mathematicians of those times defined, that "the quantity of motion is the measure of the same, arising from the velocity and quantity of matter conjointly." Very soon after this publication, the truth or propriety of this definition was disputed by certain philosophers, who contended, that the measure of the quantity of motion should be estimated by taking the quantity of matter and the square of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increases of velocity are acquired by given bodies, when unresisted by a medium; thus gravity causes a body, in obeying its impulse during one second of time, to acquire a velocity which would carry it uniformly forward, without an additional impulse, at the rate of 32 ft. 2 in. per second; and if gravity be suffered to act on it for 2 seconds, it will have in that time acquired a velocity that would carry it, at a uniform rate, just double of the former; that is, at the rate of 64 ft. 4 in. per second. Now, if in consequence of this equal increase of velocity, in an equal increase of time, by the continuance of the same impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the same impelling power for a double time; this will coincide with Sir Isaac Newton's definition abovementioned; whereas, in trying experiments on the total effects of bodies in motion, it appears, that when a body is put in motion,

by whatever cause, the impression it will make on a uniformly resisting medium, or on uniformly yielding substances, will be as the mass of matter of the moving body, multiplied by the square of its velocity: the question, therefore, properly is, whether those terms, the quantity of motion, the momenta of bodies in motion, or forces of bodies in motion, which have generally been esteemed synonymous, are with the most propriety of language to be esteemed equal, double, or triple, when they have been generated by an equable impulse, acting for an equal, double, or triple time; or that it should be measured by the effects being equal, double, or triple, in overcoming resistances before a body in motion can be stopped? For, according as those terms are understood in this or that way, it will necessarily follow, that the momenta of equal bodies will be as the velocities, or as the squares of the velocities respectively; it being certain, that, which ever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumstances that attend the application of it, the same conclusion, in point of computation, will result. I should not therefore have thought it worth while to trouble the society on this subject, had I not found, that not only myself and other practical artists, but also some of the most approved writers, had been liable to fall into errors, in applying these doctrines to practical mechanics, by sometimes forgetting or neglecting the due regard which ought to be had to these collateral circumstances. Some of these errors are not only very considerable in themselves, but also of great consequence to the public, as they tend greatly to mislead the practical artist in works that daily occur, and which often require very great sums of money in their execution. I shall mention the following instances.

Desaguliers, in his 2d volume of Experimental Philosophy, treating on the question concerning the forces of bodies in motion, after taking much pains to show that the dispute, which had then subsisted 50 years, was a dispute about the meaning of words; and that the same conclusion will be brought out, when things are rightly understood, either on the old or new opinion, as he distinguishes them; among other things, tells us, that the old and new opinion may be easily reconciled in this instance: that the wheel of an undershot water-mill is capable of doing quadruple work when the velocity of the water is doubled, instead of double work only; "because (the adjutage being the same), says he, we find, that as the water's velocity is double, there are twice the number of particles of water that issue out, and therefore the ladle-board is struck by twice the matter, which matter moving with twice the velocity that it had in the first case, the whole effect must be quadruple, though the instantaneous stroke of each particle is increased only in a simple proportion of the velocity." See vol. 2, Annotations on lecture 6th, p. 92.

Again, in the same volume, lecture 12th, p. 424, referring to what went

before, he tells us, "The knowledge of the foregoing particulars is absolutely necessary for setting an undershot wheel to work; but the advantage to be reaped from it would be still guess-work, and we should be still at a loss to find out the utmost it can perform, if we had not an ingenious proposition of that excellent mechanic M. Parent, of the Royal Academy of Sciences, who has given us a maximum in this case, by showing, that an undershot wheel can do the most work, when its velocity is equal to the 3d part of the velocity of the water that drives it, &c. because then $\frac{2}{3}$ of the water is employed in driving the wheel with a force proportionable to the square of its velocity. If we multiply the surface of the adjutage or opening by the height of the water, we shall have the column of water that moves the wheel. The wheel thus moved will sustain on the opposite side only $\frac{4}{9}$ of that weight, which will keep it in equilibrio; but what it can move with the velocity it goes with, will be but $\frac{1}{3}$ of that weight of equilibrio; that is, $\frac{4}{27}$ of the weight of the first column, &c.—This is the utmost that can be expected."

The same conclusion is likewise adopted by Maclaurin, in art. 907, p. 728, of his Fluxions, where, giving the fluxionary deduction of M. Parent's proposition, he says, "that if A represents the weight which would balance the force of the stream, when its velocity is a; and v represents the velocity of that part of the engine, which it strikes when the motion of the machine is uniform, &c.—the machine will have the greatest effect when v is equal to $\frac{1}{3}$ a; that is, if the weight that is raised by the engine be less than the weight which would balance the power, in the proportion of 4 to 9, and the momentum of the weight is $\frac{4}{27}$ Aa."

Finding that these conclusions were far from the truth, and seeing, from many other circumstances, that the practical theory of making water and wind-mills was but very imperfectly delivered by any author I had then an opportunity of consulting;* in the year 1751 I began a course of experiments on this

* Belidor, Architecture Hydraulique, greatly prefers the application of water to an undershot mill, instead of an overshot; and attempts to demonstrate, that water applied undershot will do 6 times more execution than the same applied overshot. See vol. 1, p. 286. While Desaguliers, endeavouring to invalidate what had been advanced by Belidor, and greatly preferring an overshot to an undershot, says, Annotat. on lecture 12, vol. 2, p. 532, that from his own experience, "a well made overshot mill ground as much corn in the same time with 10 times less water;" so that between Belidor and Desaguliers here is a difference of no less than 60 to 1.

Again, Belidor, vol. 2, p. 72, says, that the centre of gravity of each sail of a windmill should travel in its own circle with one-third of the velocity of the wind; so that, taking the distance of this centre of gravity from the centre of motion at 20 feet, as he states it p. 38, art. 849, the circumference will be exceeding 126 feet English measure: a wind therefore, to make the mill go 20 turns per minute, which they frequently do with a fresh wind, and all their cloth spread, would require the wind to move above 80 miles an hour; a velocity perhaps hardly equalled in the greatest storms we experience in this climate.—Orig.

subject. These experiments, with the conclusions drawn from them, have already been communicated to this society, who printed them in vol. 51 of their Transactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir Godfrey Copley, from the hands of our very worthy president the late earl of Macclesfield. Those experiments and conclusions stand uncontroverted, so far as I know, to this day; and having since that time been concerned in directing the construction of a great number of mills, which were all executed on the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation: and the agreement I have always found between these two, appears to me fully to establish the truth of the principles on which they were constructed, when applied to great works, as well as on a smaller scale in models.

Respecting the explanatory deduction of Desaguliers, in the first example abovementioned, which indeed I have found to be the commonly received doctrine among theoretical mechanics, it is shown, Phil. Trans., vol. 51, p. 120, 121, and 123, part 1, maxim 4, that, where the velocity of water is double, the adjutage or aperture of the sluice remaining the same, the effect is 8 times; that is, not as the square but as the cube of the velocity; and the same is investigated concerning the power of the wind arising from difference of velocity, p. 156, being part 3, maxim 4.

The conclusion in the 2d example abovementioned, adopted both by Desaguliers and Maclaurin, is not less wide of the truth than the foregoing; for if that conclusion were true, only $\frac{4}{27}$ ths of the water expended could be raised back again to the height of the reservoir from which it had descended, exclusively of all kinds of friction, &c. which would make the actual quantity raised back again still less; that is, less than $\frac{1}{7}$ of the whole; whereas it appears, from table 1, p. 115 of the said volume, that in some of the experiments there related, even on the small scale on which they were tried, the work done was equivalent to the raising back again about $\frac{1}{4}$ of the water expended; and in large works the effect is still greater, approaching towards half, which seems to be the limit for the undershot mills, as the whole would be the limit for the overshot mills, if it were possible to set aside all friction, resistance from the air, &c.

The velocity also of the wheel, which, according to M. Parent's determination adopted by Desaguliers and Maclaurin, ought to be no more than $\frac{1}{3}$ of that of the water, varies at the maximum, in the abovementioned experiments of table 1, between $\frac{1}{3}$ and $\frac{1}{2}$; but in all the cases there related, in which the most work is performed in proportion to the water expended, and which approach the nearest to the circumstances of great works, when properly executed, the maximum lies much nearer to $\frac{1}{2}$ than $\frac{1}{3}$; one half seeming to be the true maximum,

if nothing were lost by the resistance of the air, the scattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminish the effect more, at what would be the maximum if these did not take place, than they do when the motion is a little slower.

Finding these matters, as well as others, to come out in the experiments, so very different from the opinions and calculations of authors of the first reputation, who, reasoning according to the Newtonian definition, must have been led into these errors from a want of attending to the proper collateral circumstances; I thought it very material, especially for the practical artist, that he should make use of a kind of reasoning in which he should not be so liable to mistakes; in order therefore to make this matter perfectly clear to myself, and possibly so to others, I resolved to try a set of experiments from which it might be inferred, what proportion or quantity of mechanical power is expended in giving the same body different degrees of velocity. This scheme was put in execution in the year 1759, and the experiments were then shown to several friends, particularly my very worthy and ingenious friend Mr. William Russell.

In my experimental inquiry concerning the powers of water and wind before referred to, I have defined what I meant by power, as applied to practical mechanics, that is, what I now call mechanical power; which, in terms synonymous to those there used, may be said to be measured by multiplying the weight of the body into the perpendicular height from which it can descend; thus the same weight, descending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight descending from the same height is also a double power, because it likewise is capable of producing a double effect; and a given body, descending through a given perpendicular height, is the same power as a double body descending through half that perpendicular; for, by the intervention of proper levers, they will counterbalance each other, conformably to the known laws of mechanics, which have never been controverted. It must however be always understood, that the descending body, when acting as a measure of power, is supposed to descend slowly, like the weight of a clock or a jack; for, if quickly descending, it is sensibly compounded with another law, viz. the law of acceleration by gravity.

Description of the Machine, pl. 1. fig. 1.—AB is the base of the machine placed on a table; AC is a pillar or standard; CD is an arm on the extremity of which is fixed a plate fg, here seen edge-ways, through which is a hole for receiving a small steel pivot e, fixed in the top of the upright axis EB; the lower end of which axis finishes in a conical steel point, resting on a small cup of hard steel polished at B.—HI is a cylinder of white fir, which passing through fixes in a perforation in the axis; and on the 2 arms thus formed, capable of sliding, are K, L, two cylindric weights of lead of equal size, which are capable of being fixed on any part of the cylindric arms, from the axis to their extremities, by means of 2 thin wedges of wood. The 2 weights therefore being at equal distances

from the centre, and the axis perpendicular, the whole will be balanced on the point at *B*, and moveable on it by an impelling power, with very little friction. On the upper part of the axis are formed 2 cylindrical barrels, *M*, *N*, of which *M* is double the diameter of *N*; and they have a little pin stuck into one side of each at *o*, *p*.

Q is a piece capable of sliding higher or lower, as occasion requires; and carries *R*, a light pulley of about 3 inches diameter, hung on a steel axis, and moveable on 2 small pivots. The plane of the pulley however is not directed to the middle of the upright axis, but a little on one side, so as to point (at a mean) between the surface of the larger barrel and the less. *S* is a light scale for receiving weights, and hangs by a small twine, cord, or line, that passes the pulley, and terminates either on the larger barrel or the less, as may be required; the sliding-piece *Q* being moved higher or lower for each, that the line, in passing from the pulley to the barrel may be nearly horizontal. The end of the line farthest from the scale is terminated by a small loop, which hangs on the pin *o*, or the pin *p*, according as the larger or the lesser barrel is to be used.

Now, having wound up a certain number of turns of the line on the barrel, and having placed a weight in the scale *s*, it is obvious, that it will cause the axis to turn round, and give motion to its arms, and to the weights of lead placed on them, which are the heavy bodies to be put in motion by the impulse of the weight in the scale; and when the line is wound off to the pin, the loop slips off, and the scale then falling down, the weight will cease to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except so far as it must be gradually lessened by the friction of the machine and resistance of the air, which being small, the bodies will revolve some time before their velocity is apparently diminished.

Measures of some Parts of the Machine.

	Inches.
Diameter of the cylinders of lead, or the heavy bodies.....	2.57
Length of ditto	1.56
Diameter of the hole in them72
Weight of each cylinder 3lbs. Avoirdupois.	
Greater distance of the middle of each body from the centre of the axis.....	8.25
The smaller distance of ditto	3.92
10 turns of the smaller barrel, or 5 of the larger raises the scale	25.25

When the bodies are at the smaller distance from the axis of rotation, they are then in effect at half the greater distance from that axis; for, since the axis itself, and the cylindric arms of wood, keep an unvaried distance from the centre of rotation; the bodies themselves must be moved nearer than half their former distance, in order that, compounded with the unvariable parts, they may be virtually at the half distance. In order to find this half distance nearly, I put in an arm of the same wood, that only went through the axis, without extending in the opposite direction; one of the bodies being put on the end of this arm, at the distance of 8.25 inches, the whole machine was inclined till the body and arm became a kind of pendulum, and vibrated at the rate of 92 times per minute; and as a pendulum of the half length vibrates quicker in the proportion of $\sqrt{1}$ to $\sqrt{2}$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the same inclination of the machine, the weight was moved on the arm till it made 130 vibrations per minute; which was found to be, when it was at 3.92 inches distance from the centre as above stated, which is about $\frac{2}{10}$ ths of an inch nearer than the half distance. The double arm was then put in, and marked accordingly, and the bodies being mounted on it, the whole was adjusted

ready for use; and with it were tried the following experiments, each of which was repeated so many times as to be fully satisfactory.

Table of Experiments.

No	Ounces Avoirdupois in the Scale.	Barrel used, M the larger, N the smaller.	The Arms, w the whole, H the half length.	Number of Turns of the Line wound on the Barrel.	Time of the Descent of the Weight in the Scale.	Time in making 20 Revolutions with equable Motion.
1	8	M	w	5	$14\frac{1}{2}$ sec.	29 sec.
2	8	N	w	10	$28\frac{1}{2}$	$29\frac{1}{4}$
3	8	N	w	$2\frac{1}{2}$	$14\frac{1}{2}$	$58\frac{1}{2}$
4	32	M	w	5	7	14
5	32	N	w	10	14	$14\frac{3}{4}$
6	32	N	w	$2\frac{1}{2}$	7	$28\frac{3}{4}$
7	8	M	H	5	7	$14\frac{3}{4}$
8	8	N	H	10	14	15
9	8	N	H	$2\frac{1}{2}$	7	$30\frac{1}{4}$
1	2	3	4	5	6	7

The $58\frac{1}{2}$ in number 3, column 7, was determined in fact from $29\frac{1}{4}$, being the time of making 10 equable revolutions, after the weight was dropped off, in order to prevent the sensible retardation that might take place, and affect the observation, if continued for 20 revolutions made so slowly.

I have already defined what I mean by mechanic power; but, before proceeding further, it will be necessary also to define the following terms:

Impulse or impulsion, impulsive force or power, impelling force or power, by all which, I understand the uniform endeavour that one body exerts on another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be measured either by its being a weight of itself, or by being counter-balanced by a weight. It may also act either immediately on the body to be moved, so that if motion is the consequence, they move with the same velocity; and that, either by a simple contact, or by being drawn as by a cord, or pushed as by a staff: or it may act by the intervention of a lever or other mechanic instrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers must be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a standard length of lever, which is the method taken in the subsequent reasoning on the preceding experiments. An impelling power therefore, consisting of a double weight, or requiring a double weight to counter-balance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intensity.

Observations and Deductions from the preceding Experiments.—1st. By the 1st experiment it appears, that the mechanic power employed, consisting of 8

ounces in the scale, deliberately descending, by 5 turns of the larger barrel, through a perpendicular space $25\frac{1}{4}$ inches, will represent the quantity of mechanic power which causes the two heavy bodies, from a state of rest, to acquire a velocity, such as to carry them equably through 20 circumferences of their circle of revolution in the space of 29^s ; and that the time in which the mechanic power produced this effect was $14\frac{1}{4}^s$, as appears by the 6th column. And this mechanic power we shall express by the number 202, the product of the number of ounces in the scale multiplied by the inches in its perpendicular descent, for $8 \times 25\frac{1}{4} = 202$.

2d. By the 2d experiment, as 10 turns of the smaller barrel are equal to the same perpendicular height as 5 turns of the larger, it follows that the same mechanic power, viz. 202, acting on the same heavy bodies to accelerate them, produces the very same effect in generating motion in the bodies as it did before, viz. 20 revolutions in $29\frac{1}{4}^s$, the small difference of $\frac{1}{4}$ of a second being no more than may reasonably be attributed to the unavoidable errors arising from friction of the machine, want of perfect accuracy in its measures, resistance of the air, and imperfections in the observations themselves, which must not only be allowed for in this, but the rest; but as the impelling power is acting here on a lever of but half the length, and consequently but half the intensity, when referred to the bodies to be moved, it takes just double the time to generate the same velocity.

Deduction.—Hence it appears, that the same mechanic power is capable of producing the same velocity in a given body, whether it is applied so as to produce it in a greater or a less time; but that the time taken to produce a given velocity, by a uniformly continued action, is in a simple inverse proportion of the intensity of the impulsive power.

3dly. The 3d experiment being made with $2\frac{1}{2}$ turns of the less barrel, the same weight in the scale of 8 ounces descending only one quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, viz. $50\frac{1}{2}$; but as only one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in $58\frac{1}{2}''$; that is, nearly 10 revolutions in $29''$; we may conclude, in this instance, that the mechanic power, employed in producing motion, is as the square of the velocity produced in the same body; and that the velocity produced is as the time that an impelling power, of the same intensity, continues to act on it, as appears by the near agreement of numbers 2 and 3, column 6th.—4thly. In the 4th experiment, the apparatus is the same as the first, only here the weight in the scale is 32 ounces; that is, the impelling power is the quadruple of the 1st, and here a double velocity is given to the bodies; for they make 20 revolutions in 14^s , which is a small matter less than

half the time taken up in making 20 revolutions in the 1st experiment. It also appears, that the velocity acquired is simply as the impelling power compounded with the time of its action; for a quadruple impulsion acting for 7^s, instead of 14^s, generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being 4 times greater than the first, it holds here also, that the mechanic power, to be necessarily employed, is as the square of the velocity to be generated; that is, in the same proportion as turned out in the 3d experiment, where the mechanic power employed was only a 4th part of the first.—5thly. The 5th and 6th experiments were made with a mechanic power 4 times greater than those employed in numbers 2 and 3 respectively; and since the same deductions result from these as from numbers 2 and 3, they are additional confirmations of the conclusions drawn from them and from the last article.—6thly. In the 7th experiment, the disposition of the apparatus is the same as number 1, only here the bodies are placed on the arms at the half length; whence it appears, that the same mechanic power still produces the same velocity in the same bodies; for though 20 revolutions were performed in $14\frac{3}{4}$ ^s (see column 7,) which is nearly half the time that 20 revolutions were performed in the first experiment; yet, since the circles in which the bodies revolved in the 7th are only of half the circumference as those of number 1, it is obvious, that the absolute velocity acquired by the moving bodies in the two cases is equal. But, by column 6th, the time in which it was generated is only half; yet this will coincide with the former conclusions, if the intensity of the impelling power is compounded with it; for though the barrel was the same with the same number of turns as in number 1, and therefore the lever the same, by which the impelling power acted, yet as the bodies, on which this lever was to act, were placed on a lever of only half the length from the centre, the impelling power, acting by the 1st lever, would act on the 2d with double the intensity, according to the known laws of mechanics; that is, it would require a double weight opposing the bodies, to prevent their moving, in order to balance it. An impulsive power therefore of double the intensity, acting for half the time, produces the same effect in generating motion, as an impulsive power of half the intensity, acting for the whole time.—7thly. The 8th and 9th experiments afford the same deductions and confirmations relative to the 7th experiment, that the 5th and 6th do respecting the 4th, and that the 2d and 3d do respecting the 1st; and from the near agreement of the whole, when the necessary allowances before-mentioned are made, together with some small inequality arising from the mechanical power lost by the difference of the motion given by gravity to the weight in the scale: I say, from these agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may safely

conclude, that this is the universal law of nature, respecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power necessary to be employed to produce or generate different velocities, the bodies being supposed equal in their quantity of matter; that the mechanic powers to be expended are as the squares of the velocities to be generated, and vice versâ; and that the simple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and vice versâ.

We shall perhaps form a still clearer conception of the relation between velocities produced, and the quantities of mechanic power required to produce them; together with the collateral circumstances attending, by which these propositions, seemingly 2, are reconciled and united, by stating the following popular elucidation, which indeed was the original idea that occurred to me on considering this subject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppose then a large iron ball of 10 feet diameter, turned truly spherical, and set on an extended plane of the same metal, and truly level. Now, if a man began to push at it, he will find it very resisting to motion at first; but, by continuing the impulse, he will gradually get it into motion, and having nothing to resist it but the air, he will, by continuing his efforts, at length get it to roll almost as fast as he can run. Suppose now, in the first minute he gets it rolled through a space of one yard; by this motion, proceeding from rest, similar to what happens to falling bodies, it would continue to roll forward at the rate of 2 yards per minute, without further help; but supposing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a space of 2 yards more, in addition to the former, that is, at the rate of 4 yards per minute; and at the end of the 3d minute, he again added an equal increase of velocity, and made it proceed at the rate of 6 yards per minute; and so on, increasing its velocity at the rate of 2 yards in every minute. The man therefore in the space of every minute exerts an equal impulse on the ball, and generates an equal increase of movement, correspondent to the definition of Sir Isaac Newton. But let us see what happens besides; the man, in the first minute, has moved but one yard from where he set out; but he must in the 2d minute move 2 yards more, in order to keep up with the ball; and as he exerted an impulse on it, so as at the 2d minute to have given it an additional velocity of the 2 yards, he must also in this time have gradually changed its velocity from the rate of 2 yards per minute to that of 4, and the space that he will of consequence have actually been obliged to go through in the 2d minute, will be according to the mean of the extremes of velocity at the beginning and end of it, that is, 3 yards in the 2d minute; so that being 1 yard from his original place at the beginning of the

2d minute, at the end of it he will have moved the sum of the journies of the 1st and 2d minute, that is, in the whole 4 yards from its original place. As he has now generated a velocity in the ball of 4 yards per minute, in the 3d minute he must travel 4 yards to keep up with the ball, and one more in generating the equal increment of velocity; so that in the 3d minute, he must travel 5 yards to keep up the same impelling power on the ball that he did in the 1st minute in travelling 1, so that these 5 yards in the 3d minute, added to the 4 yards that he had travelled in the 2 preceding minutes, sets him at the end of the 3d minute 9 yards from where he set out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of 6 yards per minute, as before stated. We may now leave the further pursuit of these proportions, and see how the account stands. He generated a velocity of 2 yards per minute in the 1st minute, the square of which is 4, when he had moved but 1 yard from his place; and he had generated a velocity of 6 yards per minute, the square of which is 36, at the end of the 3d minute, when he had travelled 9 yards from his place. Now, since the square of the velocity, generated at the end of the 1st minute, is to that of the velocity generated at the end of the 3d minute, as 4 to 36, that is, as 1 to 9; and since the spaces moved through by the man to communicate these velocities, are also as 1 to 9, it follows, that the spaces through which the man must travel, in order to generate these velocities respectively, preserving the impelling power perfectly equal, must be as the squares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted on the man equally resisting, this would be the measure of what the man has done in order to give motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that must of necessity be employed in giving different degrees of velocity to the same body, must be as the square of that velocity; and if the converse of this proposition did not hold, viz. that if a body in motion, in being stopped, would not produce a mechanical effect equal or proportional to the square of its velocity, or to the mechanical power employed in producing it, the effect would not correspond with its producing cause.

Thus the consequences of generating motion on a level plane, exactly correspond with the generating of motion by gravity; viz. that though in 2 seconds of time the equal impulsive power of gravity gives twice the velocity to a body that it does in 1 second, yet this collateral circumstance attends it, that at the end of the double time, in consequence of the velocity acquired in the first half, the body has fallen from where it set forward 4 times the perpendicular; and, therefore, though the velocity is only doubled, yet 4 times the mechanical power

has been consumed in producing it, as 4 times the mechanical power must be expended in bringing up the fallen body to its first place.

This then appears to be the foundation, not only of the disputes that have arisen, but of the mistakes that have been made, in the application of the different definitions of quantity of motion, that while those who have adhered to the definition of Sir Isaac Newton, have complained of their adversaries, in not considering the time in which effects are produced, they themselves have not always taken into the account the space that the impelling power is obliged to travel through, in producing the different degrees of velocity. It seems therefore that, without taking in the collateral circumstances both of time and space, the terms, quantity of motion, momentum, and force of bodies in motion, are absolutely indefinite; and that they cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, viz. mechanic power.

From the whole of what has been investigated, it therefore appears, that time, properly speaking, has nothing to do with the production of mechanical effects, otherwise than as, by equally flowing, it becomes a common measure; so that, whatever mechanical effect is found to be produced in a given time, the uniform continuance of the action of the same mechanical power will, in a double time, produce 2 such effects, or twice that effect. A mechanical power therefore, properly speaking, is measured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a less time; thus, having treasured up 1000 tuns of water, which I can let out on the overshot wheel of a mill, and descending through a perpendicular of 20 feet, this power applied to proper mechanic instruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that, at a certain rate of expending it, it will grind this corn in an hour. But suppose the mill equally adapted to produce a proportionable effect, by the application of a greater impulsive power as with a less, then, if I let out the water twice as fast on the wheel, it will grind the corn twice as fast, and both the water will be expended and the corn ground in half an hour. Here the same mechanical effect is produced; viz. the grinding a given quantity of corn, by the same mechanical power, viz. 1000 tuns of water descending through a given perpendicular of 20 feet, and yet this effect is in one case produced in half the time of the other. What time therefore has to do in the business is this: let the rate of doing the business, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the same rate, twice the effect will be produced in twice the time, on supposition that I have a supply of mechanic power to go on with. Thus 1000 tuns of water, descending through 20 feet of perpendicular, being, as has been shown, a given mechanic power,

let me expend at what rate I will, if when this is expended, I must wait another hour before it be renewed, by the natural flow of a river, or otherwise, I can then only expend 12 such quantities of power in 24 hours; but if, while I am expending 1000 tuns in 1 hour, the stream renews me the same quantity, then I can expend 24 such quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common measure; but the quantity of mechanic power arising from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.*

XXVIII. A New and General Method of finding Simple and Quickly-converging Series; by which the Proportion of the Diameter of a Circle to its Circumference may easily be computed to a great Number of Places of Figures. By Charles Hutton, Esq. F. R. S. p. 476.

In a late examination of the methods of Mr. Machin and others, for computing the proportion of the diameter of a circle to its circumference, I discovered the method explained in this paper. This method is very general, and discovers many series which are very fit for the abovementioned purpose. The advantage of this method is primarily owing to the simplicity of the series by which an arc is found from its tangent. For if t denote the tangent of an arc a , the radius being 1, then it is well known, that the arc a will be equal to the infinite series, $t - \frac{1}{3} t^3 + \frac{1}{5} t^5 - \frac{1}{7} t^7 + \frac{1}{9} t^9 - \&c.$ where the form is as simple as can be desired. And it is evident that nothing further is required, than to contrive matters so as that the value of the quantity t , in this series, may be both a small and a very simple number. Small, that the series may be made to converge sufficiently fast; and simple, that the several powers of t may be raised by easy multiplications, or easy divisions.

Since the first discovery of the above series, many have used it, and that after

* In this paper, Mr. Smeaton does not seem clearly to distinguish between what he calls Mechanical Power, and the Newtonian term Momentum, or Quantity of Motion. These two powers are, from their very definition, as well as from their nature, of different kinds. The one is measured or estimated by its momentary or instantaneous action; the other by its action during some certain time. The one, by its definition, is in the compound ratio of the mass of a body and its velocity, or as the product of the body and its velocity, and therefore simply as the velocity in a given body. Whereas the other, by its definition, is estimated by the mass or weight compounded with the space it has fallen or described in acquiring its velocity: and since, as is well known, the space fallen by a body, is as the square of the velocity acquired; it follows that this force must needs be as the square of the velocity in a given body. The Newtonian momentum or force, therefore, and Mr. Smeaton's mechanical force, are two things that are quite different in their measure, and in their mode of action: though both may produce true results when applied to their proper objects.

different methods, for determining the length of the circumference to a great number of figures. Among these were, Dr. Halley, Mr. Abraham Sharp, Mr. Machin, and others, of our own country; and M. De Lagny, M. Euler, &c. abroad. Dr. Halley used the arc of 30° , or $\frac{1}{12}$ th of the circumference, the tangent of which being $= \sqrt{\frac{1}{3}}$, by substituting $\sqrt{\frac{1}{3}}$ for t in the above series, and multiplying by 6, the semicircumference is $= 6\sqrt{\frac{1}{3}} \times$

$(1 - \frac{1}{3.3} + \frac{1}{5.3^2} - \frac{1}{7.3^3} + \frac{1}{9.3^4} - \&c.)$; which series is, to be sure, very simple; but its rate of converging is not very great, on which account a great many terms must be used to compute the circumference to many places of figures. By this very series however, the industrious Mr. Sharp computed the circumference to 72 places of figures; Mr. Machin extended it to 100; and M. De Lagny, still by the same series, continued it to 128 places of figures. But though this series, from the 12th part of the circumference, does not converge very quickly, it is perhaps the best aliquot part of the circumference which can be used for this purpose; for when smaller arcs, which are exact aliquot parts, are used, their tangents, though smaller, are so much more complex, as to render them, on the whole, more operose in the application; this will easily appear, by inspecting some instances, that have been given in the introductions to logarithmic tables. One of these methods is from the arc of 18° , the tangent of which is $\sqrt{(1 - 2\sqrt{\frac{1}{5}})}$; another is from the arc of $22\frac{1}{4}^\circ$, the tangent of which is $\sqrt{2} - 1$; and a third is from the arc of 15° , the tangent of which is $2 - \sqrt{3}$. All of which are evidently too complex to afford an easy application to the general series.

In order to a still further improvement of the method by the above general series, Mr. Machin, by a very singular and excellent contrivance, has greatly reduced the labour naturally attending it. His method is explained in Mr. Maseres's Appendix to his Dissertation on the Use of the Negative Sign in Algebra; and I have given an analysis of it, or a conjecture concerning the manner in which it is probable Mr. Machin discovered it, in my Treatise on Mensuration; which, I believe, are the only two books in which that method has been explained, as I never had seen it explained by any, till I met with Mr. Maseres's book abovementioned on the Use of the Negative Sign. For though the series discovered by that method were published by Mr. Jones, in his Synopsis Palmariorum Matheseos, which was printed in the year 1706, he has given them merely by themselves, without the least hint of the manner in which they were obtained. The result shows, that the proportion of the diameter to the circumference, is equal to that of 1 to quadruple the sum of the two series,

$$\frac{4}{5} \times (1 - \frac{1}{3.5^2} + \frac{1}{5.5^4} - \frac{1}{7.5^6} + \frac{1}{9.5^8} - \&c.) \text{ and } \frac{1}{239} \times (1 - \frac{1}{3.239^2} + \frac{1}{5.239^4} - \frac{1}{7.239^6} + \frac{1}{9.239^8} - \&c.)$$

The slower of which converges almost thrice as fast as Dr. Halley's, raised from the tangent of 30° . The latter of these two series converges still a great deal quicker; but then the large incomposite number 239, by the reciproca ls o the powers of which the series converges, occasions such long, tedious divisions, as to counterbalance its quickness of convergency; so that the former series is summed with rather more ease than the latter, to the same number of places of figures. Mr. Jones, in his Synopsis, mentions other series' besides this, which he had received from Mr. Machin for the same purpose, and drawn from the same principle. But we may conclude this to be the best of them all, as he did not publish any other besides it.

M. Euler too, in his *Introductio in Analysin Infinitorum*, by a contrivance something like Mr. Machin's, discovers, that $\frac{1}{2}$ and $\frac{1}{3}$ are the tangents of two arcs, the sum of which is just 45° ; and that therefore the diameter is to the circumference, as 1 to quadruple the sum of the two series', $\frac{1}{2} \times (1 - \frac{1}{3.4} + \frac{1}{5.4^2} - \frac{1}{7.4^3} + \frac{1}{9.4^4} \&c.$ and $\frac{1}{3} \times (1 - \frac{1}{3.9} + \frac{1}{5.9^2} - \frac{1}{7.9^3} + \frac{1}{9.9^4} \&c.$ Both which series' converge much faster than Dr. Halley's, and are yet at the same time made to converge by the powers of numbers producing only short divisions; that is, divisions performed in one line, or without writing down any thing besides the quotients.

I come now to explain my own method, which indeed bears some little resemblance to the methods of Machin and Euler: but then it is more general, and discovers, as particular cases of it, both the series' of those gentlemen, and many others, some of which are fitter for this purpose than theirs are. This method then consists in finding out such small arcs, as have for tangents some small and simple vulgar fractions, the radius being denoted by 1, and such also that some multiple of those arcs shall differ from an arc of 45° , the tangent of which is equal to the radius, by other small arcs, which also shall have tangents denoted by other such small and simple vulgar fractions. For it is evident, that if such a small arc can be found, some multiple of which has such a proposed difference from an arc of 45° , then the lengths of these two small arcs will be easily computed from the general series, because of the smallness and simplicity of their tangents; after which, if the proper multiple of the first arc be increased or diminished by the other arc, the result will be the length of an arc of 45° , or $\frac{1}{8}$ th of the circumference. And the manner in which I discover such arcs is thus:

Let r , t , denote any two tangents, of which r is the greater, and t the less; then it is known, that the tangent of the difference of the corresponding arcs is equal to $\frac{r-t}{1+rt}$. Hence, if t , the tangent of the smaller arc, be successively denoted by each of the simple fractions $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. the general expression

for the tangent of the difference between the arcs will become respectively $\frac{2\tau-1}{2+\tau}$, $\frac{3\tau-1}{3+\tau}$, $\frac{4\tau-1}{4+\tau}$, $\frac{5\tau-1}{5+\tau}$, &c.; so that if τ be expounded by any given number, then these expressions will give the tangent of the difference of the arcs in known numbers, according to the values of t , severally assumed respectively. And if, in the first place, τ be equal to 1, the tangent of 45° , the foregoing expressions will give the tangent of an arc, which is equal to the difference between that of 45° and the first arc; or that of which the tangent is one of the numbers $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. Then if the tangent of this difference, just now found, be taken for τ , the same expressions will give the tangent of an arc, which is equal to the difference between the arc of 45° and the double of the first arc. Again, if for τ we take the tangent of this last found difference, then the foregoing expressions will give the tangent of an arc, equal to the difference between that of 45° and the triple of the first arc. And again taking this last found tangent for τ , the same theorem will produce the tangent of an arc equal to the difference between that of 45° and the quadruple of the first arc; and so on, always taking for τ the tangent last found, the same expressions will give the tangent of the difference between the arc of 45° and the next greater multiple of the first arc; or that of which the tangent was at first assumed equal to one of the small numbers $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. This operation, being continued till some of the expressions give such a fit, small, and simple fraction as is required, is then at an end, for we have then found two such small tangents as were required, viz. the tangent last found, and the tangent first assumed.

Here follow the several operations adapted to the several values of t . The letters a , b , c , d , &c. denote the several successive tangents.

1. Take $t = \frac{1}{2}$, then the theorem $\frac{2\tau-1}{2+\tau}$ gives $a = \frac{1}{3}$, $b = -\frac{1}{7}$. Therefore the arc of 45° or $\frac{1}{8}$ of the circumference, is either equal to the sum of the two arcs of which $\frac{1}{2}$ and $\frac{1}{3}$ are the tangents, or to the difference between the arc of which the tangent is $\frac{1}{7}$, and the double of the arc of which the tangent is $\frac{1}{2}$; that is, putting $A =$ the arc of 45° , then

$$A = \frac{1}{2} \times \left(1 - \frac{1}{3.4} + \frac{1}{5.4^2} - \frac{1}{7.4^3} + \frac{1}{9.4^4} - \&c.\right) + \frac{1}{3} \times \left(1 - \frac{1}{3.9} + \frac{1}{5.9^2} - \frac{1}{7.9^3} + \frac{1}{9.9^4} - \&c.\right)$$

$$\text{Or, } A = 1 - \frac{1}{3.4} + \frac{1}{5.4^2} - \frac{1}{7.4^3} + \frac{1}{9.4^4} - \&c. - \frac{1}{7} \times \left(1 - \frac{1}{3.49} + \frac{1}{5.49^2} - \frac{1}{7.49^3} + \frac{1}{9.49^4} - \&c.\right)$$

The former of these values of A is the same with that before-mentioned, as given by M. Euler; but the latter is much better, as the powers of $\frac{1}{4.9}$ converge much faster than those of $\frac{1}{9}$.

Corol. From double the former of these values of A subtracting the latter, the remainder is,

$$A = \frac{2}{3} \times \left(1 - \frac{1}{3.9} + \frac{1}{5.9^2} - \frac{1}{7.9^3} \&c.\right) + \frac{1}{7} \times \left(1 - \frac{1}{3.49} + \frac{1}{5.49^2} - \frac{1}{7.49^3} \&c.\right);$$

which is a much better theorem than either of the former.

2. If t be taken $= \frac{1}{5}$, then the expression $\frac{3t-1}{3+t}$ gives $a = \frac{1}{2}$, $b = \frac{1}{7}$. Here the value of $a = \frac{1}{2}$ gives the same expression for the value of Λ as the first in the foregoing case, and the value of $b = \frac{1}{7}$ gives the value of Λ the very same as in the corollary to the case above.

3. Taking $t = \frac{1}{4}$, the expression $\frac{4t-1}{4+t}$ gives $a = \frac{3}{5}$, $b = \frac{7}{23}$, $c = \frac{5}{99}$, $d = -\frac{79}{401}$. Here it is evident that the value of $c = \frac{5}{99}$ is the fittest number afforded by this case; and hence it appears, that the arc of 45° is equal to the sum of the arc of which the tangent is $\frac{5}{99}$, and the triple of the arc of which the tangent is $\frac{1}{4}$. Or that

$\Lambda = \frac{3}{4} \times (1 - \frac{1}{3.16} + \frac{1}{5.16^2} - \frac{1}{7.16^3} + \&c.) + \frac{5}{99} \times (1 - \frac{5^2}{3.99^2} + \frac{5^4}{5.99^4} - \frac{5^6}{7.99^6} + \&c.)$ Which is the best theorem that we have yet found, because the number 99 resolves into the two easy factors 9 and 11.

4. Let now t be taken $= \frac{1}{5}$; then the expression $\frac{5t-1}{5+t}$ will give $a = \frac{2}{3}$, $b = \frac{7}{17}$, $c = \frac{9}{46}$, $d = -\frac{1}{239}$. Where it is evident that the last number, or the value of d , is the fittest of those produced in this case; and from which it appears, that the arc of 45° is equal to the difference between the arc of which the tangent is $\frac{1}{239}$, and quadruple the arc of which the tangent is $\frac{1}{5}$. Or that

$\Lambda = \frac{4}{5} \times (1 - \frac{1}{3.5^2} + \frac{1}{5.5^4} - \frac{1}{7.5^6} + \&c.) - \frac{1}{239} \times (1 - \frac{1}{3.239^2} + \frac{1}{5.239^4} - \frac{1}{7.239^6} + \&c.)$ Which is the very theorem that was invented by Mr. Machin, as we have before mentioned.

5. Take now $t = \frac{1}{6}$; then the expression $\frac{6t-1}{6+t}$ will give $a = \frac{5}{7}$, $b = \frac{23}{47}$, $c = \frac{91}{305}$, $d = \frac{241}{1921}$, $e = -\frac{475}{11767}$. Of which numbers, it is evident, that none are fit for our purpose.

6. Again, take $t = \frac{1}{7}$, and the expression $\frac{7t-1}{7+t}$ will give $a = \frac{3}{4}$, $b = \frac{17}{31}$, $c = \frac{11}{28}$, $d = \frac{49}{205}$, $e = \frac{69}{742}$, $f = -\frac{259}{5263}$. Neither are any of these numbers fit for our purpose.

7. In like manner take $t = \frac{1}{8}$, so shall $\frac{8t-1}{8+t}$ give $a = \frac{7}{9}$, $b = \frac{47}{79}$, $c = \frac{297}{679}$, $d = \frac{1697}{5729}$, $e = \frac{7847}{47529}$, $f = \frac{14047}{388079}$.

8. And if t be taken $= \frac{1}{9}$, the expression $\frac{9t-1}{9+t}$ will give $a = \frac{4}{5}$, $b = \frac{31}{49}$, $c = \frac{115}{236}$, $d = \frac{799}{2239}$, $e = \frac{2467}{10475}$, &c.

9. Also, if we take $t = \frac{1}{10}$, the expression $\frac{10t-1}{10+t}$ will give $a = \frac{9}{11}$, $b = \frac{79}{119}$, $c = \frac{671}{1269}$, $d = \frac{5441}{13361}$, $e = \frac{41049}{139051}$, &c.

10. Further, if we take $t = \frac{1}{11}$, the expression $\frac{11t-1}{11+t}$ gives $a = \frac{5}{6}$, $b = \frac{49}{71}$, $c = \frac{234}{415}$, $d = \frac{2159}{4799}$, $e = \frac{9475}{27474}$, &c.

11. Lastly, if we take $t = \frac{1}{12}$, the expression $\frac{12t-1}{12+t}$ gives $a = \frac{11}{13}$, $b = \frac{113}{167}$, $c = \frac{41}{73}$, $d = \frac{419}{917}$, $e = \frac{4111}{11423}$, &c.

Here it is evident, that none of these latter cases afford any numbers that are fit for this purpose. And to try any other fractions less than $\frac{1}{12}$ for the value of t , does not seem likely to answer any good purpose, especially as the divisors after 12 become too large to be managed in the easy way of short division in one line.

By the foregoing means it appears, that we have discovered 5 different forms of the value of A , or $\frac{1}{4}$ of the semicircumference, all of which are very proper for readily computing its length; viz. 3 forms in the first case and its corollary, 1 in the 3d case, and 1 in the 4th case. Of these, the first and last are the same as those invented by Euler and Machin respectively, and the other 3 are quite new, as far as I know.

But another remarkable excellence attending the first 3 of the beforementioned series is, that they are capable of being changed into others which not only converge still faster, but in which the converging quantity shall be $\frac{1}{10}$, or some multiple or sub-multiple of it, and so the powers of it raised with the utmost ease. The series, or theorems, here meant are these 3.

$$1. A = \frac{1}{2} \times (1 - \frac{1}{3.4} + \frac{1}{5.4^2} - \frac{1}{7.4^3} + \&c.) + \frac{1}{3} \times (1 - \frac{1}{3.9} + \frac{1}{5.9^2} - \frac{1}{7.9^3} + \&c.)$$

$$2. A = 1 - \frac{1}{3.4} + \frac{1}{5.4^2} - \frac{1}{7.4^3} + \&c.) - \frac{1}{7} \times (1 - \frac{1}{3.49} + \frac{1}{5.49^2} - \frac{1}{7.49^3} + \&c.)$$

$$3. A = \frac{2}{3} \times (1 - \frac{1}{3.9} + \frac{1}{5.9^2} - \frac{1}{7.9^3} + \&c.) + \frac{1}{7} \times (1 - \frac{1}{3.49} + \frac{1}{5.49^2} - \frac{1}{7.49^3} + \&c.)$$

Now if each of these be transformed, by means of the differential series in cor. 3, p. 64, of the late Mr. Thomas Simpson's Mathematical Dissertations, they will become of these very commodious forms, viz.

$$1. A = \frac{4}{10} \times (1 + \frac{4}{3.10} + \frac{8\alpha}{5.10} + \frac{12\epsilon}{7.10} + \&c.) + \frac{3}{10} \times (1 + \frac{2}{3.10} + \frac{4\alpha}{5.10} + \frac{6\epsilon}{7.10} + \&c.)$$

$$2. A = \frac{4}{5} \times (1 + \frac{4}{3.10} + \frac{8\alpha}{5.10} + \frac{12\epsilon}{7.10} + \&c.) - \frac{7}{50} \times (1 + \frac{4}{3.100} + \frac{8\alpha}{5.100} + \frac{12\epsilon}{7.100} + \&c.)$$

$$3. A = \frac{6}{10} \times (1 + \frac{2}{3.10} + \frac{4\alpha}{5.10} + \frac{6\epsilon}{7.10} + \&c.) + \frac{7}{50} \times (1 + \frac{2}{3.50} + \frac{4\alpha}{5.50} + \frac{6\epsilon}{7.50} + \&c.)$$

Where α , ϵ , γ , &c. denote always the preceding terms in each series.

Now it is evident that all these latter series' are much easier than the former ones, to which they respectively correspond: for, because of the powers of 10 here concerned, we have little more to do than to divide by the series of odd numbers 1, 3, 5, 7, 9, &c.

Of all these 3 forms, the 2d is the fittest for computing the required proportion; because that, of the 2 series' of which it consists, the several terms of the one are found from the like terms of the other, by dividing these latter by 10 and its several successive powers 100, 1000, &c.; that is, the terms of the one consist of the same figures as the terms of the other, only removed a certain number of

places farther towards the right hand, in the decuple scale of numbers; and the number of places by which they must be removed, is the same as the distance of each term from the first term of the series, viz. in the 2d term the figures must be moved 1 place lower, in the 3d term 2, in the 4th term 3, &c. so that the latter series will consist of but about half the number of the terms of the former. Thus then this method may be said to effect the business by one series only, in which there is little more to do, than to divide by the several numbers 1, 3, 5, 7, &c.; for as to the multiplications by the numbers in the numerators of the terms, after they become large, they are easily performed by barely multiplying by the number 2, and subtracting one number from another: for since every numerator is less by 2 than the double of its denominator, if d denote any denominator (exclusive always of the powers of 10) then the co-efficient of that term is $\frac{2d-2}{d}$, or $2 - \frac{2}{d}$, by which the preceding term is to be multiplied; to do which therefore, multiply it by 2, that is double it, and divide that double by the divisor d , and subtract the quotient from the said double.

By a pretty exact estimate, which I have made, of the proportion of the trouble or time in computing the circumference by this middle form of the value of A , and by Mr. Machin's theorem, I have found, that the computation by his method requires about $\frac{1}{8}$ or $\frac{1}{10}$ more time than by mine. And its advantage over any of the series' invented by Euler or others, is still much more considerable.

XXIX. A very Extraordinary Effect of Lightning on a Bullock, at Swanborow, in the Parish of Iford near Lewes in Sussex. In sundry Letters from Mr. James Lambert, Landscape Painter at Lewes; and one from Wm. Green, Esq. at Lewes, to Wm. Henly, F. R. S. p. 493.

The bullock was pyed, white and red. The lightning, as supposed, stripped off all the white hair from his back, but left the red hair without the least injury. The bullock did not seem to have been hurt; his skin looked fair and well. A Mr. Rogers said, that he had 2 other bullocks struck in the same manner; one last summer, that was all white, was stripped of his hair like this; though not all over his back, but chiefly on his shoulders; the other, 2 years before, was pyed, and affected much like the present. He thought it could not be the effect of any disease, because the beasts were all in good health before and after this accident happened. He was more inclined to think it was the effect of lightning, because when he has had cattle disordered, so as to make their hair come off, he has never observed white hair to come off more than red, &c.; but that it has, if party-coloured, fallen off promiscuously, and generally in patches; and also by slow degrees, and never suddenly, as in the case of these bullocks. Mr. Green says, in the evening of Sunday the 28th of August 1774, at this place, there was an appearance of a thunder storm, but they heard no report.

A gentleman, who was riding near the marshes not far distant from this town, saw 2 strong flashes of lightning, seemingly running along the ground of the marsh, at about 9 o'clock in the evening. On Monday morning, when the servants of Mr. Rogers, a farmer at Swanborough, in the parish of Iford, went into the marsh, to fetch the oxen to their work, they found one of them, a 4-year old steer, standing up, to appearance much burnt, and so weak as to be scarcely able to walk; and by the description of it, it seemed to have been struck with lightning in a very uncommon manner. The ox is of a red and white colour; the white in large marks, beginning at the rump-bone, and running, in various directions, along both the sides; the belly is all white, and the whole head and horns also white. The lightning, with which it must have been struck, fell on the rump-bone, which is white, and distributed itself along the sides, in such a manner, as to take off all the white hair from the white marks as low as the bottom of the ribs, but so as to leave a list of white hair, about half an inch broad, all round where it joined to the red; and not a single hair of the red was touched. The whole belly is unhurt, but the end of the sheath of the penis has the hair taken off; it is also taken off from the deulap; the horns and the curled hair on the forehead are uninjured, but it is taken off the sides of the face, from the flat part of the jaw-bones, and it is taken off from the front of the face in stripes. There are a few white marks on the side and neck, which are surrounded with red, and the hair is taken off from them, leaving half an inch of white adjoining to the red. No hair was taken from the feet or legs, they were very dirty, except from the joint a little above one of the hoofs, where it was partly off. The farmer anointed the ox with oil for a fortnight; the animal purged very much at first, and is greatly reduced in flesh.

But though all the white hair on the upper parts was taken away, yet the tuft of white hair on the forehead never received any hurt at all. A Mr. Tooth, a farrier and bullock-leach, said, that this circumstance is not new to him; that he has seen a great many pyed bullocks struck by lightning in the same manner as this, both in his father's time, his father being of the same trade, and since; and that the texture of the skin under the white hair was always destroyed, though looking fair at first; and after a while it became sore, throwing out putrid matter in pustules, like the small-pox with us, which in time falls off, when the hair grows again as before; and that the bullocks receive no further injury. In this state Mr. Lambert found Mr. Rogers's bullock, the 2d time he saw it, which was about a month after the first visit; some of the scabs were then dropping off, and the hair was coming on afresh. He remembers perfectly well, that all the cattle that he had seen, which were killed by lightning, were either black, brown, or red, without any white at all in them. This bullock is both marked and affected by the stroke exactly alike on both sides.

Mr. Tooth gave an account of a pyed horse of his being killed, 4 or 5 years before, in a stable adjoining to his house, by a stroke of lightning which happened in the night; and being very great, Mr. Tooth thinking it struck his house, immediately got up and went to the stable, when he saw his horse was struck, and almost dead to appearance, though it kept on its legs near half an hour before it expired. The horse was pyed white on the shoulder and most part of the head; that is, all the forehead and nose, where the greatest force of the stroke came. The hair was not burnt nor discoloured, only so loosened at the root, that it came off at the least touch. And this is the case, Mr. Tooth observes, with all he has seen or heard of, viz. the hair is never burnt, but the skin always affected. In the above horse, Mr. Tooth says, all the blood in the veins, under the white parts of the head, was quite stagnated, though he could perceive it to flow as usual in other parts of the body, under the brown hair; and the skin, together with one side of the tongue, was parched and dried up to a greater degree than any he had ever seen before. The horse stood in a stall close to the door of the stable, which was boarded on that side, and through them, he thinks, the lightning struck him:

Mr. Lambert sent another instance of the effect of lightning on a bullock of Mr. Alse's, at Glynd; it was similar to the other he sent in every respect, except that the stroke on this must have been greater, as the scarf-skin seemed to be peeling off with the hair all over the rump. He thinks too that this is more curious, as all the red spots, even those small ones on the side, remain firm and smooth, without the least injury. Here also, as in the former instance, after the lightning had passed the greatest diameter of the body, the white hair is left entire, particularly under the belly, on the legs, &c.

To the preceding account Mr. Henly adds the following queries: 1st. Are not the dark-coloured hairs stronger in their texture than the white or light-coloured ones? * 2dly. If the dark-coloured hairs are the strongest, may not this be owing to their being more deeply rooted, and partaking more largely of that nutritive matter which produces and supports hair? And does not the change of dark-coloured hair to grey, in persons advanced in years, seem to favour this supposition? 3dly. If the above suppositions are allowed, may not any internal injury to the skin, as a violent electric explosion passing through it, prove more fatal to the white or light hair than to the black, red, or darker colours?

Having mentioned the foregoing particulars to Dr. A. Fothergill, at Northampton, he has favoured me with some conjectures, which I shall take the liberty of annexing to this paper; viz. "The recent fact you mention, of the

* This is a fact so well known to house-painters, that they do not by choice admit a dark hair into their brushes, as they would occasion a disagreeable roughness in their work. J. COVENTRY, —Orig.

effects of lightning on the white hair of a bullock, is extremely curious, but seems difficult of solution. Whether it can be explained from the difference of texture between red hair and white, is doubtful; or whether there is not something peculiar in colours, as being conductors or non-conductors of electricity, may deserve inquiry. The phlogiston, or inflammable principle, is thought to be the foundation of colour in bodies, and to abound in proportion to the intensity of the colour. But phlogiston and the electric fluid are probably the same, or at least modifications of the same principle; therefore red bodies are perhaps replete with electric matter, while white bodies may be destitute of it.* A body saturated with it cannot receive more, and may escape, while a neighbouring body, not calculated to receive it, may, on its admission, be destroyed.† Or there may exist a chemical affinity between electricity and the different rays of light, which, in attracting some, and repelling others, may be the foundation of many curious phenomena. But, while we admire the effects, the habitudes and modus operandi of these subtile fluids may perhaps for ever elude the cognizance of our senses."

XXX. Of the Light produced by Inflammation. By George Fordyce,‡ M. D., F. R. S. p. 504.

When a body is heated to a certain degree, it becomes luminous, and is said to

* Many substances must certainly be excepted from this rule. W. HENLY.—Orig.

† This effect of lightning generally happens to such bodies which, in some measure, resist its entrance, &c. merely on account of their being imperfect conductors. W. HENLY.—Orig.

‡ This distinguished philosopher and physician was born at Aberdeen, in 1736, and died in London of a dropsical affection in 1802. After the usual elementary instruction at a grammar-school, he was sent to the college at Aberdeen, and from thence, at the age of 15, he went to England, and was received as a pupil, by his uncle, Dr. J. Fordyce, who practised physic at Uppingham, in Northamptonshire. With this relation he remained several years, and then removed to Edinburgh, for the further prosecution of his studies in physic. He took his degree at that university in 1758 or 1759. After this he went to Leyden, and on his return to England; he resolved to settle in London. As he had expended the whole of his patrimony on his education, and had acted contrary to the wishes of his relatives and friends, in choosing the metropolis of England as the place of his residence, he felt that every thing depended on his own abilities and exertions. Thus circumstanced, he began to deliver a course of lectures on chemistry; and the emolument he derived from this source, induced him a few years afterwards to lecture on the materia medica and practice of physic, also. Before this period, no other lectures, appertaining to the profession of physic, beside those on anatomy, had been delivered in the metropolis: Dr. F. may therefore be considered as one of the founders of the London Medical School, having first set the example of publicly communicating instruction on three very important branches of medical study, without a knowledge of which anatomy is of no avail; namely, the materia medica, pharmaceutical chemistry, and the history and treatment of diseases. This example originated in Dr. F. and has since been followed by many other physicians, in London, with no less credit to themselves than advantage to their pupils.

In 1765, Dr. F. was admitted a Licentiate of the College of Physicians, and about 5 years after he was chosen physician to St. Thomas's Hospital. In 1776, he was elected F. R. S. and in 1787 he

be ignited. One of the means of producing heat is inflammation ; and this, as is well known, is sufficient for ignition. But besides the light produced by ignition, there is also light produced by the inflammation itself. For the investigation of this principle, it will be necessary to consider ignition in the first place.

Substances, heated to between 6 and 700° of Fahrenheit's thermometer, begin to be luminous in the dark. If they be colourless, the light which is first observed is red ; as the heat is increased, there is a mixture of yellow rays ; and lastly a due proportion of all the coloured rays to form a pure white, which has been commonly called, by chemists, a melting heat. The intenseness of this light depends much on the density of the heated body ; for while metals, heated to this degree, throw out a strong light, the vapour at the end of the flame of a blow-pipe, properly applied to a lamp, is not visibly luminous, though the heat be so great as immediately to give a white heat to glass. The colour of this light is affected by the colour of the ignited matter. While zinc is calcining, the pure white calx throws off a light, which vies with that of the sun in brightness and purity ; the green calx of copper gives to the flame of a fire, in which it is calcining, a beautiful green ; and tallow burning in a candle, being converted into empyreumatic oil, as it passes off from the wick, the yellow colour of this oil gives a yellowness to the flame, which very much alters the colours of objects seen by candle light from what they appear to be in the day.

The light produced by the decomposition of bodies in inflammation is totally independent of the heat, and its colour is blue ; for substances which burn, without producing 600° of heat of Fahrenheit's thermometer, give light during their inflammation. Thus, phosphorus of urine, exposed to the air, burns and is decomposed, producing light with very little heat ; and that this is a true in-

was admitted a Fellow of the College of Physicians. At this time the college was occupied in revising its Pharmacopœia, a business for which Dr. F. was peculiarly qualified ; accordingly it is said that many of the improvements, especially in the chemical processes, were suggested by him.

Dr. F.'s medical works consist of *Elements of the Practice of Physick* ; a *Treatise on the Digestion of the Food* ; his 4 *Dissertations on Fever* ; and 3 *Memoirs* inserted in the *Medical and Chirurgical Transactions*. His philosophical writings, besides his *Elements of Agriculture and Vegetation*, consist of the following papers, many of them relating to chemistry, inserted in the *Phil. Trans.* namely, the present paper on the *Light produced by Inflammation* ; an *Examination of various Ores in the Museum of Dr. W. Hunter*, (*Phil. Trans.* vol. 69 ;) *A New Method of Assaying Copper Ores*, (*Ibid.* vol. 70 ;) *Experiments on the Loss of Weight in Bodies when Melted or Heated*, (*Ibid.* vol. 71 ;) *Account of an Experiment on Heat*, (*Ibid.* vol. 77 ;) the *Cronian Lecture on Muscular Motion*, (*Ibid.* vol. 78 ;) *On the Cause of the additional Weight of Metals on being Calcined*, (*Ibid.* vol. 82 ;) *Account of a New Pendulum*, being the *Bakerian lecture*. Dr. F. was moreover the author of those curious experiments related by Sir Chas. Blagden, (*Phil. Trans.* vol. 65 ;) in which different persons were exposed to high degrees of temperature, in rooms heated for the purpose.

For other particulars concerning Dr. F., the reader is referred to the *Gent. Mag.* and *Month. Mag.* for the year 1802.

flammation and decomposition appears from this experiment: take a receiver of white glass, capable of holding 6 or 8 gallons; put into it a drachm of phosphorus of urine, finely powdered, and half an ounce of water; cork the mouth of the receiver, and tie it over with a bladder, so as to exclude the external air; incline the receiver to all sides gently, and afterwards set it to rest; the powder will adhere to the sides, and the water will drain from it. As soon as the water is sufficiently drained off, the particles of the phosphorus will become luminous, and emit a thick smoke: this will continue for some days; but at last no more light or vapour will appear. Open the receiver, and you will find that the air will have contracted, as it does from the inflammation of a candle in Van Helmont's experiment; that is, about a 20th part. It is become unfit for inflammation; for if a lighted candle be immersed in it, it will be extinguished as well as the phosphorus, and an animal will be suffocated by it. The air then has suffered the same change, as that air which has served for the inflammation of other bodies; and the phosphorus is partly decomposed, the water in the receiver being impregnated with its acid, and the air saturated with its phlogiston. Blow fresh air into the receiver, and the light and smoke will immediately re-appear. In like manner, it is known that sulphur will burn and give light without heat sufficient for ignition. Take a piece of iron heated nearly red hot, and throw a little gunpowder on it. If the heat be of a proper degree, the sulphur will burn off with a blue flame, without heat sufficient for ignition; for if such heat had been produced, the gunpowder would certainly have taken fire, which it does not. It is the inflammation and decomposition of the sulphur, and not its evaporation, which produces light; for if we sublime sulphur in close vessels, made of the most transparent glass, no light will be visible, except at the very beginning, when a small portion of it burns till the air in the vessel be saturated, and rendered unfit for inflammation.

That the light produced by the decomposition in inflammation is blue, in whatever degree of heat the inflammation takes place, appears from observing the bottom and lower part of the flame of a candle, where the inflammation is; the light produced is blue. Or take a candle which has burnt for some time, extinguish it by applying tallow to the wick, and let it stand to cool; afterwards set it on fire by the flame of another candle: at first no more vapour will arise than can be acted on by the air at once; inflammation therefore will go on in the whole small flame, and it will be blue. It may be necessary to observe here, that when a candle burns, the following process happens: the tallow boils in the wick, and is converted into empyreumatic oil, rising from it in the form of vapour. As it rises from every part of the wick, the volume is increased till it comes to the top, and gives to the lower part of the flame the form of a frustum of an inverted cone. The air is applied to the outer surface of the column

of vapour, and, there decomposing the empyreumatic oil, produces heat and blue light; the stratum of vapour, within the outer burning surface, is heated white-hot; the heat diminishes towards the centre, which, if the flame be large, is scarcely red-hot; as the column rises, decomposition taking place constantly on its surface, it necessarily diminishes, and the upper part of the flame is conical. That the tallow boils in the wick can be seen: that it is converted into empyreumatic oil is proved by drawing the vapour, rising in the middle of the flame, where it does not burn, into a glass tube; the empyreumatic oil condenses. This also shows that the flame does not burn in the middle. That the heat is produced on the outer surface appears, if we take a small rod of glass, and put the end of it in the blue flame on the surface; it will be heated white-hot and melt. Immerse the rod into the flame, so that the point shall be in the centre, it will melt and bend where it is in the blue flame on the surface; whereas, if the flame be large, the point which is in the centre will hardly be heated red-hot. That the empyreumatic oil is decomposed, is proved by burning a candle with a very small wick in distilling vessels, no condensation of empyreumatic oil takes place. We may conclude therefore that light is produced by the decomposition, as well as by the ignition, in inflammation.

Lest the manner of powdering phosphorus should not be known, Dr. F. gives the process: take phosphorus of urine 2 drs.; put it into a 4 oz. phial; pour on it 3 oz. of water; heat it gently, by immersion in warm water, till the phosphorus melts; shut the phial with a cork; take it out of the water, and shake it briskly till it be cold; the phosphorus will be found in powder.

XXXI. Experiments on Ignited Bodies. By J. Roebuck, M. D., F. R. S. p. 509.

Mr. Buffon asserts, that he found a ball of iron, which weighed 49 lbs. 9 oz. when cold, to weigh, when heated to a white heat, 49 lbs. 11 oz. which is an augmentation of weight of $19\frac{1}{2}$ grains to the pound. This extraordinary fact, circumstantially narrated by that ingenious author, being contrary to the opinions of those philosophers who have most enlarged our natural knowledge by their candid and cautious inquiry into the qualities of bodies, induced Dr. R. to make similar experiments.

Some time ago, when at Birmingham, he had very luckily an opportunity, by the aid of two accurate balances of Mr. Bolton's; one of which would, without straining the beam, weigh a pound and turn with the 10th of a grain; and the other weigh half an ounce, and turn with the 100th part of a grain. Dr. R. heated a piece of iron, of nearly 1 lb. weight, to a white heat, or what the smiths call a welding heat, and found, by the most accurate experiments he could make, and several times repeated, that the iron, when left several hours in the balance to cool, weighed nearly one grain less when cold than when hot;

and that a piece of iron, of about 5 dwts. which was tried by the smaller and more accurate balance, weighed, as appeared by an index which moved opposite to a quadrant, somewhat more when cold than when hot. Dr. R. tried the same experiment on copper; and found a piece of copper, of nearly 1lb. weight, 4 grains lighter after it had been left some hours to cool in the balance than when it was put in. He repeated the experiment, and found the event the same; but suspecting this might possibly arise from the copper casting scales, he placed a sheet of white paper under the balance, and collected as many scales as made up nearly the deficiency of the weight.

April 29, 1776, Dr. R. heated a cylinder of wrought iron, which weighed 55 lb. to a white heat, and exactly balanced the same, when hot, in the presence of many members of the R. S. After the cylinder had been 2 hours cooling in the scale, he weighed it again, and found that it had increased in weight 3 dwts. and a few grains. Five hours after cooling, Mr. Magellan weighed it, and found it had increased in weight 3 dwts. 17 grains. Six hours after cooling, when the cylinder was blood-warm, Dr. R. weighed it again, and found it to have increased in weight 4 dwts. The day following, about 22 hours after cooling he again weighed it, and found that it had increased in weight 6 dwts. 17 grains. Mr. Abram Whitehouse, who was very solicitous to have the above experiment made accurately, procured from Mr. Samuel Read a very exact beam, which readily turned, to the conviction of all the above gentlemen, with less than a pennyweight, though loaded with the above iron cylinder; but Matthew Raper, Andrew Crosby, Esquires, and Dr. R. examined the beam leisurely and accurately, and found it turned very distinctly with 4 grains, though loaded as above. In order to discover the cause of this increase of weight of the cylinder when cold, Dr. R. heated 2 oz. 8 dwts. of the scales or calx of wrought iron, and found the same to increase in weight 5 grains when cold. Dr. R. heated 2 pieces of pure silver which weighed 2 lb. 10 oz. 5 dwts.; and when the silver was cold it increased in weight 5 grains, though it produces no calx from being heated red-hot.

XXXII. Experiments and Observations on a New Apparatus, called, A Machine for exhibiting perpetual Electricity. By Mr. William Henley, F. R. S. p. 513.

Mr. George Adams, philosophical instrument maker to his majesty, lately showed to Mr. H. a little apparatus, which he called a machine for exhibiting perpetual electricity, invented by Mr. Volta. This machine consisted of a circular plate of glass, about 8 inches in diameter, covered on one side with a coating of bees-wax and rosin, about the 16th part of an inch thick. This coat of wax, &c. being strongly excited with a dry warm flannel, he placed on it a cir-

cular board, of the same dimensions, coated with tin foil, and furnished with a glass handle screwed to, and standing upright on it. These bodies having remained in contact some seconds, the board was raised up by the glass handle; when, applying the knuckle to the tin foil coating, a snap was heard, a spark seen, and a small sensation felt. On replacing the board, and permitting it to remain some seconds, as before, having touched the tin foil with a finger, on removing it again, and applying the knuckle, as at first, the same phenomena were produced; and might, Mr. Adams observed, be repeated for a long time, without any renewal of the excitation of the wax, any further than the replacing the board might be said to excite it. It immediately occurred that, as this plate of wax, &c. was made by excitation, a strong negative electric, the phenomena produced by it could only be the reverse of those Mr. H. had formerly made with an excited plate of glass, and published in the *Phil. Trans.*, vol. 64, viz. where Mr. H.'s were positive, these were negative; and where his were negative, these were positive.

But, to determine this matter, Mr. H. made the following experiments. First, he insulated Mr. Canton's electrometer, and having electrified the balls positively, he presented toward them the excited wax, as soon as it had been separated from the coated board; and perceived that the balls were attracted by the wax; but, if the balls were electrified negatively, they were as plainly repelled by it. The board produced just the contrary effect. Secondly, he held his Leyden vacuum, or analysis of the Leyden bottle, described *Phil. Trans.*, vol. 64, by the coated bulb, and touched the brass ball on the neck of it with the coated board, the moment it had been separated from the excited wax, &c. and instantly perceived a variety of beautiful streams dart from the point of the wire in the bottle, and spread themselves in different directions through the bulb. On repeating the experiment, and presenting the coated part of the bottle toward the board, a small spark of light appeared on the point of the inclosed wire; a plain indication that the point had received electricity, and that the coated board, being separated from the wax, &c. was strongly electrified plus; and consequently the coating of wax, &c. on the plate of glass, minus. These phenomena, being so often produced, without a fresh excitation of the wax, though they are astonishing to strangers, will not be so surprizing to electricians, who have considered Mr. Grey's experiment with a cone of sulphur, contained in a glass vessel, which, as often as they were separated, showed signs of electricity in all states of the weather. See Dr. Priestley's *History of Electricity*, 2d edit. p. 39.

Mr. H. has showed, at large, in a former paper, that merely heating either glass or amber will not make them electrical; but the friction of glass against glass, or sealing-wax against sealing-wax, previously warmed, will excite either

of these substances. But, pressing a finger in the gentlest manner on the amber, after heating, will excite it. Indeed, a fine piece, which Mr. H. frequently carried in his pocket, he always perceived to be electrical, without any other friction than what it received from the pocket. And negative electrics, per se, being once thoroughly excited, are observed to retain their electrical quality very long, as they do not so soon attract the moisture in the atmosphere as glass. Glass however will retain its electricity many hours. Mr. Canton informed him, that having excited a rod of glass very strongly, he set it at some distance from the fire in his parlour, and found that it was electrical, after standing in that situation, in dry air, 24 hours. How much longer it would have retained its electricity, had he let it remain there, he knew not. How long a large and neatly prepared Leyden bottle will retain its charge, so as to be sensibly electrical, Mr. H. had never experienced; but Dr. Priestley observes, *History of Electricity*, p. 516, that he has more than once received such shocks, as he should not like to receive again from the residuum of his battery, even 2 days after the discharge, and when papers, books, his hat, and many other things, had lain on the wires the greatest part of the time. Even the residuum of a residuum, he says, he has known to remain in his battery many days. One thing, however, is very remarkable in Mr. Adams's apparatus, viz. supposing the negative electric to have parted with its electricity to the rubber; why, when the coated board or plate of metal is set upon it, and that plate is touched by a finger, the equilibrium is not thus presently restored? But, perhaps, when the electric matter, naturally inherent in bodies, is once thoroughly excited and put in action, it is not so soon as might be suspected reduced again to a quiescent state, especially in bodies so peculiarly adapted to affect each other as these appear to be.

XXXIII. Account of the Iron Ore lately found in Siberia. By Petr. Simon Pallas, M. D., F. R. S. Dated Petersburg, Nov. 6, 1775. p. 523.

“ I have embraced the opportunity (says Dr. P.) of a parcel I sent to Mr. Drury, to offer the Society a specimen of the native iron, of which I found out a large mass in the Siberian mountains, which is now transporting to Petersburg. I read in some foreign journals, that a short account of this mass has been published in the last volume of *Philos. Trans.* from a letter of M. Staehlin, of our Academy; but as the contents of it, drawn from the informations I gave to our Academy in my itinerary relations, seem not to have been exact, I beg leave to give you here a faithful and fuller account of the place and circumstances in which that memorable mass was found.

It is to be observed, that in the neighbourhood of the river Jenisei, one of the largest that runs from the south through Siberia and to the Northern

Ocean, and near which the mass of native iron has been discovered, there is great plenty of iron ores, as well in the flat layers towards the northern level of the country, where, among others, whole banks of ochraceous minerals, with scattered trees and pieces of wood turned to rich iron ore, and near the town of Jeniseisk, a rich iron ore, in the form of white clay and white sparry stones, is to be found; as also in the steep mountains, where the strata dip very considerably, and ores of iron, copper, and even impregnated with gold, are found in veins and nests. On the mountains, that lie along the eastern side of the above-mentioned rivers, from 56° to 52° of latitude, where the highest ridge of mountains begins, iron ores are most common, and the mountains generally consist of grey or black slates and shivers, which rise steeper, or in a greater angle to the horizon, as they come nearer to the high ridge of mountains, and approach more to a level position, as they extend to the north. Some of these secondary mountains are very high, rising very often to some thousand feet above the sea surface, and most of them are covered with forests. A very rich iron ore in veins was here discovered in the year 1749, on a steep, woody mountain, about 10 English miles from the river Jenisei, and 180 miles from the town of Krasnojarsk, situated on that river to the southward, about 54° of latitude, between two rivulets, known by the names of Ubeï and Sisim, and running into the river on the eastern side. This place was then visited by the Russian miners; but as there was plenty of iron ores situated much nearer to the Fabrics, the mine never was worked, though the ore contains above 70 pounds of iron in the hundred weight, being of a dark steel colour, turning red when rubbed, and in some parts endowed with a magnetic virtue. On the same mountain, where this mine is situated, on the north side, much below the top of the mountain, the mass of native iron lay on the very ridge, without being fixed to the rock, which is a grey, stratified saxum. There was, on that and the neighbouring mountains, no trace of ancient miners and their kilns, which are found in many other parts of Siberia, and in which those miners, of some former and hitherto unknown nation inhabiting these parts, mostly worked on copper ores. Nor could so large a mass ever have been formed in the small kilns of these people, which never could yield more than 50 or 60 pounds of metal at a time; whereas this mass, in its first condition, weighed above 1680 Russian pounds. It is throughout of the nature you may see in the specimen which M. Drury will deliver. The iron is formed in a coarse, spongy texture, mostly pure, perfectly flexible, and fit to be worked to small tools by a moderate fire; but in a more violent one, and chiefly being melted down, it becomes dry and brittle, resolves in grains, and will no more stick together, nor extend under the hammer. In its natural state, the iron itself is incrustated with a kind of varnish, which has preserved it from rust; but, wherever this is lost, or the iron bars broken, rust

comes on very readily. The cavities formed by the iron are equally filled up with a kind of fluor, which for the most part is of a clean, transparent, amber colour, cuts glass, has none of the properties of scoria, and forms, according to the hollows it fills, various roundish grains or drops, very glossy and clean, on their surface, having one or more flat surfaces. This fluor is extremely brittle, and thus, by cutting off any part of the mass, this substance is lost, and comes off partly in grains, and partly in form of a coarse powder of vitrescent matter. The whole mass has no regularity of form, but resembles a large, oblong, somewhat flattish pebble, and is coated on the outside with a matter resembling some blackish brown iron ores. This coat however covers not the whole mass; it is also very rich of iron, and even the transparent fluor yields some pounds of iron in the hundred. Whoever will consider the mass itself, or large specimens of it, will not have the least doubt of its being worked by nature, since it has no one character of scoriaceous matters melted by artificial fire, or commonly found among volcanoes.

With regard to these, as seeming a probable place where this mass could have been formed, it may not be amiss to add the following observations. The mountains, where it was found, are part of the northern extensions of that mighty chain of mountains which runs from west to east through Asia, and forms the natural limits of Siberia, with the Desarts of Tartary, the Mongols, and the Chinese Empire. From the river Irtysh, where the forehills and lower parts of these mountains yield, in a great many places, the richest silver ores, the chain runs generally somewhat to the north-east, and therefore extends to the east of the river Jenisei, over a much greater part of Siberia than what it did before. Its forehills are almost every where composed of rocks and strata, rising very steep to the horizon, and the horizontal layers are only found in the level country, in which also all kinds of fossil and petrified sea productions are very scarce, and only found in the very northern parts of Siberia. Common flint is as scarce in Siberia as petrifications, and nothing like productions of volcanoes any where to be found. Even in some places, where hot springs are found, these seem only due to collections of pyritæ of no great extent, and the slight earthquakes which are sometimes observed about the river Irtysh, and more frequently about the lake Baikal, certainly rise in the very neighbourhood of this lake and of the Noor Saissan, which gives rise to the river Irtysh; and about these lakes never any thing like a volcano has been heard of, nor is there one known in the northern part of Asia, except those in Kamtschatka and the islands newly-discovered between that peninsula and the continent of North America. The same may be assured of the Urallian mountains, a ridge that runs from south to east, and continues to the very Northern Ocean and Nova Semlja, being only interrupted by the Strait of Waygat. It is this

ridge of mountains that makes the natural limit between Europe and Asia, and to the east of which the largest share of true remains of elephants, rhinoceroses, and large buffaloes, is found in the banks of all the larger rivers that run from the above-mentioned chain of mountains to the Northern Ocean, and yield such remains from the places where they reach the plains of Siberia (no such bones being ever found in the higher mountains) to the very ocean; where the frozen earth of the northern plains preserves these remains of southern animals in such perfection, that when I was at Irkuzk, the head and two legs of a true rhinoceros were sent from the river Wilui, with its skin and part of the tendons preserved on them, which are now in the Museum of our Academy, and are fully described and figured in the 17th volume of *Nova Commentaria Petropolitana*."

XXXIV. On the Crystallizations observed in Glass. By James Keir, Esq.,
p. 530.

The peculiar figure of rock crystal has been long observed. Many other substances, as spars, precious stones, pyrites, ores, metals,* salts, water† and oil,‡ are also known to affect a uniformity of shape, when they are exposed to certain degrees of heat, cold, fluidity, and other necessary circumstances. From their resemblance in this respect to rock crystal, they are said, when they assume their peculiar forms, to crystallize; and the regularly shaped bodies, into which these substances concrete, are also called crystals.

In many substances, when broken, the parts appear to have some determinate figure. This determination of figure, or grain, as it is called, is obvious in bismuth, regulus of antimony, zinc, and all other metallic bodies, which may be broken without extension of parts; and though the ductility of gold, silver, lead, and tin, prevents the appearance of the peculiar grains, when pieces of these metals are broken, yet we have reason to believe, that by exposing them to proper circumstances, they also would show a disposition to this species of crystallization, as it may be called, by a further extension of that term; for Mr. Homberg has observed, that when lead is broken while hot, in which state it is not ductile, a granulated texture appears. Perhaps all homogeneous bodies,

* Native gold has been found in a crystallized form. M. Rome de Lisle in his *Essai de Crystallographie*, p. 390, says, that he has seen pieces of native gold which were 8 sided solids, like crystals of alum, and one piece which was an hexagonal plate. In Dr. Hunter's museum, some fine specimens of crystallized native gold are to be seen. Gold may be crystallized by art also. Some æther having been poured into a solution of this metal in aqua regia, I observed, a few months afterwards, the gold separated from the menstruum, in the form of a distinct polygonous prisms.—Orig.

† The various and regular forms of the particles of snow, which is nothing else than water crystallized, are well known.—Orig.

‡ The crystals, formed by cold, in the oil of sassafras, have been observed to be very beautiful, regular, hexagonal prisms.—Orig.

in their transition from a fluid to a solid state, would, if this transition were not effected too hastily, concrete into crystals, or bodies similarly figured. Instances of such crystallization have occurred in glass, which had passed very slowly from a fluid to a solid state; and the form, regularity, and size of these vitreous crystals have varied according to the circumstances with which their concretions had been accompanied. Mr. K. sent, along with this paper, a few specimens of this crystallized glass, together with a drawing of some of the most remarkable crystals.

The pieces of glass, marked N^o 1, were taken from the bottom of a large pot, which had stood in a glass-house furnace at the time the fire was allowed gradually to extinguish. In this case, the mass of heated matter was so great, that without the addition of fuel, the heat continued long, and the transition of the glass from a fluid to a solid state was very slowly accomplished. The upper part of this glass was found to be changed into a white, semi-opaque substance, resembling in colour and texture some of the white spars. Under this crust, which, in some places, was a quarter of an inch thick, and in others more, the glass was transparent, but considerably obscured, and its colour was changed from a dark green to a dull blue. In this semi-pellucid glass were dispersed many white, opaque, regular crystals, the form of which was generally that of a solid, whose side view is represented by fig. 2, and its basis by fig. 3, pl. 1. The surface of these crystals seems to be bounded by lines rather elliptical than circular, which are so disposed, that a transverse section of a crystal, that is, a section perpendicular to its axis, is a hexagon, as in fig. 4 and 5; the former of which represents a view, and the latter a plan, of that section. In the middle of each basis of the crystal, a conical cavity appears, as in fig. 2 and 3. The elliptical lines which bound the surface of the crystals seem to be occasioned by the edges of many thin plates, so arranged round the axis of each crystal, that their longitudinal diameters are parallel to that axis. Of these plates, 12 are larger, more conspicuous, and better defined, than the rest. They are placed in pairs, at an equal distance from each other, forming the 6 angles of the hexagonal section and basis, as appears in fig. 2, 3, 4 and 5. The intervals between the pairs of plates, that is, the areas of the triangles into which the hexagonal section is divided by these pairs, are filled up partly by smaller plates affixed to the sides of the principal plates, and of each other, at an angle of 60°, and partly by a substance somewhat less opaque and darker coloured than that of the plates. The size of the contiguous and of the neighbouring crystals does not vary much, though that of crystals found at different depths of the same pot were observed to differ considerably. The greatest diameter of the crystals from which the figures 2, 3, 4, and 5, were copied, was about $\frac{1}{20}$ part of an inch, so that these figures represent the crystals con-

siderably magnified, All the crystals are not by any means formed with the same exactness as those described; some having the hexagonal form less distinctly marked; but the regularity of most of them is so obvious, that no doubt can remain of the perfection of the crystallization.

Another kind of vitreous crystallization appears on the piece of glass marked N^o 2, which was taken from the bottom of a pot, that had been pulled out of the furnace while the glass was red hot. The crystals are of 2 kinds; those represented by fig. 6 are of the columnar form; their altitude is about the 8th of an inch, and the diameter of their bases about a 5th part of their altitude; their sides seem to be irregularly fluted, or cut in grooves. The other kinds of crystals, represented by fig. 7, 8, and 9, have bases of nearly the same diameter as the columnar ones; but their altitude is much less, being only about a 6th part of their diameter. Their bases are bounded by lines seemingly ragged and irregular; but several of them show a tendency to an hexagonal form, the regularity of which may have been disturbed by the motion of the melted glass acting on and bending these very thin crystals, while they were hot and flexible, at the time when the pot was drawn out of the furnace.

The specimens marked N^o 4, are pieces of a glass-house pot, down the outer sides of which some melted glass had run, and adhered long enough for the formation of various kinds of crystals. The inner sides also of these pieces are covered with glass variously crystallized. Some of these crystals seem to be semi-columns, of which the flat sides, or interior surfaces, are exposed to view, and are represented by fig. 10. Other crystals, represented by fig. 11, seem to consist of several semi-columnar ones, uniting together in the same plane round a common centre, like broad, flat spokes of a wheel. Many of these spokes seem to become narrower as they approach the centre of the wheel, and therefore resemble more the segments of frusta of cones cut along their axis, than of cylinders. But perhaps this appearance proceeds only from the semi-columns being so disposed near the centre of the wheel, that the edge of one is laid over the edge of the contiguous semi-column, like the spokes of a fan.

In the specimen of glass, marked N^o 4, which had run through a crack in a pot, and had remained adhering to the bars of the grate of a furnace sufficiently long for a crystallization to take place; some of the crystals appear oblong and needle-like, and others globular, or nearly of the globular form. In this piece of glass, many of the needle-like crystals are seen to unite round a common centre; and though they have probably been prevented, by the too sudden cooling of the glass, from concreting in a sufficient number to make complete globular crystals, yet they sufficiently show the manner in which those which are complete have been formed. All the crystallizations, hitherto described, were observed in a dark, green window-glass, made at Stourbridge, and called broad-

glass. This glass is composed of sand, kelp, calcarious earth, and lixiviated vegetable ashes.

Crystallizations frequently occur also in the glass of which common bottles are made, the materials used in the composition of which are nearly the same as those abovementioned for broad-glass, with the addition sometimes of the scoria of iron furnaces. Of this kind is the specimen marked N° 5, in which the crystals are not enveloped in a medium of transparent uncrystallized glass, for the whole piece is an opaque, crystallized substance; but they are prominent from the surface of the mass. The form of the crystals is that of the blade of a two-edged sword, whose point is truncated. In no other glass had Mr. K. seen such perfect crystals as in those 2 kinds abovementioned, broad and bottle-glass, which being more fluid and less tenacious, when melted, than any other, the minute particles of which crystals consist, more easily concrete, and apply themselves to each other with less resistance from the medium. Perhaps also the greater proportion of calcarious and other earthy particles may dispose these glasses to crystallize more than others, which contain a larger quantity of saline and metallic fluxes.

Flint-glass, when long exposed to a dull red heat, acquires a cloudiness, which probably proceeds from a number of small white particles, concreted by means of crystallization: but these crystals are too minute for observation. Mr. K. suspects also that the opaque whiteness, given to glass by arsenic, is the effect of a crystallization, to which this substance disposes certain kinds of glass; for the opacity given to such glass by arsenic, being greater than the opacity of the arsenic itself, cannot be communicated to a large proportion of transparent glass, merely by the mechanical interposition of this opaque, and sometimes only semi-opaque substance.

Mr. Reaumur has observed, that some kinds of glass, by long exposure to certain degrees of heat, acquire a white opaque crust on their surface; and that this change of colour and texture, by a longer continuance of the heat, penetrates farther, till at length the whole substance of the glass is converted into a white, opaque body, which, from some supposed resemblance to porcelain, has been distinguished by the name of Reaumur's porcelain, but is really nothing else than glass indistinctly crystallized.

Some of the properties of glass are considerably changed by crystallization; its transparency is destroyed, and it acquires an opaque or semi-opaque whiteness, its density is increased, for the density of a piece of crystallized glass was found, by experiment, to be to that of water as 2676 to 1000; whereas the density of a piece of uncrystallized glass, which had been contiguous to the former, and consequently had been composed of the same materials, and exposed to the same heat and other circumstances, was to the density of water, as 2662 to

1000. The brittleness of glass is diminished by crystallization; for crystallized glass is less apt to crack by change of heat and cold.

Crystallization is always accompanied or preceded by an evaporation of the lighter and more fluid parts of the glass; for Mr. K. found that, by exposing a piece of transparent glass till it was entirely crystallized, a 58th part of its weight was lost by evaporation: and he was induced to believe, from other trials, that glass, which contains too large a proportion of saline fluxes, is less capable of crystallizing than other harder glasses, till it has lost its superfluous quantity of such fluxes by evaporation. A doubt may therefore arise, whether the change of properties, induced by crystallization, be merely the effects of altering the texture, that is, the arrangement of the minute integrant parts of glass; since this change is always accompanied with a loss of the lighter parts of the crystallizing substance. But though a superfluous quantity of saline or other fluxes may impede the crystallization, yet that the change of properties induced by crystallization is principally or solely the effect of an alteration of texture, is evident from this observation; that a piece of crystallized glass, when exposed to a heat considerably more intense than is sufficient merely for its fusion, and afterwards hastily cooled, loses all its acquired properties, and is again reduced to the state of transparent brittle glass, which however, by means of the evaporation it has sustained of its lighter and more volatile parts, is rendered considerably harder, denser, and less fusible, than it was before the crystallization.

Many analogous instances might be adduced to show how much the properties of bodies depend merely on the different arrangements of their integrant parts, or on their modes of crystallization. Thus, for instance, cast-iron and steel, when cooled suddenly, acquire a much finer grain or texture than when annealed, or slowly cooled, and are also more hard, elastic, brittle, and sonorous. From the above description of vitreous crystals we learn, that very different crystallizations occur in the same kind of substance exposed to different circumstances; and even that sometimes differently shaped crystals are found in the same piece of glass; in which case, the circumstances must have been the same. Perhaps indeed the difference observable in the shape of the crystals, in the same piece of glass, may only mark the different periods in the progress of crystallization; for the crystals represented by fig. 7, 8, and 9, which are found in the same piece of glass as those represented by fig. 6, chiefly differ from these in their altitude; and perhaps the latter kind may have been composed of a number of the former uniting by their bases. The wheel-like crystals also, fig. 11, seem to consist of several of the semi-columnar ones, arranging themselves round a common centre, like the spokes of a wheel. The globular crystals in the specimen N^o 4 have been already observed to consist of many needle-like crystals, converging to one central point.

Does not this discovery, of a property in glass to crystallize, reflect a high degree of probability on the opinion, that the great native crystals of basaltes, such as those which form the Giant's Causeway, or the pillars of Staffa, have been produced by the crystallization of a vitreous lava, rendered fluid by the fire of volcanos? This opinion is further confirmed by the following considerations. The prismatic and other regularly-shaped basaltes have been almost always found to be accompanied with lava, pumice-stones, and other vestiges of the fire of the volcanos, whenever they have been carefully examined by intelligent naturalists, as has been shown by M. Desmarests, in his *Memoir on the Basaltes of the province of Auvergne, in France*; *Mem. de l'Acad. des Sciences*, 1771. Basaltic columns have even been discovered, according to the same author, among the productions of volcanos now existing, as of those of Mount Etna and of the Isle of Bourbon.

2. The substance of which these basaltic masses consist, is generally of the same nature and appearance as the neighbouring and adjoining lava. It is generally compact, fusible, and of various degrees of hardness, probably according to the matters of which the vitreous mass was compounded. M. Desmarests has further observed, that the prismatic basaltes of Auvergne is actually a continuation, and generally the termination, of a current of lava.

3. Though the variety of the forms of the crystals, in the same kinds of glass, and even in the same piece of glass, which has been already remarked, sufficiently shows the uncertainty of any inference drawn from a similarity of shape; yet it may not be improper to mark the analogy, in this respect, between the basaltic and vitreous crystals. The columnar or prismatic form is known to appear most generally in the crystallized basaltes. Of this form also are evidently the crystals represented by fig. 6. The semi-columnar, vitreous crystals, fig. 10, seem to be analogous to the no less singular basaltic semi-columns observed in the Giant's Causeway by Bishop Pocock, *Phil. Trans.*, vol. 48; which he says were exactly like hexagonal columns cut in two. M. Desmarests has observed, in the province of Auvergne, great quantities of spherical and ellipsoid basaltic concretions, which were formed of polygonal columns, rather pyramidal than prismatic, converging from the circumference to the centre. These seem to be perfectly analogous to the vitreous globular concretions which have been above observed to be composed of oblong crystals, arranged in a similar manner. The same author also observed, in the same province, regularly-shaped tables or plates of basaltes; of which, he says, assemblages were accumulated in all directions. We have shown, that the crystals represented by fig. 2, 3, 4, and 5, are really assemblages of plates or tables, disposed in every direction round a common axis.

Lastly. The stone on which the columns of basaltes generally rest, and which sometimes also is supported by these columns, being of the same nature and

texture as the columns themselves, seems to be a mass irregularly crystallized, analogous to the irregularly-shaped masses in the specimens of glass N^o 1 and 2, which evidently consist of a similar substance as the neighbouring crystals, and seem to have been composed of a number of these crystals indistinctly united; for the peculiar figures of crystals are distinct only when they are insulated, or when they are separated from each other by a pellucid or differently-coloured medium. A medium of this kind appears between the vitreous crystals, and is nothing else but the more fluid parts of the glass, which longer resist the concretion, but which, by a further continuance of the heat would have become with the parts already crystallized, one uniform, white, opaque substance, without any interposition of transparent glass, or distinction of crystal, except on the surface, as in specimen N^o 5, in which the crystals stand prominent from the indistinct mass, and unenveloped in any medium, in the same manner as the basaltic crystallizations appear standing above the mass of stone or lava which supports them.

Further observations on the basaltic and vitreous crystals may probably suggest more instances of analogy between these two substances. No just objection can be drawn against this analogy from the magnitude of the former compared with the minuteness of the latter: for the difference of size between the small vitreous crystals and the stupendous basaltic columns, which support mountains, islands, and provinces, is no more than is proportionate to the difference usually observed between the little works of art and the magnificent operations of nature.

XXXV. A Belt on the Disc of Saturn described in an Extract of a Letter from Mr. Messier, F. R. S., to Mr. Magellan, F. R. S. Dated Paris, May 29, 1776. p. 543.

I have observed, since the 14th of May, a belt of a fainter light on the body of Saturn, opposite to the part of the ring behind the planet. It is pretty broad, and almost as distinct as those of Jupiter. It was with a very good achromatic of 3 feet and a half, made by Dollond, that I discovered this appearance. I wish you would communicate it to the astronomers, because those who are furnished with better instruments may perhaps see some inequalities in this belt of Saturn, and so the time of the planet's revolution on its axis may be better ascertained than it is at present. Messrs. John and James Cassini seem to have been the only astronomers who discovered this phenomenon about the end of the last century.

XXXVI. An Account of some Poisonous Fish in the South Seas. By Mr. Wm. Anderson, Surgeon of H. M. Ship the Resolution. p. 544.*

* The fishes which caused the symptoms detailed in this paper appear to have been the *Tetrodon*

Some of this ship's company having eaten of fish which were afterwards supposed to be poisonous, Mr. Patten, the surgeon, ordered some warm water to be drunk, in order to make the patients vomit: which a little relieved some of them. After the nausea had ceased, he gave some weak portable soup, as a diluent; and for the most troublesome symptom, viz. the heat on the surface of the body, he prescribed a sudorific julep, the active ingredients of which were the antimonial wine and spiritus mindererii. This brought on a breathing sweat, which, for a time, abated the violence of the pains. No other medicines were used, except some purging salts, for preventing inflammation, in 2 or 3, whose mouths and throats had been more particularly affected. Their diet consisted chiefly of tea, sago, and portable soup. Mr. A. has omitted saying any thing about the manner in which the poison operates, as the instances were too few, to draw any certain consequences from them; but only observes, that its action may be such, as to affect and deprave some of the organs of sensation, without much irritating the first passages; because in all the patients the disorder of the stomach and bowels had long ceased before the other symptoms went off. And he was confirmed in this opinion by a circumstance which afterwards happened to Capt. Cook, who, having eaten a small piece of the liver of another kind of fish (a tetraodon)* was not sensible of being hurt by it, till waking in the night, and calling for a draught of water, he neither could feel the vessel with his hands, nor was sensible of its weight when he grasped it. On the other hand, it was remarked, that some of the other gentlemen, who had likewise eaten of that fish, had also a vomiting and looseness. The difference perhaps depended on the quantity taken into the stomach, and the particular constitution of the person.

July 23d, 1774, on board his majesty's ship the Resolution, off the Island Malicolo, in the South Sea, 3 fish, of the same species, that had been caught, being dressed for dinner, affected all those who ate of them in an uncommon manner; but 5 persons, who had eaten of one of them, were more severely attacked than the rest. Immediately after eating, nothing was felt but some uneasiness, or such pain as follows from swallowing any acrid substance, in the mouth and throat. About 2 in the afternoon, some felt an uneasiness in the stomach, with an inclination to vomit; but it was near the evening before those who suffered most were affected. The symptoms at first were universal lassitude

ocellatus, and the *Sparus Pagrus* of Linnæus. It has been supposed, with great appearance of truth, that when such consequences follow, they have been owing rather to the food of the fish, than to any thing inherently poisonous in its flesh. Many marine animals among the tribe of *Mollusca* are of a highly acrimonious and irritating nature, and the fish which prey upon them are more or less imbued with the acrimonious juices of such animals.

* *Tetrodon ocellatus*? Linn. This fish, unless very carefully cleaned before eating, is said to have often proved fatal in the space of two hours.

and weakness, followed by a retching; and in some, by gripings and looseness. To these succeeded a flushing heat and violent pains in the face and head, with a giddiness and increase of weakness; also a pain, or, as they expressed it, a burning heat in the mouth and throat. Some had the mouth affected in such a manner, that they imagined their teeth were loose; which might really be the case, as a considerable spitting attended this symptom. The pulse all this time was rather slow and low. The retching and looseness did not last long; but the pain and heat of the head were extended to the arms, hands, and legs. The patients continued in this manner all the night, but with some intervals of ease. Towards the morning, the pains, especially those in the legs and arms, but more particularly about the knees, were severer than before. These would sometimes remit and frequently shift, or be more violent in one place than in another. Sometimes the pain would remove suddenly from the legs, and fix in the head; the palms of the hands were hot; and the fingers, legs, and toes, felt often as if benumbed: even the whole limbs became in some measure paralytic, the sick person being unable to walk unless supported. Though there appeared no swelling in the face it might be observed to have a sort of shining or gloss on it; and the patient sometimes imagined his nose was grown to a great size.

24th, In the forenoon they continued much the same; but some, after sleeping, were rather easier; and one had a copious spitting, which however gave him no relief, for at noon the pains in his limbs ceasing, they were removed to his head, which they affected violently with a sense of throbbing and great heat; nor were there any of the patients, but this one, who complained much of thirst during the illness. Another, in particular, had the pains in his knees so increased, that they made him cry out. The uneasiness at the stomach and heat of the throat in all had nearly ceased. When the mucus about the fauces was forced away by straining, it felt hot, and left the same sensation about the throat for some time. In the afternoon, most of them grew much easier, but continued weak; and 2, who now seemed better than they had been before, complained of heat and soreness in their hands and feet.

25th, All 5 had rested tolerably in the night; but complained of weakness and soreness of the mouth, with heat in the hands and feet. One, who had been rather worse than the others, still had a considerable spitting. 26th, All continued better, but the pains were not entirely gone. The great weakness, with heat in the hands and feet, were still general complaints, and the soreness in the mouth remained in some. The one mentioned as having a large discharge of saliva, continued spitting; and another began to have the same complaint, though in a less degree.

27th, One of the 5 had no complaint but a disagreeable sensation on rubbing

his skin in any part of his body. Another, as yesterday, mentioned a slight giddiness, weakness, &c. A 3d, who had been the worst of all, and who had taken some purging salts, was much better. The other 2, who had seemed so well the day before, became worse at night; one of them, who before had violent pains in his knees, had a return of the same symptom; the other had pains in his legs, and a universal uneasiness. 28th, They were all considerably better, except one, who had been seemingly well the night before, but complained now of much weakness, and flying pains in his limbs. 29th, All of them mended, but still complained of wandering pains in the limbs, weakness and heat in their hands and feet. 30th, Continued as yesterday. At times they seemed quite well; but in the morning they complained of more weakness than in the preceding evening; and the pains, which generally appeared to be almost gone in the day, returned at night. 31st, All were better, but not without some slight pains and a weakness in the morning; and some still felt a disagreeable heat in their hands and feet, and others in their mouth.

August 1, One had no complaint. The rest not altogether free from pains, and a sense of weariness or weakness. 2d, All were pretty free from pain; but one had still too much heat in his hands. 3d, All recovered; some trifling pains and a little weakness excepted.

These notes Mr. A. kept of the 5 who had eaten of one fish. With regard to those who had eaten of the two other fish, they were not so severely affected. No signs of illness appeared among them till night; some had then a nausea, retching, and some loose stools. Others felt only pains in the backs, arms, and legs, as in a rheumatism; but it was observed, that in all, the face was more or less affected with an uneasy sense of stiffness. All these, though at first slightly ailing, and though the symptoms were later in coming on, had this circumstance attending them, that they were all worse next day in the evening. On the 25th, they continued much the same as on the day before in the afternoon; but towards night they got somewhat better. They had pains in their arms and legs, with a weakness and sense of heat on the surface of the body. 26th, All complained in the same manner; and, though the pains were less, they continued, often shifting suddenly from one place to another, and the weakness rather increased. 27th, Two of them, who had suffered least, remained much the same. The others were sensibly better. 28th, Most of them had wandering pains in their arms and legs, but were, in other respects, visibly better. One who, for the first 2 or 3 days, had scarcely complained, said, that this day he had a head-ach and pains in his legs and arms. 29th, None of them yet perfectly recovered; all complaining of lesser pains in their legs, arms, or back. 30th, All continued much the same. 31st, All better, but not altogether free from pains and weakness. August 1, Some were

recovered, and the rest better. 2d, All of them were pretty well; and they continued so.

Several dogs, who had eaten of these fish, and especially of the guts and bones, seemed affected in a higher degree than the men. Their illness might perhaps have begun in the night, but was not perceived till next morning. One, in particular, had violent retchings, a paralysis, especially of the hind legs, lay down, rolled, howled, and showed other signs of great pain. More saliva or froth hung about his mouth; and, when fatigued with struggling, would fall down, and seemingly breathe with great labour. Two of them continued in the same way all day; though others that had eaten as much appeared but slightly affected, their principal symptom being only weakness in the hind legs. It was observed, that most of them were troubled with a swelling of the penis. One lay the first day, all the afternoon, without being able to move, but groaned perpetually, and lay in appearance in great anguish. Next day, they seemed all somewhat better, except one, to whom tobacco-juice had been given the day before, to make him vomit: he died in the afternoon. On the 3d day, though the dogs were more free from pain, yet they continued almost motionless, nor did they begin to eat. On the 4th day, they were all much better, the worst of them running about, and eating his victuals. On the 5th, all of them seemed recovered, one excepted, who had not been so soon or so violently attacked as the others. This remark was made of the men likewise; to wit, that those who were more slightly and later attacked continued ill as long as the others. Some days after, the same dog became so paralytic in his hind legs, that he could not stand; but afterwards he recovered the use of them pretty well.

A hog, which had eaten of the offals, died next morning; as did a perroquet on the 2d day, which had got from his master some of the boiled fish. It will be proper to add, that another hog died, which had fed on the entrails of that fish of which Captain Cook had eaten a bit of the liver; and that a dog, which about 3 weeks after had eaten of a fish of the same species, died, after a lingering illness, of the same nature with that which had affected others of his kind.

XXXVIII. Experiments on Ignited Substances. By Mr. John Whitehurst.
p. 575.

The experiments of Buffon on ignited bodies seem to prove, that when heated to the degree he mentions, they are more ponderous than when cold. The experiments which I have made on heated metals, suggest a different idea, and contradict the fact he relates; so that I am induced to believe, that some circumstance, not attended to, has introduced a mistake in the account this learned

philosopher has published as the result of his inquiry. His experiment stands thus recorded (Suppl. Nat. Hist. vol. 2, p. 11): a mass of iron, after receiving a white heat, weighed 49 lbs. 9 oz.; when restored again to the temperature of the atmosphere 49 lbs. 7 oz. Hence he concluded, that the igneous particles, contained in the heated iron, increased its absolute weight 2 ounces.

My experiments are as follow: 1st, 1 dwt. of gold, made red hot, became apparently lighter; but, when restored again to the temperature of the atmosphere, its former weight was perfectly restored. 2d, 1 dwt. of iron, heated as above, was also apparently lighter; but, when it became cool again, its weight was visibly augmented. I repeated these experiments several times, and the results were always the same. The beam used in these experiments was sensibly affected by the $\frac{1}{2000}$ part of a grain; and each of the metals was heated on charcoal, by means of a candle and a blow-pipe, and both were brought nearly to a state of fusion.

It seems needless to observe, that the apparent levity of the gold and of the iron, when hot, was owing to the ascent of the rarefied air above the scale, and to the tendency of that underneath to restore the equilibrium of its pressure. The increase of weight in the iron might probably arise from its having, in some degree, acquired the property of steel, by means of the flame and charcoal. I am at a loss to account for the fallacy which seems to have attended M. Buffon's experiment; but it seems probable, that the heat of the mass of iron employed by him, had a greater effect on that arm of the beam from which it hung than on the other; which, being less heated, would consequently be less expanded; and this difference of expansion might produce the error in his account of the weight of heated iron.

XXXIX. An Account of a Suppression of Urine cured by a Puncture made in the Bladder through the Anus. By Dr. Robert Hamilton, M. D., of King's-Lynn, Norfolk. p. 578.

The subject of this history had laboured under a suppression of urine for 3 days. The treatment usual in such cases had been adopted without success, and attempts had been made to introduce catheters of various sizes, but all in vain. The bladder was distended to an enormous size; the pulse was small and quick; hiccup had come on, and he had vomited every thing he had swallowed from the 1st day of his illness. In this extremity, Dr. H. resolved on puncturing the bladder, as the only chance of saving the patient. While Dr. H. was considering which would be the best mode of performing the operation, the mother of the patient told him that she had several times that day attempted to give him a clyster, but had not been able to introduce the pipe, by reason of a large substance low in the gut near the fundament, which stopped up the passage. It

immediately occurred to Dr. H. that the obstructing body could be no other than the distended bladder, which, having filled the pelvis, pressed downwards where there was the least resistance towards the anus, as well as upwards into the abdomen. From this circumstance he was led to think of discharging the urine by a puncture into the bladder, with a trocar introduced by the anus. He conceived that this method would have advantages superior to any other he had heard of, for simplicity, ease, and safety. The finger could guide the point of the instrument to the very spot of the bladder to be pierced. The coats of the intestinum rectum and bladder, and the intervening cellular membrane, were all that were to be perforated, and they were now pressed so closely together, that there could be no more art required than in piercing any simple bag of water. When the surgeons (who had gone to fetch their instruments) returned, Dr. H. told them what had occurred to him concerning the descent of the distended bladder (for he had not yet examined it) and its pressing the rectum downwards; he represented to them the hazard and difficulty attending the operation hitherto in use, and proposed this method of perforating the bladder with a trocar introduced by the anus. They readily acknowledged the advantage of such a practice, and agreed to give it the preference. They examined with a finger in ano, and felt a large round tumor, a very little way within the orifice, pressing the anterior side of the rectum downwards, and pushing the anus and perinæum considerably outwards. The gut itself was loose and empty; and through its relaxed sides the tumor, which was evidently the bladder, was distinctly felt stretching every way, completely filling the pelvis, and feeling like the membranes which contain the waters of a woman in labour, thrust into the dilated vagina. Dr. H. described the manner in which he thought the puncture should be made; and, as he imagined that he could better execute what he had himself conceived than another person, he offered to do it; which being readily assented to, the operation was performed in the following manner. Having placed the patient on his back on a bed, with his breech projecting a little over the side of it towards the light, and his legs bent into the position they are placed in for the operation of lithotomy, and held by two assistants, a trocar of the middle size, with its point guarded by the extremity of the fore-finger well oiled, was introduced into the anus, till the tip of the finger reached the anterior part of the tumor; when the finger being a little withdrawn, and the point of the instrument brought into contact with the tumor, it was plunged into it, in a direction parallel to the axis of the bladder, in an erect posture; and the perforator being pulled out, the water immediately followed. A straight catheter was quickly introduced through the canula into the bladder, lest, as it collapsed and shrunk upwards as the water was discharged, the canula should prove too short, for its shell was then close to the anus. The canula was then slipped out of the aper-

ture as far as the rings of the catheter, as being no longer of use, and the catheter remained in the bladder till the urine was all drawn off; during which time, that instrument was moved different ways, to search for the stone which was supposed originally to have occasioned the disorder; but none was to be found. The water being discharged, the catheter was taken out, and the patient put to bed. The parts were repeatedly fomented; and a draught, with half a drachm of nitre and 25 drops of laudanum were given, and 2 more of the same kind were ordered for the night.

26th. He had had a very good night, and had made water 5 or 6 times through the aperture made by the trocar. He said, that as soon as the bladder had collected a certain quantity of urine, he had felt an inclination to make water; that then sitting on a chamber-pot or bed-pan, and straining in the usual way, the urine had rushed out at the aperture per anum in a stream; and that none had passed by the urethra. Not contented with his account of the matter, Dr. H. desired him to make water in his presence, and was witness to this curious and extraordinary power of retention of the urine in a wounded bladder, and of discharging it at pleasure through an artificial passage. The emphysematous swelling of his head and face was almost gone. He was directed to drink the pectoral decoction with the addition of some marsh-mallow root, sweetened with manna, and acidulated with orange or lemon-juice; and the nitrous opiate was repeated.

27th. He complained of a fulness of his belly, probably owing to his not having had a stool. He still made water through the trocar aperture as before, and whenever he pleased; but now he began to perceive a little urine come by the urethra at the same time. They did not choose to order a clyster, lest the gut should be injured by the pipe, but to wait the effect of the manna, which, with the night-draught, was still continued. The resolution of the inflammation being now begun, as was conjectured by the urine finding its natural passage by the urethra, a bougie was introduced beyond the stricture at the neck of the bladder, and to very good purpose. 28th. He was much better, he had had some stools, and had passed most of his urine by the urethra. 30th. No water had issued through the aperture either this morning or the preceding day; the whole, though in a small stream, had flowed by the urethra. He complained of a soreness in ano. 31st. This day he felt that soreness only when he went to stool. The stream of urine by the urethra was still small. April 6th. The puncture through the rectum and bladder appeared to be quite healed. The urine was discharged in a tolerable stream, the passage being, as he observed, much wider than it had been for 13 years before.

He continued the daily use of the bougies: and, being sensible of the great benefit he had received from them, willingly persevered in their use, till the

stricture was so much lessened, as to permit a free discharge of his water, and by these means he obtained a complete cure : for, in 2 months after, he left the town in every respect well. It was remarkable, that during the progress of the cure no urine was perceived to ooze involuntarily through the opening ; but it was always retained till the patient, prompted by a fulness of the bladder, made his water, as has been related.

Dr. H. subjoins that he had neither heard nor read of any method of perforating the bladder similar to that here related, before Sir J. Pringle informed him that it was not a new operation ; and that he would find an account of it in Pouteau's *Melanges de Chirurgie*. Dr. H. had since procured the book, and read the paper with great satisfaction, and was much obliged to Sir J. P. for his intelligence. He did not presume to claim any merit from a thing which took its rise from mere accident. The obstruction which the pipe met with when the patient's mother attempted to give him the clyster, suggested the method of piercing the bladder by the anus ; and he was persuaded, that the same thought would have arisen, on a like occasion, in the mind of any thinking man. And he begged Sir J. P. would believe, that he had no small satisfaction in finding it occurred to M. Flurant, the ingenious author of the paper on that subject, in that publication, from a circumstance which, though not exactly similar, amounted nearly to the same thing. This surgeon having introduced his finger into the anus, to examine the state of the bladder, in order to perform the puncture in perinæo, found it so round and tumid, and so much within the reach of his instrument, that he thought he could perforate it with safety in that place ; and, from a little reflection on the structure of the parts, was convinced of the expediency of operating in this manner, in preference to any other.

Dr. H. adds that he had found a composition of calomel and opium, in large doses, the best internal remedy for suppressions of urine, in general ; and that he had repeatedly seen this medicine succeed after the usual means have failed.

He was convinced, from these trials, that the principal or specific efficacy is in the calomel, as large doses of opium alone, or joined with camphire, have proved unsuccessful. He was so well satisfied of the advantages of this practice, that, if called early in the disorder, he directs, 10 grs. of calomel, with 2 grs. of solid opium, made into a bolus with any conserve, to be taken immediately, and repeated in 6 hours. He had seldom occasion to order a 3d dose, the patient being generally relieved by the 2d, if the first has failed. He did not administer it in the case here related, because the alarming situation the patient was in when he came to him required the bladder to be emptied without delay.

XL. Observations made during the late Frost at Northampton. By A. Fothergill, M.D. p. 587.

This account relates to the severe frost on the last 4 days of Jan. 1776. The

lowest state of the thermometer was at 9° , that is, 23° below the freezing point, being $3\frac{1}{2}^{\circ}$ below that of the remarkable frost in the year 1739. Having placed on the garden wall, half an ounce of each of the following liquors in a cup, viz. lemon-juice, vinegar, and red-port wine, in that state of the thermometer he found them all perfectly congealed.

Being desirous to see the effect of a high degree of artificial, added to the natural cold that now prevailed, the thermometer was immersed into a frigorific mixture; but though it sunk the quicksilver in a few seconds, into the bulb of the thermometer, yet the result was by no means adequate to that of the experiment of Professor Braun at Petersburg: for though the quicksilver in the thermometer, and that in the phial, contracted a film on the top, yet it remained quite fluid below.

Jan. 31st to Feb. 1st, the barometer at 29; the thermometer only at 16° , that is, 16° below the point of congelation; the atmosphere serene and pleasant. Feb. 2d, wind s.; barometer $29\frac{1}{2}$; a warm, misty morning, succeeded by a pleasant, spring-like day, ushered in a very mild and agreeable thaw, the thermometer from 9° being got up to 40° ; so great was the change of temperature in so short a space of time! And it seems worthy of observation, that the epidemic cold, which had prevailed universally during the preceding mild season, suddenly disappeared in the late intense frost; but now began to re-appear, together with rheumatic affections and other diseases of the former period.

XLI. Account of the Magnetical Machine contrived by the late Dr. Gowin Knight, F.R.S., and presented to the R. S. By Jahn Fothergill, M.D., F.R.S. p. 591.

Dr. F. being left executor to Dr. Knight, a very extraordinary magnetic machine of his contrivance, and which had cost him much labour and expence, came into Dr. F.'s possession. This machine, which may be observed to consist of 2 parts, is by no means so strongly magnetical as it was at the doctor's decease. Not long after this event, it was necessary to remove this apparatus from his apartments in the British Museum. One of these parts was fixed up in the Museum, the other was left at the lodgings of J. H. De Magellan, for the purpose of some experiments, and also for impregnating strongly the needles of sea-compasses. Here it was accidentally destroyed by fire, and the parts it consisted of rendered almost wholly useless. A new one was however made, and impregnated with the magnetical power, by the ingenious gentleman abovementioned, according to the method of Dr. Knight. It has acquired a considerable degree of magnetic force, by being placed in the polar line with the other part of this machine that was unhurt, and where in time it will, perhaps, acquire a considerable degree of magnetic energy.

The first thing, it seems, that engaged the doctor's attention more particularly to magnetism, was the accident that befel a ship's compass from lightning; and of which the doctor gave a very circumstantial account to the society. This affair led him to consider the structure of the compass more minutely. He procured compass-cards ready armed, as it is called, from different makers both at home and abroad. He found most of the needles strangely erring from due polarity; some being many points to the west, others as many to the east of the right position. Among them all there was only one, which to him seemed constructed on a rational plan, and was of French make, procured from Marseilles; but even this was not without very evident faults.

To fix on the proper form of a needle through which the magnetic effluvia could pass with the least interruption; to give the needle such a degree of hardness as to retain the magnetic influx the longest, and with the greatest force, were material objects; and, it seems, with a view to have such a degree of magnetic power at his command, as to force the magnetic virtue through the most consolidated bars, was his first inducement to try whether he could not collect such a magazine of magnetism, as would be sufficient for every purpose of this kind, and at the same time exhibit some new phenomena in physics yet undiscovered. With this view he planned and executed his machine.

His first attempt however was much smaller; a few bars were laid in the due course of the magnetic flux, and impregnated by constant attrition. To these other bars were successively added, after they had been impregnated, both by the force he could give them by attrition, and what he could derive from the preceding stock collected in the bars. To these he added still fresh bars, till he had formed the whole mass, resting on wheels and pivots, in such manner, as to be easily manageable for the purpose of impregnating the needles he was employed to see prepared, for the service of government, and others, who had generosity enough to think, that the compass, on which depended the lives of the ship's crew, could not be made too perfect, and that it deserved a reasonable compensation. It is to the Doctor's ingenuity and indefatigable attention to this useful instrument, that it has acquired among us a degree of perfection unknown to our predecessors.

When the machine was completed, he still was adding continually to its power. He impregnated every single bar of which it is composed, by repeated attritions, and applied it to the remaining bars in their magnetic position. After this operation, he always found its efficacy for a season considerably diminished; for the effluvia of each bar, though increased in virtue, seemed not immediately to have acquired a communication with each other. However, it grew always more powerful after each of these operations; and it is more than probable, if a person could be found, who, with equal patience and skill, would at proper distances

repeat the same process, that the present machine would acquire a degree of force superior to what the original ever possessed; for much depends on time and a due position. If to these was added a fresh impregnation of each single bar, by the means hitherto made use of, we would probably possess a larger fund of magnetic power, than exists in any artificial magnet now in being. But if this cannot be obtained, if an able person cannot be prevailed on to renew its vigour in this manner, it might possibly afford the curious some satisfaction, to know whether, in its present state, it loses any force, or acquires fresh virtue; to know, with some degree of precision, how much weight it will now suspend; and to observe annually its variation. A trial of this nature demands no small attention. Even the motion of a carriage in the street, though at such a distance as the society's apartments, will make a considerable variation.

It is not known, that the Doctor left behind him any description of a composition he had made to form artificial load-stones. Many of his friends have seen such a composition; which retained the magnetic virtue in a manner much more fixed, than either any real load-stone or any magnetic bar, however well tempered. In the natural ones he could change the poles in an instant, so likewise in the hardest bars; but in the composition the poles were immovable. He had several small pieces of this composition, which had strong magnetic powers. The largest was about half an inch in breadth, very little longer than broad, and near a quarter of an inch thick. It was not armed, but the ends were powerfully magnetic; nor could the poles be altered, though it was placed between two of his largest bars, and they were very strongly impregnated. The mass was not very heavy, and had much the appearance of a piece of black lead, though not quite so shining. It is believed he never divulged the composition; but Dr. F. thinks he once told him, the basis of it was filings of iron, reduced by long-continued attrition with water to a perfectly impalpable state, and then incorporated with some pliant matter, to give it due consistence. Perhaps some of his acquaintance may have been more fully informed of this circumstance; and it may be rendering great aid to future inquirers, to know every thing that can be collected relative to so curious a subject.

Each magazine, in the two parts of this machine, consists of 240 bars, disposed in 4 lengths; every length containing 60 bars, placed in 6 courses or layers, in contact with one on another; and 10 in each course, placed side by side, in contact also. The bars being very nearly of a size, the ends of those in one length are in contact with the corresponding ends of those in the adjacent lengths. The magnetical north ends of these bars, in each magazine, are all directed one way towards *N*; and the south ends the contrary way towards *S*; thick plates of iron cover these ends *N* and *S*; the junction of the ends of the bars fall under brass braces.

As it has been found difficult, after the final hardening of these bars, to preserve among them a perfect equality in size; therefore, the contact of their sides are perfected by thin iron plates, slipped in between the braces and the junction of the ends of the bars: and these plates, being pressed by the screws passing through the sides of the braces, keep the ends of the bars in as close contact as their figures will permit; and, that the bars may be kept end to end in contact, the iron plate at the north end and at the south end is perforated with 60 holes, one against the end of each bar, with a screw fitted to each hole: every screw having a square head may, by help of the key, be turned, and, by pressing against the end of the bar in the 4th length, force it against its abutting bar in the 3d length, and so on till the bars, end to end, are brought into contact and kept so. The braces are in 2 pieces; the sides and bottom in 1; and the other piece forms the top, which is held close to the bars by the screws passing through it into the upright sides of the braces.

As each of these magazines weighed about 500 lb. it became necessary to have them so placed as to be conveniently used. The Doctor, therefore, by screws fixed the braces, containing the bars, to a strong mahogany plank, about 1 $\frac{3}{4}$ inches thick; the screws passing through the plank entered the bottom parts of the braces. Against the middle of the whole length, 2 strong brass plates are well fixed to the sides of the plank; to these brass plates are fixed 2 cylindrical gudgeons, which projecting from the sides, like the trunnions of a cannon, lie in the sockets of the standard, by which the magazine easily turns, as on an axis, and is so well poised as to stand in any inclination of the line NS; and in this the equilibrium is assisted by the strong mahogany semi-circular pieces, fixed in a vertical position to the middle of the under part of the plank, on which the magnetic apparatus rests. The standards are fixed to the square frame, and the whole supported on 4 trucks, by which the 2 magazines are easily brought end to end, or set at a convenient distance, so as to admit a bar to be placed between the ends, to be made magnetical.

XLIII. A Demonstration of two Theorems mentioned in Art. XXV of the Philos. Trans., for the Year 1775. By Charles Hutton, Esq., F.R.S. p. 600.

This is a geometrical demonstration of the 2 polygonal theorems, communicated by Mr. Lexel of Petersburg, and printed in p. 647 of this volume of Abridgments; but unnecessary to be here repeated.

XLIV. Experiments to Ascertain the Nature of some Mineral Substances; and, in particular, to see how far the Acids of Sea Salt and of Vitriol contribute to mineralize Metallic and other Substances. By P. Woulfe, F.R.S. p. 605.

The component parts of the several mineral substances on which Mr. W.'s

experiments were made, have been so accurately determined by subsequent chemists, as to render it unnecessary to reprint this paper.

END OF THE SIXTY-SIXTH VOLUME OF THE ORIGINAL.

I. Account of a Woman in the Shire of Ross living without Food or Drink. By Dr. Mackenzie, Physician at New Tarbat, dated April 3, 1775. Vol. LXVII. Anno 1777. p. 1.

Janet M'Leod, unmarried, aged 33 years and some months, daughter of Donald M'Leod, of Kincardine, Ross-shire; in the 15th year of her age had a pretty sharp epileptic fit; she had till then been in perfect health, and continued so till about 4 years after, when she had a 2d fit, which lasted a day and night; and a few days afterwards, she was seized with a fever of several weeks continuance, from which she had a very tedious recovery of several months. During this period she lost the natural power of her eye-lids, was under the necessity of keeping them open with the fingers of one hand, when she had any thing to do with the other, or went out, or wanted to look about her; in every other respect she was in health and tolerable spirits, only that she never had the least appearance of the menses, but periodically spit up blood in pretty large quantities, and at the same time it flowed from the nose. This vicarious discharge happened regularly every month for several years. About 5 years before, a little previous to which time the abovementioned periodical discharge had disappeared, she had a short 3d epileptic fit, which was immediately succeeded by a fever of about a week's continuance, and of which she recovered so slowly, that she did not go out of doors till 6 weeks after the crisis; when, without the knowledge of the family, she stole out of the house, and bound the corn of a ridge before they observed her. On that same evening she took to her bed, complaining much of her heart and head; and afterwards she never rose out of it except when lifted, seldom spoke a word, and had so little craving for food, that at first her parents could only by compulsion get her to take as much as would support a sucking infant: afterwards she gradually fell off from taking even that small quantity; so that, at Whitsuntide 1763, she totally refused food and drink, and her jaw became so fast locked, that it was with the greatest difficulty her father was able with a knife or other methods to open her teeth so as to admit a little thin gruel or whey, and of which so much generally ran out at the corners of her mouth, that they could not be sensible that any of it had been swallowed.

About this time, they got a bottle of the water from a medicinal spring in Brea-mar, of which they endeavoured to get her to swallow a part, by pouring some out of a spoon between her lips, her jaws all the while fast locked, but it

all ran out. With this however they rubbed her throat and jaws, and continued the trial to make her swallow, rubbing her throat with the water that ran out of her mouth for 3 mornings together. On the 3d morning, during this operation, she cried, "give me more water;" when all that remained of the bottle was given her, which she swallowed with ease. These were the only words she spoke for almost a year, and she continued to mutter some more for 12 or 14 days, after which she did not speak, and rejected, as formerly, all sorts of nourishment and drink, till July 1765, when a sister of hers thought, by some signs she made, that she wanted her jaws opened; which her father effected by putting the handle of a horn-spoon between her teeth. She said then intelligibly, "give me a drink;" and drank at one draught about a pint of water. Her father then asked her, "why she would not make some signs, though she could not speak, when she wanted a drink?" She answered, "why should she when she had no desire?" At this period they kept the jaws asunder with a bit of wood, imagining she got her speech by her jaws being opened, and continued them thus wedged about 20 days, though in the first 4 or 5 days she had wholly lost the power of utterance. At last they removed the wedge, as it made her lips sore. At this time she was sensible of every thing done or said about her; and when her eye-lids were opened for her, she knew every body; and when the neighbours in their visits lamented her condition, they could observe a tear stand in her eye.

In some of the attempts to open her jaws, 2 of the under fore-teeth were forced out; by which opening they often put some thin nourishing drink into her mouth, but without effect, for it always returned by the corners; and about a twelvemonth before (1766) they thought of thrusting a little dough of oatmeal through this gap of the teeth, which she would retain a few seconds, and then return with something like a straining to vomit, without one particle going down; nor had the family been sensible of any appearance like that of swallowing, for the last 4 years, excepting the small draught of Brea-mar water and the English pint of common water; and for the last 3 years she had not had any evacuation by stool or urine, except that, once or twice a week, she had passed a few drops of urine, but little as it was it gave her some uneasiness till she voided it: for they knew all her motions, and when they saw her thus uneasy, they carried her to the door of the house, where she made these few drops. Nor had they, in all these 3 years, ever discovered the smallest wetting in her bed; in proof of which, notwithstanding her being so long bed-ridden, there had never been the least excoriation, though she never attempted to turn herself, or made any motion with hand, head, or foot, but lay like a log of wood. Her pulse, which with some difficulty Dr. M. felt, was distinct and regular, slow, and to the extremest degree small. Her countenance was clear and pretty fresh, her features not disfigured nor sunk; her skin felt natural both as to touch and warmth; and

her breasts were round and prominent, like those of a healthy young woman; her legs, arms, and thighs, not at all emaciated; the abdomen somewhat tumid, and the muscles tense; her knees bent, and her ham-strings tight as a bow-string; her heels almost close to the nates. When they struggled with her, to put a little water within her lips, they observed sometimes a dewy softness on her skin; she slept much, and very quietly; but when awake kept a constant whimpering like a new-born weakly infant, and sometimes made an effort to cough. At the date of this account (June 1767) no degree of strength could force open her jaws. Dr. M. put the point of his little finger into the gap in her teeth, and found the tongue, as far as he could reach, soft and moist; as he did with his other fingers the mouth and cheeks quite to the back teeth. She never could remain a moment on her back, but always fell to one side or the other; and when her mother sat behind her in the bed, and supported her while he was examining her body, her head hung down, with her chin close to her breast, nor could he with any force move it backwards, the anterior muscles of the neck being rigid, like a person in the *emprosthotonos*, and in this posture she constantly lay.

The above case was taken in writing, at the diseased woman's bed side, from the mouths of her father and mother, who were known to be people of veracity, and under no temptation to deceive: their daughter's situation was a great mortification to them, and universally known and regretted by all their neighbours. Dr. M. had along with him for interpreters, as the family spoke only Erse, Mr. Henry Robertson, son to the minister of the parish, and David Ross, their neighbour, and one of the elders of the parish, who verified from his own knowledge all that is above related. The situation and appearances of the patient were carefully examined the 21st of October, 1767, by Dr. M. who likewise in October, 1772, being informed that the patient was recovering, visited her, and found her condition to be as follows; about a year preceding this last date, her parents one day returning from their country labours (having left their daughter as for some years before fixed to her bed) were greatly surprized to find her sitting on her hams, on the side of the house opposite to her bed place, spinning with her mother's distaff. Dr. M. asked, whether she ever ate or drank? whether she had any of the natural evacuations? whether she ever spoke or attempted to speak? And was answered, that she sometimes crumbled a bit of oat or barley cake in the palm of her hand, as if to feed a chicken; that she put little crumbs of this into the gap of her teeth, rolled them about for some time in her mouth, and then sucked out of the palm of her hand a little water, whey, or milk; and this, once or twice a day, and even that by compulsion: that the *egesta* were in proportion to the *ingesta*; that she never attempted to speak; that her jaws were still fast locked, her hamstrings tight as before, and her eyes shut. On

opening her eyelids Dr. M. found the eye-balls turned up under the edge of the os frontis, her countenance ghastly, her complexion pale, her skin shrivelled and dry, and her whole person rather emaciated; her pulse with the utmost difficulty to be felt. She seemed sensible and tractable in every thing, except in taking food; for, at his request, she went through her different exercises, spinning on the distaff, and crawling about on her hams; by the wall of the house, with the help of her hands; but when desired to eat, she showed the greatest reluctance, and indeed cried before she yielded; and this was no more than, as he had said, to take a few crumbs as to feed a bird, and to suck half a spoonful of milk from the palm of her hand. On the whole, her existence was little less wonderful at this time than when he first saw her, when she had not swallowed the smallest particle of food for years together. He attributed her thinness and wan complexion, the great change of her looks from what he had first seen when fixed to her bed, to her exhausting too much of the saliva by spinning flax on the distaff, and therefore recommended her being totally confined to spinning wool: this she did with equal dexterity. The above was her situation in October 1772; and in March 1775, Dr. M. was told by a neighbour of her father's, that she had still continued in the same way, without any addition to her support, and without any additional ailment.

At Croick, the 15th day of June, 1775.—"To authenticate the history set forth in the preceding pages, Donald M'Leod, of Granics, Esq., sheriff depute of Rosshire, George Munro, Esq., of Cuteain, Simon Ross, Esq., of Gladfield, Captain George Sutherland, of Elphin, all justices of the peace; Messrs. William Smith, preacher of the gospel, John Barclay, writer, in Tain, Hugh Ross, student of divinity, and Alexander M'Leod, came to this place, accompanied by the above Dr. Alexander Mackenzie, physician at New Tarbat, and after explaining the purport and meaning of the above history to Donald M'Leod, father to Janet M'Leod abovementioned, and to David Ross, elder in the parish of Kincardin, who was one of the doctor's original interpreters; they, to our full satisfaction, after a minute examination, authenticated all the facts set forth in the above account: and, for our further satisfaction, we had Janet M'Leod brought out before us to the open air, when the doctor discovered a very great improvement in her looks and health since the period of his having seen her last, as now she walked tolerably upright, with a little hold by the wall. And notwithstanding her age, which on inquiry we found to be exactly as set forth in the above account, her countenance and looks would have denoted her not to be above 20 years of age at most. At present, the quantity of food she uses is not above what would be necessary for the sustenance of an infant of 2 years of age. And we do report, from our knowledge of the above men, and the circumstances of the case, that full faith and credit is to be given to every article of the above history.

<i>William Smith.</i>	<i>Alexander M'Leod.</i>	<i>Simon Ross, J. P.</i>
<i>John Barclay, N. P.</i>	<i>D. M'Leod, Sh. Dep.</i>	<i>Geo. Sutherland, J. P."</i>
<i>Hugh Ross.</i>	<i>Geo. Munro, J. P.</i>	

II. On the Usefulness of Washing and Rubbing the Stems of Trees, to promote their Annual Increase. In an Extract of a Letter from Mr. Marsham to the Lord Bishop of Bath and Wells. p. 12.

I had for several years intended to put in practice Dr. Hales's advice of washing, with that of Mr. Evelyn of rubbing the stem of a tree, in order to increase its growth; but other avocations prevented me till the last spring: when, as soon as the buds began to swell, I washed my tree round from the ground to the beginning of the head, viz. between 13 and 14 feet in height. This was done first with water and a stiff shoe-brush, till the tree was quite cleared of the moss and dirt; then I only washed it with a coarse flannel. I repeated the washing 3, 4, or 5 times a week, during all the dry time of the spring and the fore-part of the summer; but after the rains were frequent, I very seldom washed. The unwashed tree, whose growth I proposed to compare with it, was, at 5 feet from the ground, before the last year's increase, 3 ft. 7 in. $\frac{9}{10}$; and in the autumn, after the year's growth was completed, 3 ft. 9 in. $\frac{1}{10}$; viz. increase 1 in. $\frac{9}{10}$. The washed tree was last spring 3 ft. 7 in. $\frac{9}{10}$, and in the autumn it was 3 ft. 9 in. $\frac{7}{10}$; viz. increase 2 in. $\frac{5}{10}$, that is, $\frac{1}{10}$ of an inch above double the increase of the unwashed tree. As the difference was so great, and as some unknown accident might have injured the growth of the unwashed tree, I added the year's increase of 5 other beeches of the same age, being all that I had measured, and found the aggregate increase of the 6 unwashed beeches to be 9 in. $\frac{3}{10}$, which, divided by 6, gives 1 in. $\frac{1}{10}$ and a half, for the growth of each tree; so the gain by washing is $\frac{9}{10}$ and a half. To make the experiment fairly, I fixed on 2 of my largest beeches, sown in 1741, and transplanted into a grove in 1749. The washed tree had been, from the first year, the largest plant till the year 1767, when its rival became and continued the largest plant, till I began to wash the other: I therefore fixed on the less thriving tree as the fairest trial. The trees were nearly of the same height and shape, spreading a circle of about 50 feet diameter. I think it necessary to mention these circumstances; for I know by experience, that a short and spreading tree, having ample room, will increase 2 or 3 times, and perhaps 4 times as much, as a tall small-headed tree of the same age, that stands near other trees. Thus my washed beech increased above 6 times as much as Mr. Drake's beautiful beech at Shardeloes, though that tree seemed in good health when I saw it in 1759 and 1766. But it increased only 2 in. $\frac{9}{10}$ in those 7 years; which may perhaps be owing to its vast height, being $74\frac{1}{2}$ feet to the boughs, only 6 feet 4 inches round, and having a small head, and little room to spread.

III. Discoveries on the Sex of Bees, explaining the Manner in which their Species is propagated; with an Account of the Utility that may be derived from those Discoveries by the actual Application of them to Practice. By Mr. John Debraw, Apothecary to Addenbrook's Hospital, Cambridge. p. 15.
Mr. Debraw, from various experiments detailed in this paper, is of opinion

that no real junction of sexes takes place in bees, but that the eggs which are laid by the queen bee are impregnated by a deposition from the males or drones, in the same manner as the ova of fishes are supposed to be impregnated. The drones which Mr. D. observed thus impregnating the eggs laid in the respective cells by the queen, were of the small kind, or such as do not exceed in size the working or common bees, and which are often mistaken for them by persons who are not aware that there are drones of different sizes in a hive. Mr. D. is convinced that the working or common bees have the rudiments of the female sex in them, and that they possess the power of forming a queen from any one of the maggots or larvæ by feeding it with a more plentiful nourishment, so as to develope the necessary organs. This Mr. D. has ascertained by placing a piece of brood-comb, in which were contained eggs, worms, and chrysalises, with food, viz. honey, into separate glass hives, and confining in each a sufficient number of common bees, with some drones, but taking good care that there should be no queen. The bees, finding themselves without a queen, made a strange buzzing noise for about 2 days, at the end of which time they settled, and betook themselves to work. On the fourth day he perceived in each of those hives the beginnings of a royal cell; a certain indication that one of the inclosed worms would soon be converted into a queen. The construction of the royal cell being nearly accomplished, he ventured to leave an opening for the bees to get out, and found that they returned as regularly as they do in common hives, showing no inclination to desert their habitation; and at the end of twenty days he found four young queens among the new progeny. These experiments were repeated several times, in order to remove an objection made to the theory of the bees having the power of raising a common bee-worm into a queen, viz. that the queen bee of a hive, besides the eggs which she deposits in the royal cells, might also have laid royal or female eggs either in the common cells, or indiscriminately throughout the different parts of a hive; and that in the pieces of brood-comb which had been successfully employed in the experiments for the production of a queen, it had constantly happened that one or more of these royal eggs, or rather worms, proceeding from them, had been contained. But the force of this objection was removed by subsequent experiments, and those who objected to the theory were at length obliged to admit, that every common or working-bee-worm is capable, under certain circumstances, of becoming the mother of a generation; and that it owes its metamorphosis into a queen partly to the extraordinary size of the cell, and its particular position in it; but principally to a certain nourishment appropriated to the occasion, and carefully administered to it by the working-bees while in its worm state; and that it appears evident, from the experiments of Mr. Debray, as well as from those of Mr. Schirach, that the received opinion of the queen

bees laying a particular kind of eggs appropriated to the production of other queens, is erroneous.

The great advantage accruing from these experiments is that of forming artificial swarms or new colonies at pleasure, and thus increasing the propagation of the insect for the advantage of commerce and the arts.

The practice, according to Mr. Schirach, has already extended itself through many parts of Germany, and even of Poland. Mr. Debraw's experiments were begun nearly two years before Mr. Schirach's work had appeared.

IV. Account of a Portrait of Copernicus, presented to the Royal Society by Dr. Wolf, of Dantzick: extracted from a Letter of his to Mr. Magellan, F. R. S. Translated from the French. p. 33.

The captain who will deliver this to you, will also put into your hands a copy of an original portrait of the famous Copernicus, which I beg you will present to the R. S., as a testimony of my devotion and attachment to that respectable body. The original, from which it is copied with the greatest accuracy, is in the possession of the Chamberlain Hussarzewski. We have a portrait of Copernicus in the great church at Thorn in a kind of mausoleum, erected about 30 years after the death of that great man, by a physician of that town, who is said to have been one of his relations.

Hartknoch has inserted a print taken from this portrait in his *Chronicles of Prussia*. Our original has been compared with that of the mausoleum, and the features of the face are found to be perfectly similar, but there is a great difference in the dress. That at Thorn represents him kneeling before an altar, in the attitude of a priest officiating; in ours he is clothed in fur, with his hair more carefully dressed, and as it were in a habit of ceremony. The painter of it was certainly one of the old Italians, as will appear by comparing it with other works of those masters; for instance, it is known that the painters of those times, and even Raphael, never gave to the eyes that brightness which the most indifferent artists within this century never fail to express in their portraits: not but what the serene and seemingly inanimated countenances of the former artists came nearer to nature than the sparkling eyes which are now all the fashion. This however is a proof that the portrait is at least 150 years old; the inscription shows that the painter was an Italian: and it must further be observed, that it is now 2 centuries since they left off painting on wood.

The history of this portrait is as follows: it was formerly in the collection of Saxe Gotha, where it was always considered as an original, which is even said to appear from the archives of that court, and is the more probable, as the prince-bishop of Warmia, who obtained it from the late duke of Saxe Gotha, was too good a connoisseur and too cautious to be deceived in this respect. That

bishop being at Gotha in the year 1735, observed this portrait in the gallery of that palace; the proofs that were produced of its authenticity made him very desirous to acquire it. He at length obtained it by a kind of theft which it was necessary to commit on the cathedral of Warmia, in which there was a very old portrait of one of the ancestors of the dukes of Saxe Gotha, who had been bishop of that see, and whose picture was wanting in the duke's collection of the portraits of his family. An exchange was accordingly made of the two originals, and the bishop has since bequeathed that of Copernicus to his favourite Mr. Hussarzewski.

V. Description of a New Species of Cuckoo. By Dr. Andrew Sparman, of the Royal Academy of Stockholm. p. 38.

*History of the Honey-Guide, or Cuculus Indicator.**—This curious species of cuckoo is found at a considerable distance from the Cape of Good Hope, in the interior parts of Africa, being entirely unknown at that settlement.† The first place where Dr. S. heard of it was in a wood, called the Groot Vaader's Bosch, the Grand Father's Wood, situated in a desert near the river which the Hottentots call T'kaut'kai. The Dutch settlers there have given this bird the name of honig-wyzer, or honey-guide, from its quality of discovering wild-honey to travellers. Its colour has nothing striking or beautiful; and its size is considerably smaller than that of our cuckoo in Europe: but in return, the instinct which prompts it to seek its food in a singular manner, is truly admirable. Not only the Dutch and Hottentots, but likewise a species of quadruped, which the Dutch name a ratel, are frequently conducted to wild bee-hives by this bird, which as it were pilots them to the very spot. The honey being its favourite food, its own interest prompts it to be instrumental in robbing the hive, as some scraps are commonly left for its support. The morning and evening are its times of feeding, and it is then heard calling in a shrill tone cherr, cherr, which the honey-hunters carefully attend to as the summons to the chace. From time to time they answer with a soft whistle, which the bird hearing, always continues its note. As soon as they are in sight of each other, the bird gradually flutters towards the place where the hive is situated, continually repeating its former call of cherr, cherr. And if it should happen to have gained a considerable way before the men, who may easily be hindered in the pursuit by bushes, rivers, and the like, it returns to them again, and redoubles its note, as if to reproach them with their inactivity. At last the bird is observed to hover for a few moments over a certain spot, and then silently

* *Cuculus cauda cuneiformi, corpore ferrugineo-griseo subtus albido, humeris macula flava, rectricibus tribus exterioribus basi macula nigra.*—*Lath. Ind. Orn.*

† It is also a native of the island of Ceylon.

retiring to a neighbouring bush or other resting-place, the hunters are sure of finding the bees nest in that identical spot, whether it be in a tree, or in the crevice of a rock, or, as is most commonly the case, in the earth. While the hunters are busy in taking the honey, the bird is seen looking on attentively to what is going forward, and waiting for its share of the spoil. The bee-hunters never fail to leave a small portion for their conductor, but commonly take care not to leave so much as would satisfy its hunger. The bird's appetite being only whetted by this parsimony, it is obliged to commit a second treason, by discovering another bees nest, in hopes of a better salary. It is further observed, that the nearer the bird approaches the hidden hive, the more frequently it repeats its call, and seems more impatient.

While in the interior parts of Africa, a nest was shown to Dr. S., which some peasants assured him was the nest of a honey-guide. It was woven of slender filaments or fibres of bark, in the form of a bottle. The neck and opening hung downwards, and a string in an arched shape was suspended across the opening, fastened by the two ends, perhaps for the bird to perch on.*

VI. An Account of some new Electrical Experiments. By Mr. Tiberius Cavallo. p. 48.

This paper may be consulted in the 2d vol. of Mr. Cavallo's Electricity, the 3d edition, 1786.

VII. A Third Essay on Sea Anemonies. By the Abbé Dicquemare. p. 56.

In this paper the Abbé Dicquemare observes, that sea anemonies, having their bases unequally extended upon and fixed to the hard substance of rocks, &c. do often contract, and thus tear off and leave on the above hard substance one or more small shreds of their bases, covered with pieces of the old coat of the animal, and that these shreds soon after become small anemonies. This singular circumstance he has often had occasion to observe, having been very attentive and assiduous in observations of this nature. This process is completed in less than three months, when the small anemonies thus produced from shreds or fragments may be considered as perfect. After having observed the strips or small fragments thus naturally detached from the animals to be capable of growing into new and complete ones, the Abbé severed with the point of a knife several small pieces from the basis of several sea anemonies, at the places where their base seemed most extended while adhering to oyster-shells, &c. and putting

* In abridging this paper we have taken the liberty of substituting the specific character of the bird from Mr. Latham's Index Ornithologicus for the tedious, and now useless descriptive character detailed by Dr. Sparrman.

them separately into different vases, he found that, after a certain number of days, they fixed themselves, and in less than three months became furnished with limbs, &c.

In observing the progress of such fragments as had been left naturally, or from the bases of such anemonies as had moved from their place, he noticed, that some fragments produced several small anemonies, which sometimes have remained united for a considerable time, while others have soon separated. These experiments were principally made during the winter season.

N. B. In conducting such experiments it is necessary that the seawater the animals are kept in be clear, and frequently renewed.

VIII. Experiments and Observations in Electricity. By Mr. William Henley, F. R. S. p. 85.

The most useful parts of these observations may be consulted in Cavallo's *Electricity*, in 2 vols. 8vo.

IX. Extract of a Letter from Jno. Strange, Esq., His Majesty's Resident at Venice, to Sir John Pringle, Bart., P. R. S.: with a Letter to Mr. Strange from the Abbé Joseph Toaldo, Professor in the University of Padua, &c. giving an Account of the Tides in the Adriatic. p. 144. From the Latin, dated Patavi, Nov. 9, 1776.

Sig. Toaldo's account of these tides is here said to be grounded on the observations of a very accurate person at Venice, a Sig. Temanza, a celebrated architect and engineer. The first material observation is, that at Venice, it is high water at $10\frac{1}{4}$ hours after the moon's passage over the meridian, either above or below the horizon, on the day of the full or change. Then, as to the height of the tides, it is observed that, at new and full moon, they will rise 3 or $3\frac{1}{4}$ feet, seldom 4, and very seldom 6 or 7, and that only when the waters are urged by a south wind. But that about the quarters the rise of the tide is much less, being sometimes hardly 3 inches; but at a medium about 16 inches. That the tides in other parts of the Mediterranean are still less, being indeed hardly at all perceptible, except in straits and bays. That on the first turn of the tide, the rise is very slow and gradual for the first 3 or 4 hours, so as hardly to rise 3 inches per hour; but afterwards it rises with a very rapid and violent motion; then, after being still for about half an hour at the top, it descends again in a reverse manner.

It is observed that generally the tides are a little higher in winter than in summer. Thus, the medium height of all the tides in the 6 winter months, in the 5 years 1751, 2, 3, 4, 5, was 2 ft. $\frac{1}{4}$ an inch; and the medium of all the tides in the 6 summer months of the same years, was 1 ft. 9.7 inches. It fur-

ther appears, by the observations, that the greatest tide falls in December, about the winter stolstice, the least in the month of August, and the mediums in March and September. S. Toaldo adds a slight sketch of the course of the flux of the tide through the Mediterranean Sea; first entering by the Straits of Gibraltar, it sweeps along by the south side, the coasts of Africa and Egypt; then turning by the east it sweeps along the north side, passing by Syria, Greece, Italy, Liguria, France, and Spain, and thus issues into the Atlantic Ocean by the Gibraltar side of the Straits.

X. A Letter from Mr. Peter Wargentin, F. R. S., Sec. to the Acad. of Sciences at Stockholm, to the Rev. N. Maskelyne, B. D., F. R. S., &c. concerning the Difference of Longitude of the Royal Observatories at Paris and Greenwich, resulting from the Eclipses of Jupiter's first Satellite, observed during the last Ten Years. From the Latin. p. 162.

The manner of deriving the difference of longitude, or difference of time, between two places, by the observation of the eclipses of Jupiter's satellites, is well known; viz. the same eclipse, either immersion or emersion, being observed at different places; then, because the eclipse is seen the same moment at each place, the difference of the observed times must give the difference of longitude between the places, estimated at the rate of 15° to an hour of time. In this way, by comparing together a great number of corresponding observations of such eclipses, made at Stockholm, at Paris, and at Greenwich, Mr. Wargentin deduced the differences between the meridians of these 3 places, differing among themselves but by a few seconds only: then, by taking a medium among all the results of each sort, he concludes that he has obtained the true differences of longitude between the 3 places very nearly. Hence it is ultimately concluded, that the difference in time between Paris and Greenwich, is $9^m 25^s$, and between Stockholm and Greenwich $1^h 12^m 21^s$.

XI. A Method of finding the Value of an Infinite Series of Decreasing Quantities of a certain Form, when it converges too slowly to be summed in the Common Way by the mere Computation and Addition or Subtraction of some of its Initial Terms. By Francis Maseres, Esq. F. R. S., Cursitor Baron of the Exchequer. p. 187.

Let a, b, c, d, e, f, g, h , &c. ad infinitum, represent a decreasing progression of numbers; and let their differences, $a - b, b - c, c - d, d - e, e - f, f - g, g - h$, &c. also form a decreasing progression: and likewise, the differences of these differences, which may be called the 2d differences of the original numbers, form a decreasing progression; and the differences of those 2d differences, or the 3d differences of the original numbers also form a decreasing progression;

and in like manner, the differences of the said 3d differences, or the 4th differences, of the original numbers; and the 5th and 6th differences, and all higher differences, of the same numbers, also form decreasing progressions. Also let x be a quantity of any magnitude not greater than unity. Then on these suppositions the value of the infinite series $a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - hx^7 + \&c.$ will be equal to the following differential series, viz.

$a - \frac{bx}{1+x} - \frac{D'xx}{(1+x)^2} - \frac{D''x^3}{(1+x)^3} - \frac{D'''x^4}{(1+x)^4} - \frac{D^{iv}x^5}{(1+x)^5} - \frac{D^vx^6}{(1+x)^6} - \&c.$; where $D' = b - c$, the 1st of the 1st order of differences;

$D'' = b - 2c + d$, the 1st of the 2d order of differences;

$D''' = b - 3c + 3d - e$, the 1st of the 3d order of differences;

$D^{iv} = b - 4c + 6d - 4e + f$, the 1st of the 4th order of differences; and so on.

This theorem is the same, in substance, as the 7th prop. of Stirling's *Methodus Differentialis*, first published in the year 1730; and it is the same, both in form and substance, as corol. 4, pa. 65, of Simpson's *Dissertations*, published in 1743. Mr. Maseres has not given the investigation of this series; but it is given by Stirling, and still in a way much neater and easier by Simpson; by both of whom also the method is applied to a series, the terms of which have all the same sign, $+$ or $-$, as well as to the case above given, where the terms have these signs alternately.

Mr. Maseres remarks that the foregoing differential series will always converge with a considerable degree of swiftness, so that 6 or 8 of its terms will give the value of the whole, and consequently that of the original series to which it is equal, exact to several places of figures, even in the most difficult cases: for if x be $= 1$, which is its greatest possible magnitude, $1 + x$ will be $= 1 + 1$ or 2, and consequently $(1 + x)^2$, $(1 + x)^3$, $(1 + x)^4$, $(1 + x)^5$, and the following powers of $1 + x$, will be equal to 4, 8, 16, 32, and the following powers of 2; and the powers of the fraction $\frac{x}{1+x}$ will be equal to the powers

of $\frac{1}{2}$. Therefore the series $a - \frac{bx}{1+x} - \frac{D'xx}{(1+x)^2} - \frac{D''x^3}{(1+x)^3} - \frac{D'''x^4}{(1+x)^4} - \frac{D^{iv}x^5}{(1+x)^5} - \&c.$ will in this case be $=$ to $a - \frac{b}{2} - \frac{D'}{4} - \frac{D''}{8} - \frac{D'''}{16} - \frac{D^{iv}}{32} - \frac{D^v}{64} - \&c.$ the terms of which decrease in a greater proportion than that of 1 to 2, because the numerators $a, b, D', D'', D''', D^{iv}, D^v, \&c.$ form a decreasing progression, and the denominators increase in the proportion of 2 to 1.

Mr. M. gives some examples of the use of this theorem, in the numeral calculation of the series for the length of a circular arc that is expressed by a series in terms of its tangent; and also of the series expressing the time of a body descending by gravity down a circular arc. Which examples it would be useless to repeat in this place.

XII. Translation of a Passage in Ebn Younes; with some Remarks on it. By the Rev. George Costard, M. A., Vicar of Twickenham. p. 231.

Having, by means of the R. S., been favoured with a transcript of the Arabic passage in manuscript of Ebn Younes, in the library at Leyden, I now send as exact a translation of it as I can. I give it in Latin, as the former translations of it were in that language, and as the numbers in the ms. by no means agree with calculations made by modern tables, I have ventured to suppose that they have been somehow or other altered from what they were in the original tables of Ebn Younes. I have likewise ventured to suppose that the present Leyden copy is a transcript of another copy, which is no very violent supposition, considering how long ago these observations have been made, and how long it is since Ebn Younes wrote. I have likewise made no scruple to suppose that, however distinct and elegant both the Arabic letters and figures are in later manuscripts, they were not so in those of a more ancient date, so that one might easily be mistaken for another, where there is a similarity; and this mistake would be the more easily committed by a person ignorant of the subject. This probably was the case of all such as were hired by booksellers to transcribe manuscripts for sale; and for this reason, when the transcriber had made any mistake, he would not blot it out for fear of spoiling the sale of his book. There is an instance of this sort in this very manuscript in the observations of the 3d eclipse, which is that of the moon, as may be seen in the transcript and translation sent last year by Mr. Schultens.

If what has been said be allowed me, as I hope it will not be thought too much, I think I shall be able to account possibly, if not probably, for the differences between the observations as set down in the manuscript, and the result of the calculations by modern tables: a thing which has not been hitherto attempted, as few who have been versed in astronomy have been acquainted with the Arabic language; and those, on the other hand, who have well understood Arabic, have been as little conversant with astronomy.

What I have now advanced shall be exemplified under the first eclipse, which is one of the sun. In this eclipse, according to the manuscript, at the beginning, the sun's altitude was more than 15 (ا١٥) degrees, and less than 16 (ا١٦); and at the end it was more than 33 degrees (ا٣٣) and $\frac{1}{3}$. But I make the sun's height at the beginning 30 (ا٣٠) degrees, and at the end nearly 36 (ا٣٦). In the manuscript, the digits eclipsed are said to have been 8 (٨, or ٧, as it is sometimes written); but I make them only a little more than 4 (ا٤), or about $4\frac{1}{2}$.

Whether the notation in the original manuscript of Ebn Younes was in letters or arithmetical figures is uncertain; but most probably it was in the former, as it is in most of the tables now extant, though composed since the admission and use of arithmetical figures. On this supposition then, or that they were so in

the manuscript from which the present manuscript was copied, we shall very naturally account for the mistakes we find in it. Thus, for instance, \downarrow by some accidental stroke at the bottom, would easily be taken for \smile , as \downarrow is sometimes written in manuscripts; and if the perpendicular stroke in the \downarrow was made short, as in a table it very well might be, \downarrow (30) would naturally be taken for \smile or \searrow (15); and, by the same rule, \downarrow (36) would very easily be taken for \searrow (16); and Δ (4), the digits eclipsed, for \vee which is 8 in the other form of notation, or \searrow in this. In the manuscript it is said, that the sun's altitude at the end by observation, was a little more than 33 (\searrow) degrees; but this would, in a manuscript ill written, easily be mistaken for \smile (35) or \downarrow (36).

As to the words, translated by Professor Schultens for Mr. Grischow, "accidit hoc in plano circuli ejus minus quam 7 digiti," I am apt to suspect they are nothing more than some marginal reading crept into the text; that is, somebody seeing the digits eclipsed here made 8 (\searrow), added, as the Arabic will very well bear, "imo minus quam \searrow (7) or \vee (7)," as in the other form of notation that figure is sometimes made. The writer of this manuscript, whoever he was, was certainly acquainted with both forms of notation, as he has made use of both. This interpretation is at least plausible, and clears up a sentence which greatly perplexed both Mr. Grischow and Dr. Bevis, and seemed to them quite unintelligible.

The account given by Curtius of the 2d eclipse, which was a solar one, is this: Anno eodem, die Sabbati, videlicet, 29 mensis Sywal, (numero decimi, qui Paschalis est eorum) eclipsis solis occupavit digitos $7\frac{1}{4}$. In principio, sol altus ferè 56° . In fine, sol occiduus elevabatur gradibus 26. Ex Shickardo in ms. This it is plain is not a translation of the Arabic; for that, as translated by Schultens for Mr. Grischow, and transmitted by him to Dr. Bevis, is much fuller, and is as follows:

Eclipsis Solaris.—Hæc eclipsis extitit die Sabbati, 29 mensis Siewal, anno 367 Hegiræ. Et dies Sabbati hicce ipse est dies 9 mensis Chordadma, anni 348 Jesdagirdis, et ipse 8 mensis Haziran anni 1289 Alexandri, et ipse est 14 mensis Buna, anni Dioclesiani. Fuitque maximum quod eclipsatum est de diametro solis, $5\frac{1}{4}$ digiti super calculo accuratiore. Erantque de plano circuli ejus 4 digiti et 10 minuta. Et erat elevatio solis, tempore quo eclipsis incepit, secundum oculum 56° circiter; et erat integra ejus reapparitio cum esset elevatio ejus 26° graduum, aut circiter; erantque sol et luna simul in hac eclipsi, in propinquo distantie maximæ a terrâ. Deus scit an calculus hic benè sit positus. Tempus respondet diei 8 Jun. an. Christi 978.

Thus far Mr. Schultens. And here I must observe that, according to him as well as Curtius, the sun's altitude at the beginning was about 56° , or in Arabic notation \searrow ; but by computation I make it only about $47^\circ 50'$. Suppose it were

47 (مر); then where the letters are small and ill made, و and ز may easily be mistaken for each other. The sun's altitude at the end of this eclipse, according to both Curtius and Schultens, was 26° (ز); but by calculation I make it a little more than 36° (و). But these figures are so nearly alike that they would easily be mistaken by an ignorant transcriber, and from a manuscript that was ill written.

How Schickard or Curtius for him, came to make the digits eclipsed $7\frac{1}{2}$ I know not: for in the manuscript, as translated by Schultens for Mr. Grischow above, we see they were only $5\frac{1}{2}$ and that *super calculo accuratiore*, or as the Arabic should have been translated, *juxta calculum accuratiorem*. The meaning of which, I suppose is, that Ebn Younes had found by calculation that the digits eclipsed would be $5\frac{1}{2}$, and that at the time his calculation agreed with his observation; as indeed it did, for I make them about $5\frac{3}{4}$, however widely this differs from $7\frac{1}{2}$ as in Curtius.

When the altitude of the sun, at the beginning of this eclipse, is said to have been 56° or nearly, *secundum oculum*, it is evident that this was an observation. When it is added, *erantque de plano circuli ejus 4 digiti et 10 minuta*, in words at length, it seems to have been some interpolation or marginal reading, crept into the text, as another seems to have done under the former eclipse; for if the digits eclipsed here were $5\frac{1}{2}$, agreeable both to observation and accurate calculation, they must certainly have been more than $4^\circ 10'$.

At the conclusion of the former eclipse it was added in the translation, *Deus scit an observatio sit benè instituta*; and here the passage, as translated, concludes with *Deus scit an calculus hic benè sit positus*. But in the Arabic, as I have received it, there is no mention made either of observation or calculation. The words are the same in both passages, and are only *adjuvante Deo*. The other translations seem only to have been what Mr. Grischow collected from professor Schultens, who, he says, was totally ignorant of astronomical language, as he himself was ignorant of Arabic.

The 3d is a lunar eclipse; and the account given of it by Curtius, from Schickard, is this: Anno Christi 979. Anno Hegiræ 368 (qui incipit d. 8 Aug. mihi die 9 Aug. anno Chistiano 978) die Jovis, 14 Sywal, luna fuit orta cum defectu, qui ad $5\frac{1}{2}$ digitos accrevit, cum extaret supra horizontem gradibus etiam 26 (subaudio finem tunc accidisse). Schickardus. Qui adjungit, tempus respondere diei 14 Maii, anno Christi 979. The account of this eclipse, as translated by professor Schultens for Mr. Grischow, is more particular and intelligible, viz. Eclipsis lunæ extitit in mense Sieval (sive Xaval) anno 368 Hegiræ. Orta est luna eclipsata, in nocte cujus aurora fuit feria quinta. Et hæc feria quinta fuit dies 25 mensis Ijar, anni 1290 Alexandri, et ille 20 mensis Baschner (sive Pachon) anni 695 Dioclesiani. Spatium quod eclipsatum fuit de diametro ejus,

fuit amplius quam octo digiti, et minus quam novem. Fuitque hora ortûs ejus proxima horæ oppositionis, secundum fundamenta quibus computare soleo. Et perfecta est ejus reapparitio (sive finis) cum præteriisset de nocte (i. e. post occasum solis) circiter hora justa, et quinta horæ pars, prout observavi. Et erat luna, in hac eclipsi, in propinquo distantia suæ mediæ. Tempus respondet diei 14 Maii, anno Christi 979.

With regard to the time of the opposition, and the moon's rising at Cairo, there is very little difficulty; for she rose there at $6^h 48^m 10^s$, and the time of opposition was at $6^h 24^m 36^s$. The end of this eclipse there was at $7^h 54^m 26^s$, and the time of sun-set was at $6^h 47^m 52^s$. The difference is $1^h 6^m 16^s$, and agrees very well with the manuscript. The passage, as we have it here in Curtius from Schickard, is very obscure. For it seems either to mean that when the digits eclipsed were $5\frac{1}{2}$, the moon was 26° high, or that she was 26° high when the eclipse ended. But I take the last to be intended; for the moon was 26° high at $7^h 36^m$, and the eclipse ended, as we saw, at $7^h 54^m 26^s$.

But when Schickard or Curtius say this defectus ad $5\frac{1}{2}$ digitos accrevit, the meaning must be that they amounted only to $5\frac{1}{2}$. But this is not true; for according to the manuscript, they were between 8 and 9, and I make them about $8\frac{3}{4}$. I am apt to suspect therefore that the transcriber, whoever he was, cast his eyes on the solar eclipse above, where the digits eclipsed are really $5\frac{1}{2}$, and carelessly set them down to this lunar eclipse where they do not belong. And to confirm this conjecture, it must be observed, that after the word Dioclesian, under this lunar eclipse, in the Arabic follow six lines, which are a repetition of all that was said under the last solar eclipse, from the same word Dioclesian to the end of that observation.

I shall now, in the last place, give a translation of the Arabic passage entire; omitting however the interpolations mentioned above, which embarrass the whole.—Infit ALI IBN ABDORRAHMAN, IBN ACHMED, IBN YOUNES, IBN ABDOL AALI.—Imprimis, jam commemoravi eclipses, tam solares quam lunares, quas observârunt viri docti; eruditi ii quorum nomina recensui, quasque ad eos retuli, incipiendo ab auctoribus libri dicti ALMOMTAHEN, usque ad filios Majour; quin et conjunctiones eorum cum stellis fixis, quas observârunt, et quorum loca commemorârunt, et invenerunt, tempore conjunctionum eorum.—Ipse deinde memorabo eclipses quas observavi, tam solares quam lunares, et conjunctiones cum stellis fixis, et quænam fuerunt formæ eorum in conjunctionibus suis. Ut quicunque me sequantur, et indicia habere desiderent, meis utantur, quemadmodum ac ego eorum indiciis et directionibus usus sum, qui ante me observârunt. Deus autem adjutor est.—Eclipsis solaris erat priore parte diei, feriâ quinta, die decimo octavo mensis Rabiæ posterioris, anno Hegiræ 367. Et hæc feria quinta erat dies decimus secundus mensis Adzermah, anno Yezdagerdis 346.—Caraffæ

adfuimus, in templo ABI GAAFARI ACHMED IBN NASAR Africani, cœtus eruditorum, ad hanc eclipsin observandam. E quorum numero erat HAROUN IBN MOHAMMED AL GAAFARI, et ABU ABDALLAH AL HOSEIN IBN NASAR Africanus, et ABUL' HOSEIN ALI IBN MAHARBACHT Persa, et ABUL' ABAS ACHMED IBN ACHMED AL CHURGII, et ABU ACHMED ASSUMACHI, et ABU OMAR Scriba.—Ex his, præter alios eruditos cum reliquis observatoribus, nonnulli erant astronomicè docti.—Ipse quoque eodem contendi, unâ cum ABUL' KASEM ABDORRAHMAN IBN HOSEIN, IBN TISAN, AL IDAS, et HOSAN IBN AL DARANI, et HAMED IBN AL HOSEIN.—Et hi omnes initium hujus eclipseos observârunt, quæ, ad sensum meum, apparere incepit sole plus quam gradibus 15, minus autem quam 16 elevato.—Omnes quoque præsentis opinione consentierunt obscurari de diametro ejus circiter 8 digitos.—Et splendor ejus perfectè recuperatus est cum elevaretur amplius quam gradibus 33 cum tertiâ ferè parte, prout ipse mensuravi; omnibus qui aderant consentientibus.—In hac eclipsi, sol et luna simul erant non longè a distantîâ suâ proximâ a terrâ. Adjuvante Deo.

Eclipsis Solaris.—Hæc eclipsis incidit in diem Sabbati, diem 29 mensis Shuwal, anno Hegiræ 367. Eratque hic dies Sabbati, dies 9 mensis Chordadmah, anno Yezdagerdis 347, et dies 8 mensis Hazirân, anno Alexandri 1289; et insuper dies 14 mensis Bounah anno Dioclesiani 694. Maximum quod obscuratum est de diametro solis erat $5\frac{1}{2}$ digiti.—Et quando hæc eclipsis, ad oculi aciem, jam incepisse constabat, solis altitudo erat circiter gradus 56, et lucis ejus restitutio completa est cum altitudo ejus esset 26 gradus, vel circiter.—Erantque sol et luna simul, in hæc eclipsi, propè distantias suas maximas a terrâ. Adjuvante Deo.

Eclipsis Lunar.—Hæc contigit mense Shuwal, anno Hegiræ 368. Oriebatur Luna, eclipsi jam inchoatâ, nocte cujus Aurora erat feria quinta, quæ feria quinta erat dies 28 mensis Ardbahest, anno Yezdagerdis 348, quæ fuit 18 mensis Ijar, anno æræ Alexandri 1290. Eratque dies 20 mensis Bishnis, anno Dioclesiani 698.—Eratque quantitas diametri ejus obscurata, plusquam digiti 8, et minus quam novem. Tempusque ortûs ejus erat propè tempus oppositionis juxta fundamenta quibus computavi: lucemque plenam recuperavit cum de nocte præteriisset hora circiter æquinoctialis, cum quintâ parte, prout ipse conjectavi.—Eratque luna in hac eclipsi, haud procul a distantîâ suâ mediâ a terrâ. Adjuvante Deo.

XIII. Observations on the Annual Evaporation at Liverpool in Lancashire; and on Evaporation considered as a Test of the Moisture or Dryness of the Atmosphere. By Dr. Dobson of Liverpool. p. 244.

The quantity of rain which falls during the course of the year, is a very uncertain test of the moisture or dryness of any particular season, situation, or

climate. There may be little or even no rain, and yet the air be constantly damp and foggy; or there may be heavy rains, with a comparatively dry state of the atmosphere. The same depth of rain will likewise produce different effects on the air, according as it falls on a flat or hilly country; for large quantities soon quit the hills or high grounds, while smaller quantities have more lasting and powerful effects on a flat country. Much also depends on the nature of the soil, whether clay or sand, whether firm and compact, or loose and spongy. Is not evaporation then a more accurate test of the moisture or dryness of the atmosphere, than the quantity of rain?

It is well known, that air is an active solvent of water, and that its powers of solution are in proportion to its dryness. It is also well known, that in chemical solutions, the action of the menstruum is greatly promoted by heat and agitation. If the temperature of the air then, and the state of the winds, be ascertained, which in the present case denote the heat and agitation of the menstruum, the evaporation will be the true index of the dryness of any particular season, situation, or climate.

To determine the annual evaporation in the neighbourhood of Liverpool, Dr. D. procured 2 well varnished tin vessels; one of which was to serve the purpose of a rain-gage; the other to be employed as an evaporating vessel. The evaporating vessel was cylindrical, 12 inches in diameter, and 6 inches deep. The rain-gage consisted of a funnel also 12 inches in diameter, the lower end of which was received into the mouth of a large stone bottle; and, to prevent any evaporation from the bottle, the pipe of the funnel was stopped with a grooved cork. These vessels were placed in the middle of a grass-plot, on a rising ground adjoining, and immediately overlooking the town, about 75 feet above the level of the sea, and with a free exposure to the sun, winds, and rain. The cylindrical vessel was filled with water within 2 inches of the top; and if, in consequence of heavy rains, there was danger of its overflowing, a quantity of water was taken out; but if, in consequence of long drought, it sunk lower, a quantity of water was then occasionally added; and these additions or subtractions were carefully registered. At the end of every month, the depth of rain was first calculated; and, as each vessel received the same depth of rain, he had only to examine the quantity of water which had been added to, or taken out of the evaporating vessel, and the evaporation of the month was ascertained.

The 1st column of the following tables points out the mean temperature of the air at 2 in the afternoon. The 2d, the character of the month with respect to the winds, the number of dots expressing their strength; and, to make this part tolerably accurate, daily observations on the winds were marked down, and the character of the month formed from a general survey of these observations: our winds are westerly for near two-thirds of the year. The 3d column points

out the evaporation of each month in inches and decimal parts of an inch. The 4th, the depth of rain during each month. And the 5th, the state of the seasons, E being prefixed to the evaporation of the whole 3 months, R to the rain, and T to the mean temperature. In making these experiments, 251 grains were allowed for every cubic inch of water; and 3 lb. 12 oz. of water give a depth of 1 inch on a circular area of 12 inches diameter.

TABLE I, for 1772.

TABLE II, for 1773.

TABLE III, for 1774.

TABLE IV, for 1775.

Mths.	T.	Wd.	Evap.	Rain.	Seasons.	T.	Wd.	Evap.	Rain.	Seasons.	T.	Wd.	Evap.	Rain.	Seasons.	T.	Wd.	Evap.	Rain.	Seasons.
Jan...	38	...	1.27	3.26	E. 4.87	44	1.85	3.15	R. 5.74	37	...	1.38	4.43	E. 5.92	44 $\frac{1}{2}$...	1.51	3.21	E. 7.10
Feb...	39	...	1.25	2.35	R. 7.23	42 $\frac{1}{2}$...	1.13	2.37	R. 6.17	45 $\frac{1}{2}$...	1.67	2.42	R. 8.23	49	...	3.02	4.62	R. 10.28
March	44	...	2.35	1.62	T. 40.	50	...	2.76	0.65	T. 45.	49 $\frac{1}{2}$...	2.87	1.38	T. 44.	48 $\frac{1}{2}$...	2.57	2.45	T. 47 $\frac{1}{3}$.
April	48	...	2.53	1.85	E. 11.40	54	...	2.89	2.47	E. 9.34	54 $\frac{1}{2}$...	4.56	2.23	E. 12.39	57 $\frac{1}{2}$...	3.21	1.01	E. 15.09
May..	57	...	4.25	3.42	R. 8.39	57	...	3.79	4.56	R. 8.35	59 $\frac{1}{2}$...	4.31	1.65	R. 7.14	61	...	5.02	0.85	R. 3.98
June..	67	...	4.62	3.12	T. 57.	64 $\frac{1}{2}$...	2.66	1.42	T. 58.	63	...	3.52	3.26	T. 59.	70 $\frac{1}{2}$...	6.86	2.12	T. 63.
July..	70	...	5.53	1.59	E. 13.20	67	...	4.92	1.32	E. 14.02	66 $\frac{2}{3}$...	4.97	2.68	E. 13.51	68 $\frac{1}{2}$...	5.03	5.31	E. 12.50
Aug...	68	...	5.35	3.65	R. 11.29	70	...	5.75	2.21	R. 10.08	67	...	4.52	2.36	R. 10.56	66 $\frac{1}{2}$...	4.42	4.26	R. 12.57
Sept..	62	...	2.32	6.05	T. 66.	60	...	3.35	6.55	T. 65.	61 $\frac{1}{3}$...	4.02	5.52	T. 65.	65	...	3.05	4.00	T. 56 $\frac{1}{3}$.
Oct...	60	...	3.18	3.42	E. 6.46	55	...	2.79	4.57	E. 5.49	57	...	1.95	1.68	E. 4.82	54 $\frac{1}{2}$...	2.12	7.01	E. 5.27
Nov...	50	...	2.15	4.85	R. 10.48	47 $\frac{1}{2}$...	1.15	6.69	R. 15.58	46 $\frac{1}{2}$...	1.12	2.69	R. 6.00	45	...	1.63	3.03	R. 13.39
Dec...	44	...	1.13	2.21	T. 51.	41 $\frac{1}{2}$...	1.55	4.32	T. 48.	41 $\frac{1}{2}$...	1.75	1.63	T. 48 $\frac{1}{3}$.	48 $\frac{1}{3}$...	1.52	3.35	T. 49 $\frac{1}{3}$.
	54		35.95	37.39		54 $\frac{1}{2}$		34.59	40.18		54		36.62	31.93		54		39.96	40.22	

Observations.—1. It is evident from these tables, whether we attend to separate months, seasons, or years, that the depth of rain is a very erroneous index of the moisture or dryness of the atmosphere. On comparing the 2 months July and August of the year 1772, it appears that the temperature of the air, the state of the winds, and the evaporation, were nearly the same during these 2 months, and yet the rain of August was more than double that of July. The reason why the greater quantity of rain had no more effect than the smaller in adding moisture to the atmosphere, is obvious; for on consulting the register, it appears, that the rain of August fell in heavy showers, and ran off the ground before it could be evaporated; while that of July, falling in small drizzling showers, gave more time for its evaporation.

Again, the temperature of the air, the state of the winds, and the evaporation, were nearly the same during the first 3 months of the year 1773, with what they were during the last 3 months of that year: the state of the air therefore, with respect to moisture and dryness, must have been the same during these 2 seasons; and yet the depth of rain, in one of these seasons, was much more than double what it was in the other. If we attend to whole years, the same observation is confirmed. The rain of 1775 exceeded the rain of 1774 more than 8 inches; and hence it might be concluded, that the atmosphere was more moist in 1775 than in 1774; the reverse of this however is found to be the fact: for there evaporated from a constant and determinate surface of water in 1775, full 3 inches more than evaporated from the same

surface of water in 1774. Consequently the dryness of the atmosphere, or its power of solution, during the year 1775, exceeded that of 1774.

2. If we take the medium of 4 years observations, it appears, that the annual evaporation at Liverpool amounts to 36.78 inches. Dr. Halley observed at London, that water placed in a close room, where neither the winds nor sun could act on it, exhaled only 8 inches during the whole year. He makes no doubt but that the free access of the winds would have trebled the quantity carried away; and that this again would have been doubled by the assistance of the sun. Dr. Halley therefore fixes the annual evaporation of London at 48 inches. If this calculation be admitted, it follows, that the annual evaporation of London exceeds the annual evaporation of Liverpool 11 inches; but were the experiments to be made in London, in the same circumstances with those made at Liverpool, it is probable that this would be found to be more than the real difference.

The learned Cruquius observed at Delft in Holland, that there exhaled from water set in the open air, but in a calm and shady place, about 30 inches; and it is not to be doubted, says Dr. Brownrigg, in the Art of making common Salt, but that double this quantity, or 60 inches, would have exhaled, had it been placed where the sun and winds could have had their due effects. In another part of this publication, Dr. Brownrigg fixes the evaporation of some parts of England at 73.8 inches during the 4 summer months, May, June, July, and August; and the evaporation of the whole year at upwards of 140 inches. These are calculations however which do not appear to correspond with experience; for the whole evaporation at Liverpool, instead of 140 inches, was only 36.78 inches. And the evaporation of the 4 summer months, on a medium of 4 years, instead of 73 inches, was only 18.88 inches.

3. Dr. Hales calculates the greatest annual evaporation from the surface of the earth in England, even that from a surface of hop-ground, at 6.66 inches. If we compare this with the annual evaporation from a surface of water as determined by experiment, we find, that the latter exceeds the former about 30 inches; and that the annual evaporation from a surface of water, is to the annual evaporation from the surface of the earth in this part of England, nearly as 36 to 6, or as 6 to 1.

4. On comparing the depth of rain with the annual evaporation of this part of Lancashire, we find that more falls in rain than is raised in vapour, even though the whole were a surface of water; for the rain is to the evaporation as 37.43 inches to 36.78 inches: and we further find, that the quantity exhaled from the surface of the earth, is little more than a 6th part of what descends in rain; we must therefore have very large supplies from other regions, from the surrounding sea, and from the ocean of warmer climates. Hence we see, why our south

and south-west winds are so often accompanied with rain; for as the air sweeps along the warmer latitudes, it involves a larger proportion of moisture, which is constantly and copiously exhaling from the ocean; and this moisture being retained in a state of solution till it reaches the colder climates, is then either collected in the clouds or immediately precipitated in rain, according to the different conditions of the atmosphere.

These foreign supplies however are uniformly restored to the sources from which they were derived: for that proportion of rain which rises not in vapour, after moistening and refreshing the earth, forms springs, brooks, and rivers, and is thus perpetually returning to the ocean whence it was taken; so truly philosophical are the words of the preacher, when speaking of this vast circulation: "All the rivers run into the sea, yet the sea is not full: unto the place from whence the rivers come, thither they return again."

5. About a century ago, the ingenious Mr. Townley, of Townley, in this county, made some accurate observations on the depth of rain which fell annually in the neighbourhood of the hills which divide Lancashire and Yorkshire; and on taking a medium of 15 years, he determines it to be 41.516 inches. The depth of rain therefore at Townley, exceeds the depth of rain at Liverpool, about 4 inches. This is probably however less than the real difference; for there was a source of error in Mr Townley's experiments, with which the world was not at that time acquainted. Mr. Townley's rain-gage was fixed full 10 yards above the surface of the earth; which circumstance, according to some later observations, makes a very material difference in the result of the experiment. Were the observations to be repeated at Townley, and the rain-gage placed on the ground, there can be no doubt but that the depth of rain would considerably exceed 41.516 inches; for I find from a great number of experiments, made during the last 3 years, with 2 vessels of equal dimensions, one placed on the ground, and the other 18 yards higher on the battlement of the hospital; that the quantity received in the lower vessel exceeds that in the higher more than one-third and less than one-half.

6. An ingenious friend, on perusing these observations, asked, "Whether the fact of evaporation going on equally well in an exhausted receiver, was not an unsurmountable objection to that theory concerning evaporation, which supposes a chemical solution of water in air?" With a view to ascertain this fact I made the following experiment: Two china saucers, each containing 3 ounces of water, were accurately weighed. One of them was placed in the open air; the quicksilver in the thermometer during the experiment between 48° and 50° , the day tolerably clear with a moderate breeze. The other was put under the receiver of an air pump; the air was exhausted, and the pistons occasionally worked, to draw off any of the water which might be supposed to be

converted into vapour. After 4 hours the saucers were again accurately weighed; that in the open air had lost 1 drachm and 8 grains; the weight of the other was not sensibly diminished. From this experiment it appears, that air is a chemical solvent of water, and as such is undoubtedly to be considered as one cause of the evaporation of water. Heat is another cause of evaporation, and when raised to a sufficient degree may produce this effect without the intervention of air, and the evaporation consequently go on copiously in an exhausted receiver, agreeably to the experiments of the ingenious Dr. Irving, in Phipps's Voyage to the North Pole, p. 211.

The following observations are added as a further illustration of this subject. Water may exist in air in 3 different states. 1. In a state of perfect solution. 2. In a state of beginning precipitation. Or, 3. Completely precipitated, and falling in drops of rain. In the 1st instance, where the water is in a state of perfect solution, the air is clear, dry, heavy, and its powers of solution still active, though it already contains a considerable proportion of water. In the 2d, the air becomes moist, foggy, its powers of solution are diminished, and it becomes lighter in proportion as its water is deposited. It is a singular and well-attested fact, that it never rains in the kingdom of Peru; but that during part of the year the atmosphere is constantly obscured with vapours, and the whole country involved in what they call garuas, or thick fogs.

It is not necessary to point out the causes which thus dispose the air to deposit its dissolved water; nor to consider with what bodies air has a stronger affinity than with water; neither to inquire how far the electrical fluid is engaged in the process. It is sufficient to observe, that so long as these causes have a general action on the air, they diminish its power of solution, and give a damp and foggy state of the atmosphere; that when they operate for a considerable proportion of the year, they produce a moist climate; and that when they more generally do not, and the air retains its moisture in a state of perfect solution, the climate is dry. Consequently, that the moisture or dryness of a climate, do not so much depend on the absolute quantity of water which is contained in the air, as on the air being in a state of perfect or imperfect solution. During long continued summer droughts, a very large proportion of water is dissolved in the air; notwithstanding this, the air is still dry, and continues to be so as long as the water remains in a state of perfect solution; but no sooner are the powers of solution diminished, than what was before a dry, now becomes a moist climate.

In the 3d instance, the dissolved water may be either slowly precipitated and fall in drizzling rain, or it may be more powerfully discharged in brisk rain; or there may be partial and sudden precipitations from particular regions, while other parts of the atmosphere still retain their water in a state of perfect

solution. Heavy thunder-showers are the most remarkable instances of partial, sudden, and copious precipitations.

XIV. Of Persons who could not Distinguish Colours. By Mr. J. Huddart. p. 260.

The chief subject of this paper, was one Harris, who lived at Mary-port, in Cumberland, near which place, viz. at Allonby, Mr. Huddart lived. Mr. H. had often heard from others that Harris could discern the form and magnitude of all objects very distinctly, but could not distinguish colours. This report having excited Mr. H.'s curiosity, he conversed with him frequently on the subject. The account he gave was this: that he had reason to believe other persons saw something in objects which he could not see; that their language seemed to mark qualities with confidence and precision, which he could only guess at with hesitation, and frequently with error. His first suspicion of this arose when he was about 4 years old. Having by accident found in the street a child's stocking, he carried it to a neighbouring house to inquire for the owner: he observed the people called it a red stocking, though he did not understand why they gave it that denomination, as he himself thought it completely described by being called a stocking. The circumstance however remained in his memory, and with other subsequent observations led him to the knowledge of his defect. He observed also that, when young, other children could discern cherries on a tree by some pretended difference of colour, though he could only distinguish them from the leaves by their difference of size and shape. He observed also, that by means of this difference of colour they could see the cherries at a greater distance than he could, though he could see other objects at as great a distance as they; that is, where the sight was not assisted by the colour. Large objects he could see as well as other persons; and even the smaller ones if they were not enveloped in other things, as in the case of cherries among the leaves.

Mr. H. believes he could never do more than guess the name of any colour; yet he could distinguish white from black, or black from any light or bright colour. Dove or straw-colour he called white, and different colours he frequently called by the same name: yet he could discern a difference between them when placed together. In general, colours of an equal degree of brightness, however they might otherwise differ, he frequently confounded together. Yet a striped ribbon he could distinguish from a plain one; but he could not tell what the colours were with any tolerable exactness. Dark colours in general he often mistook for black, but never imagined white to be a dark colour, nor a dark to be a white colour.

He had 2 brothers in the same circumstances as to sight; and 2 other brothers and sisters who, as well as their parents, had nothing of this defect. One of

the first-mentioned brothers, who is now living, is master of a trading vessel belonging to Mary-port. Mr. H. met him in December 1776, at Dublin, and took the opportunity of conversing with him. He wished to try his capacity to distinguish the colours in a prism, but not having one by him, he asked him whether he had ever seen a rainbow? He replied, he had often, and could distinguish the different colours; meaning only, that it was composed of different colours, for he could not tell what they were. Mr. H. then showed him a piece of ribbon: he immediately, without any difficulty, pronounced it a striped and not a plain ribbon. He then attempted to name the different stripes: the several stripes of white he uniformly, and without hesitation, called white: the 4 black stripes he was deceived in, for 3 of them he thought brown, though they were exactly of the same shade with the other, which he properly called black. He spoke, however, with diffidence as to all those stripes; and it must be owned, the black was not very distinct: the light green he called yellow; but he was not very positive: he said, "I think this is what you call yellow." The middle stripe, which had a slight tinge of red, he called a sort of blue. But he was most of all deceived by the orange colour; of this he spoke very confidently, saying, "This is the colour of grass; this is green." Mr. H. also showed him a great variety of ribbons, the colour of which he sometimes named rightly, and sometimes as differently as possible from the true colours.

The experiment of the striped ribbon was made in the day-time, and in a good light.

XV. A new Theory of the Rotatory Motion of Bodies affected by Forces Disturbing such Motion. By Mr. John Landen, F. R. S. p. 266.

I consider this paper as not unworthy the notice of this society, through a persuasion that the theory contained will conduce to the improvement of science, by enabling the reader to form a true idea, and accordingly to make a computation of the motion (or change) of the axis about which a body having a rotatory motion will turn, or have a tendency to turn, on being affected by a force disturbing its rotation; particularly of the motion of the earth's axis arising from the attraction of the sun and moon on the protuberant matter of the earth above its greatest inscribed sphere: which compound motion has not been rightly explained by any one of the eminent mathematicians whose writings have come to my hands.

1. Let the sphere $ADBE$, fig. 12, pl. 1, whose radius is r , revolve uniformly about the diameter ACB as an axis, with the angular velocity c , measured at D or E , the motion being according to the order of the letters $DGEH$, in the section at right angles to ACB , fig. 13; and while it is so revolving, let the pole A be impelled by some instantaneous percussive force to turn about the diameter

DCE, from A towards H, with the velocity w . It is proposed to find the new axis about which the sphere will revolve after receiving such impulse.

Calling al , parallel to DC, x ; cl will be $= \sqrt{(r^2 - x^2)}$: the velocity of the point a , about ACB, before the impulse on A, will be $= \frac{cx}{r}$; and the velocity, about DCE, given to the same point a by the said impulse, will be $= \frac{w\sqrt{(r^2 - x^2)}}{r}$.

Which velocities of the point a being in contrary directions, if it be so situated that they be equal, then, one destroying the other, that point will stop and become one of the new poles sought, about which the former poles A and B will revolve with the velocity w ; and the points D and E will revolve with the same velocity c , as before the perturbing impulse on the point A; but instead of describing the great circle DGEH, their motion will be about the new axis ab ; about which they, as well as the points A and B, will describe lesser circles parallel to the great circle de , in which the points d and e (de being at right angles to ab) will revolve about the same axis ab , with the velocity $\sqrt{(c^2 + w^2)}$. Which being denoted by e , and m and n being put for the sine and cosine of the angle Aca to the radius 1, me will be $= w$, $ne = c$, and consequently $mne^2 = cw$.

Now taking $\frac{cx}{r} = \frac{w\sqrt{(r^2 - x^2)}}{r}$, in order to find that new axis ab , we have from that equation $x = \frac{rw}{\sqrt{(c^2 + w^2)}} = \frac{rw}{e} = al$.

Further, it is obvious, that if a spheroid, a cylinder, or any other body, whose centre of gravity is c , and proper axis ACB, were, while revolving about that axis with the same angular velocity c , to receive such an impulse as instantly to give the point A the angular velocity w about DCE; the axis about which that spheroid, cylinder, or other body, immediately after the impulse, would revolve, or would have a tendency to revolve, would be the same line ab .

The great circle de (fig. 12), and any other great circle so situated with respect to the axis of any revolving sphere, I shall denominate the mid-circle.

2. In the manner above described, the poles of the sphere are, by the instantaneous impulse on the point A, instantly changed from A and B to a and b . But if, instead of such impulse, a continued attractive force F , like that of gravity, acted at A, fig. 14, and at the new poles a' , a'' , &c. as they become such by a successive change, caused by such continued action of the force F urging the sphere at every instant to revolve about the diameter $d'e'$, or $d''e''$, &c. of the contemporary mid-circle, the new pole, a' , a'' , &c. would not instantly be at a finite distance from the primitive pole A, but some finite time would be requisite, that by such successive change, the pole might be varied to a finite distance from A: and the force F continuing invariable, the velocity v with which the pole changed its place, would be expressed by $\frac{z}{t}$, t being the time elapsed while the

pole is varying from A to a , and z the length of the arc Aa . Therefore the velocity with which the pole will change its place, during such action of the force F , will be expressed in the same manner as the velocity v , of a body moving uniformly from A to a in the time t may be expressed; that is, in both cases v will be $= \frac{z}{t}$. But there is a material difference between the motion of a body so moving from A to a , and the change of place of the pole a' , a'' , &c. the former is permanent, and will continue to carry the body forward without the action of any force whatever; whereas the latter will instantly cease, and the axis will keep its position, if the force F ceases to act on it; like as the varying direction of a projectile near the earth's surface would immediately cease to change, if the force of gravity ceased to act.

It is observable, that while the force F acts, and the revolving sphere, in consequence of such action, every moment takes a new axis, the angular motion about the axis will continue invariable; the action of such force only altering the axis without altering the angular velocity of the sphere about it: like as the direction of a moving body is altered, without altering its velocity, by an attractive force continually acting on it, in a direction at right angles to that in which the body is moving. And if ever the force F shall cease to act, the sphere will instantly revolve with its primitive velocity c about the axis it then may have been brought to take by the preaction of that force. The new axis, about which the sphere has such tendency to revolve, at any instant during the action of the force F , I shall call the momentary axis; and its poles the momentary poles.

3. From the equation $\frac{cx}{r} = \frac{w\sqrt{(r^2-x^2)}}{r}$ (art. 1) we have $\frac{w}{x} = \frac{c}{\sqrt{(r^2-x^2)}}$. Now if a continued attractive force (F) act during the time t as abovementioned, instead of the instantaneous percussive force at A , according to the doctrine of fluxions we must, instead of w , take \dot{w} , or its equal Ft , and \dot{x} instead of x , in the expression $\frac{w}{x}$; therefore, in this case we have $\frac{\dot{w}}{x} = \frac{Ft}{x} = \frac{c}{\sqrt{(r^2-x^2)}}$. Whence, putting z for the arc (Aa' , or Aa'' , &c.) whose sine is x , and writing \dot{z} for its equal $\frac{r\dot{x}}{\sqrt{(r^2-x^2)}}$, we get $\frac{rFt}{z} = c$, or $\dot{z} = \frac{rFt}{c}$. Hence, v denoting the velocity with which the momentary pole (a' , a'' , &c.) changes its place, during the action of the accelerative force F , we have $\dot{z} = v = \frac{rFt}{c}$, and consequently $v = \frac{rF}{c}$.

4. The value of v may also be determined in the following manner (fig. 15). Conceive a very thin string, without weight, to have one of its ends fastened to a fixed point l , and the other to a heavy particle of matter m ; also conceive such particle so to revolve with the velocity c , about the line lm , that a certain accelerative force F (like that of gravity referred to a certain direction) continually acting on the said particle m , in a direction at right angles both to the string lm , and to the tangent to the curve in which m is moving, the string

shall describe a conical surface. Then lm being denoted by r , and mo , perpendicular to ln , by q ; $\frac{e^2}{q}$, the centrifugal force urging m in the direction om , will be to F , as r to $\sqrt{(r^2 - q^2)} = lo$. Therefore F must be $= \frac{e^2 \sqrt{(r^2 - q^2)}}{rq}$. Now while m is so revolving, if the force F ceases acting, the said particle m will, it is obvious, immediately proceed to describe a great circle of the sphere whose radius is r and centre l , of which great circle one of the poles will be situated in a lesser circle parallel to, and 90° distant from, that described by m during such action of the said force; which pole, during such action, will change its place in the said lesser circle in which it will at any time be found, with a velocity v , which will be to e , as (s) the radius of the last-mentioned circle, to q . But s will be $= \sqrt{(r^2 - q^2)}$; therefore we have $v : e :: \sqrt{(r^2 - q^2)} : q$, and $\frac{\sqrt{(r^2 - q^2)}}{q} = \frac{v}{e}$. Consequently $F = \frac{e^2 \sqrt{(r^2 - q^2)}}{rq}$ will be $= \frac{e^2}{r} \times \frac{v}{e} = \frac{ev}{r}$, and $v = \frac{rF}{e}$.

Let now m be a point on the surface of a sphere whose centre is l , and radius $lm = r$; and let the sphere revolve about an axis so that m shall describe a great circle with the velocity e . If then such a motive force begins to act on the sphere, that, continuing its action, the point m shall always be urged by the invariable accelerative force F , to move in a direction at right angles to the ray lm , and to the tangent to the curve which m will describe; that point it is obvious will, in consequence of the action of that force, describe a lesser circle of the same radius (q) as that described by the particle m , when fastened to a string, and acted on by the force F as abovementioned; and the centre of the sphere being always considered as at rest, one of the momentary poles of the sphere will describe a circle whose radius will be $= \sqrt{(r^2 - q^2)}$ parallel to, and 90° distant from, that described by the point m . For if the said force were to cease acting, that point of the sphere would describe a great circle, as would the particle m at the string in the like case; and therefore both the said particle and the point m of the sphere, at every instant having the same tendency, and being acted on by equal accelerative forces, the effect will be the same with respect to the motion of each. Consequently, v being put to denote the velocity with which the momentary pole changes its place, in the circle which it will describe while the motive force producing the accelerative force F acts on m as just now mentioned, v will be $= \frac{rF}{e}$, the same as in the preceding article, e here denoting that velocity which we there denoted by c .

5. Referring the point of action of the perturbing force to the midcircle, we have not hitherto considered that point as varied with a greater or less velocity than (e) that of the point m ; that is, with reference to such circle we have always considered the point m as the point of action. But it is obvious that,

cæteris paribus, the point of action with respect to the mid-circle (which point we will now denote by (q)) may be varied with a velocity greater or less than e ; and that, cæteris paribus, the velocity (v) of the momentary pole will be the same, with what velocity soever (q) the point of action of the force F be varied; the direction in which that force acts being always at right angles to the ray (lq) from the centre of the sphere, and to the tangent to the curve described by (q) such point of action.

Yet, though v continues the same whether, cæteris paribus, (u) the velocity of the point q be greater, equal to, or less than e , the immoveable circle in which the momentary pole will be found will not continue the same; that circle being greater, equal to, or less than the circle whose radius is $\sqrt{(r^2 - q^2)}$, according as u is less, equal to, or greater than e ; as will be made more evident by what follow:

6. In fig. 16, let p' , in the great circle $rp'q'r$, be one of the poles of the axis about which the sphere $rstv$, whose radius is r , is revolving, according to the letters $vq's$, with the angular velocity e , measured at the distance r from the axis; and while it is so revolving let the said pole be urged to turn about a diameter of the mid-circle $vq's$ towards q' , by an accelerative force F ; and let such force continue to act on the successive new poles p'', p''' , &c. as they become such, always urging the sphere to turn about a diameter of the contemporary mid-circle, while the direction in which such perturbing force acts is regulated in the following manner:

Conceive the said revolving sphere to be surrounded by an immoveable concave sphere of the same radius r . Then the momentary pole $(p', p'', p''', \&c.)$ will always be found in some curve $p'p''p''' \&c.$ in the said concave sphere, and in some curve $p'p''p''' \&c.$ on the revolving sphere; which last mentioned curve will continually touch and roll along the other curve $p'p''p''' \&c.$ on the immoveable sphere, the force F and the direction in which it acts varying in any manner whatever. Let F be invariable; then it is obvious, that the 2 curves so touching each other will be circles; and if great circles $p'q', p''q'', p'''q''', \&c.$ be described on the surface of the immoveable sphere whose planes shall be at right angles to the plane of the circle $p'p''p''' \&c.$ the points $q'q''q''' \&c.$ in it, each 90° distant from $p'p''p''' \&c.$ respectively, will be in a circle $(q'q''q''' \&c.)$ parallel to the said circle $p'p''p''' \&c.$ Now as a regulation to the direction in which the force F shall urge the momentary pole, let that direction be always a tangent to the great circle so passing through that pole and the correspondent point $q', q'',$ or $q''', \&c.$ while the arcs $q'q'', q'q''', \&c.$ are to the arcs $p'p'', p'p''', \&c.$ respectively in the constant ratio of u to v .

The direction in which the force F acts being so regulated, it is obvious that the radius of the circle $p'p''p''' \&c.$ being denoted by h , the radius of the circle

$q'q''q'''$ &c. will be $= \sqrt{(r^2 - h^2)}$, the distance of these parallel circles being 90° . Therefore their peripheries being as the velocities (v and u) with which they are described, their radii h and $\sqrt{(r^2 - h^2)}$ will be in the ratio of the said velocities; that is $v : u :: h : \sqrt{(r^2 - h^2)}$; whence, $\frac{u}{v}$ being $= \frac{\sqrt{(r^2 - h^2)}}{h}$, h , the radius of the circle $p'p''p'''$ &c. is found $= \frac{rv}{\sqrt{(u^2 + v^2)}} = \frac{r}{\sqrt{(1 + \frac{e^2 u^2}{r^2 F^2})}}$; and $\sqrt{(r^2 - h^2)}$ the ra-

dius of the circle $q'q''q'''$ &c. $= \frac{ru}{\sqrt{(u^2 + v^2)}} = \frac{r}{\sqrt{(1 + \frac{r^2 F^2}{e^2 u^2})}}$; v being $= \frac{rF}{e}$, the ve-

locity with which the momentary pole p'' , p''' , &c. changes its place. Consequently, if PR' be an arc in the said immoveable concave sphere whose sine is $\frac{rv}{\sqrt{(u^2 + v^2)}} = \frac{r}{\sqrt{(1 + \frac{e^2 u^2}{r^2 F^2})}}$, the great circles $q'p'$, $q''p''$, $q'''p'''$, &c. will intersect

each other at the point R .

7. Further, the force F being invariable, and acting as expressed in the preceding article, the primitive pole p' and the momentary poles p'' , p''' , &c. will all be found in a circle $p'p''p'''$ &c. described on the surface of the revolving sphere, as observed in that article; which circle, during the action of the force of F , will (as is also observed in the said article) always touch and roll along the immoveable circle ($p'p''p'''$ &c.) whose radius we have just now found $= \frac{rv}{\sqrt{(u^2 + v^2)}} = \frac{r}{\sqrt{(1 + \frac{e^2 u^2}{r^2 F^2})}}$; the point of contact being always the momentary pole.

Let the sine of the arc $p'a$ of the great circle $RP'aq'RT$ in the revolving sphere be equal to h , the radius of the said circle $p'p''p'''$ &c.; then will the point a and its opposite point (o) in the surface of the said sphere, during the action of the force F , describe circles in the surrounding immoveable concave sphere parallel to ($p'p''p'''$ &c.) the circle described by the momentary pole p'' , p''' , &c. in the same concave sphere. And such point a and its opposite point (o) being continually urged by the force F in directions at right angles to the tangents to the arcs they describe, their velocity will continue the same as before the action of the said force commenced; which velocity, and the radius of the said circle $p'p''p'''$ &c. will be determined by the following computation.

That radius being denoted by h , we have $r : h :: e : \frac{ek}{r}$, the velocity of the point a before the action of the force F commenced, and $h : v :: \kappa : \frac{\kappa v}{h}$, the velocity of the same point (a) during the action of that force, κ being put for the sine of the arc QR ; therefore, the velocity of a continuing the same during the action of F as before, we have $\frac{ek}{r} = \frac{\kappa v}{h}$. But κ is the sine of the sum of the arcs RP' , $p'a$, whose sines are h and h respectively; therefore $\frac{h\sqrt{(r^2 - h^2)}}{r} + \frac{h\sqrt{(r^2 - h^2)}}{r}$ will

be = κ ; and by substitution we get

$\frac{ek}{r} = \frac{v\sqrt{(r^2 - k^2)}}{r} + \frac{kv\sqrt{(r^2 - h^2)}}{r} = \frac{v\sqrt{(r^2 - k^2)}}{r} + \frac{ku}{r}, \frac{\sqrt{(r^2 - h^2)}}{h}$ being = $\frac{u}{v}$ by the preceding article. Hence we find $h =$

$\frac{rv}{\sqrt{(e-u)^2 + v^2}}$; and it follows that $\frac{ev}{\sqrt{(e-u)^2 + v^2}}$ ($= \frac{ek}{r}$) will be equal to the velocity of the point a , and also of its opposite point (o) in the surface of the sphere. It also follows, that κ , the radius of each of the circles described by those points, during the action of the force F , will be equal to

$$\frac{rev}{\sqrt{(u^2 + v^2)} \times \sqrt{(e-u)^2 + v^2}}.$$

By what is done it appears, that during the action of the force F , the motion of the revolving sphere will be regulated by the circle $p'p''p'''$ &c. on it (whose radius is $\frac{rv}{\sqrt{(e-u)^2 + v^2}} = \frac{r}{\sqrt{(1 + \frac{e^2(e-u)^2}{r^2F^2})}}$) continually touching and rolling along

the immoveable circle $p'p''p'''$ &c. (whose radius is $\frac{rv}{\sqrt{(u^2 + v^2)}} = \frac{r}{\sqrt{(1 + \frac{e^2u^2}{r^2F^2})}}$) so

that the velocity of the point of contact be = $v = \frac{rF}{e}$. Considering the point a as always urged from the points p', p'', p''' , &c. and consequently its opposite point (o) towards those points, it is necessary to observe, that according as u is less or greater than e , the arc $p'a$ (whose sine is $\frac{rv}{\sqrt{(e-u)^2 + v^2}}$) will be less or greater than 90° ; and the point (o) opposite to a , on the surface of the sphere, will accordingly be at a greater or less distance than 90° from p' .

If u be negative, the arc $p'a$ whose sine is $\frac{rv}{\sqrt{(u^2 + v^2)}}$ will be greater than 90° .

8. The motion of the sphere, according to the regulation in the preceding article, is one motion compounded of the primitive motion of the sphere and the motion generated by the action of the force F . But conceiving $\frac{ev}{\sqrt{(e-u)^2 + v^2}}$, the velocity of the point a , to arise from an impulse given to it while the sphere revolved about an axis of which a was an immoveable pole before such impulse, and about which the mid-circle corresponding to that primitive axis revolved with the angular velocity $\frac{e(e \sin u)}{\sqrt{(e-u)^2 + v^2}}$;* and considering that the force F , continually acting at right angles to the momentary direction of the point a and to the plane of the said mid-circle, only serves to alter the position of the said primitive axis; we may, by the help of what is done above, explain the motion which the sphere will have, during the action of the force F , so as to retain in our ideas the two primitive motions (one about the axis ao , and the other about a diameter at right angles to that axis) as remaining distinct and unaltered.

* Denoting this by c , and the velocity of a by d , $\sqrt{(c^2 + d^2)}$ is = e , agreeable to art. 1.—Orig.

In fig. 17, let ED be a great circle on the revolving sphere, of which α is a pole, and let a smaller circle DL parallel to (MA) that which we have found will be described by the point α , be drawn on the immoveable concave sphere, so as to touch that great circle in the point (D) where the great circle $\alpha P'R$ cuts it; the radius of which lesser circle will be $(= \sqrt{r^2 - K^2}) = \frac{rv^2 \propto ru(e-u)}{\sqrt{(u^2 + v^2)} \times \sqrt{(e-u)^2 + v^2}}$. Then the revolving sphere, during the action of the force F , will so move, that the first mentioned great circle (ED) shall continually touch and roll along the said lesser circle DL, the velocity of the point of contact (along that circle) being $= \frac{v^2 \propto u(e-u)}{\sqrt{(e-u)^2 + v^2}}$,* and the sphere at the same time turning about the axis of which α is a pole with the primitive angular velocity $\frac{e(e \propto u)}{\sqrt{(e-u)^2 + v^2}}$.

Thus the primitive motion about the axis of which α is a pole is preserved distinct, while that pole proceeds describing a circle, whose radius is

$\frac{rev}{\sqrt{(u^2 + v^2)} \times \sqrt{(e-u)^2 + v^2}}$, with the velocity $\frac{ev}{\sqrt{(e-u)^2 + v^2}}$ which we supposed given to it.

It is observable, that the last mentioned velocity will, according to this regulation of the motion, be to the primitive angular velocity about the axis of which α is a pole, as v to $e - u$, or as v to $u - e$, according as u is less or greater than e ; that is, according as the arc $P'\alpha$ is less or greater than 90° .

9. From what has been said it follows, that denoting the two primitive angular velocities $\frac{e(e \propto u)}{\sqrt{(e-u)^2 + v^2}}$ and $\frac{ev}{\sqrt{(e-u)^2 + v^2}}$ (specified in the preceding article) by c and d respectively, the radius (fig. 16) of the circle $p'p''p'''$ &c. (or sine of the arc $P'\alpha = P'\alpha$, &c.) will be $= \frac{dr}{e}$; the radius of the circle $P'P''P'''$ &c. (or sine of the arc $P'R = P''R$, &c.) $= \frac{dr^2 F}{e \sqrt{(d^2 e^2 \mp 2cdr_F + r^2 F^2)}}$: a great circle passing through the primitive poles α and α , on the revolving sphere, will turn from the position ORQ with the velocity

$\frac{r_F}{d}$ measured at the mid-circle, or with the velocity $\frac{dr_F}{\sqrt{(d^2 e^2 \mp 2cdr_F + r^2 F^2)}}$ measured at the fixed point R; whilst those poles describe, with the velocity d , circles parallel to $P'P''P'''$ &c. the radius (K) of each of the circles (fig. 17) so described being $= \frac{d^2 r}{\sqrt{(d^2 e^2 \mp 2cdr_F + r^2 F^2)}}$: the radius $\sqrt{(r^2 - K^2)}$ of the circle DL will be $= \frac{r \times (cd \mp r_F)}{\sqrt{(d^2 e^2 \mp 2cdr_F + r^2 F^2)}}$; and the velocity $\frac{v^2 \propto e(e-u)}{\sqrt{(e-u)^2 + v^2}}$ along the said circle DL $= c \mp \frac{r_F}{d}$: the upper or lower of the double signs taking place according as u

* This is to the velocity of the point α as $\sqrt{(r^2 - K^2)}$ to K ; that is, as the radii of the arcs described.—Orig.

($= e \mp \frac{cr^F}{de}$) is less or greater than e ; that is, according as the arc pa (whose sine is $= \frac{dr}{e}$) is less or greater than 90° .

10. As an instance of the use of the preceding conclusions, Mr. L. applies them in the solution of a very interesting problem, which he had not before seen solved, viz.

Suppose a given spheroid, while revolving uniformly about its proper axis, with a given angular velocity, to be suddenly urged by some percussive force to turn, with some given angular velocity, about a diameter of its equator; it is proposed to explain the rotatory motion of the spheroid consequent to the impulse so received.

—In fig. 18, 19, let $DOEA$ be the spheroid, whose semi-axis $co = ca$ is $= b$, and equatorial radius $CD = CE = r$; and supposing it before the impulse to revolve about its proper axis oa with the angular velocity c , measured at the distance r from the axis, let the poles (o and a) be suddenly urged by some percussive force to turn about a diameter of the equator of the spheroid, with the angular velocity d , likewise measured at the distance r from that diameter. On receiving such impulse, the spheroid will take a new axis of motion, which will be a momentary one; suppose such new axis to be $pc\pi$.* Then the particles of the spheroid being urged (or having a tendency) to turn about that axis with the angular velocity $\sqrt{c^2 + d^2}$, (which we will denote by e) their joint centrifugal force will so urge the spheroid to turn about that diameter of the equator which shall be at right angles to the momentary axis $pc\pi$, that the accelerative force of the point D of the equator to turn it about the said diameter according to the order of the letters DQE , will (as appears by what is proved in art. 1, and in the appendix following) be $= \frac{cd}{r} \times \frac{r^2 - b^2}{r^2 + b^2}$, or $\frac{cd}{r} \times \frac{b^2 - r^2}{r^2 + b^2}$ according as b is less or greater than r ; and it follows from hence, and what is proved in art. 3 and 4, that v , the angular velocity (at the distance r from c) with which the momentary pole p will change its place, will accordingly be $= \frac{cd}{e} \times \frac{r^2 - b^2}{r^2 + b^2}$ or $\frac{cd}{e} \times \frac{b^2 - r^2}{r^2 + b^2}$.

Again, referring to our observation in art. 8, let $u - e$ be to $\frac{cd}{e} \times \frac{r^2 - b^2}{r^2 + b^2}$ (the value of v) as c to d , u being greater than e ; or let $e - u$ be to $\frac{cd}{e} \times \frac{b^2 - r^2}{r^2 + b^2}$ as c to d , u being less than e ; whence, in both cases, we shall have the same expression ($\frac{c^2}{e} \times \frac{r^2 - b^2}{r^2 + b^2}$) for the value of $u - e$; and consequently u , in both cases, will be $= e + \frac{c^2}{e} \times \frac{r^2 - b^2}{r^2 + b^2}$. Conceive now a spherical surface without matter,

* To find the position of this axis see art. 1, by which the sine of the angle ocp (to the radius r) is found $= \frac{dr}{e}$.

having the same centre and radius as the equator DE , to be carried about with the revolving spheroid; and suppose a sphere, whose radius is r , to revolve about an axis $pc\pi$ with the angular velocity e , and, while it is so revolving, let an accelerative force (F) equal to $\frac{cd}{r} \times \frac{r^2 - b^2}{r^2 + b^2}$ or $\frac{cd}{r} \times \frac{b^2 - r^2}{r^2 + b^2}$, according as b is less or greater than r , urge the pole p , and the successive momentary poles as they become such, to turn about a diameter of the contemporary mid circle in the manner expressed in art. 6, u being to v as $e +$

$$\frac{c^2}{e} \times \frac{r^2 - b^2}{r^2 - b^2} \text{ to } \frac{cd}{e} \times \frac{r^2 - b^2}{r^2 + b^2}, \text{ or as } e + \frac{c^2}{e} \times \frac{r^2 - b^2}{r^2 + b^2} \text{ to } \frac{cd}{e} \times \frac{b^2 - r^2}{r^2 + b^2},$$

according as b is less or greater than r . Then will the motion of the surface of this sphere be exactly the same as the motion of the said spherical surface carried about with the revolving spheroid after receiving the impulse of the percussive force. Therefore, having reference to our conclusions in the preceding articles, by substitution we readily obtain the solution to our problem.

By substituting properly $\frac{cd}{r} \times \frac{r^2 - b^2}{r^2 + b^2}$ or $\frac{cd}{r} \times \frac{b^2 - r^2}{r^2 + b^2}$ for F , we find,

$$\frac{\frac{d^2 r}{\sqrt{(d^2 e^2 \mp 2cdr_F + r^2 F^2)}}}{\frac{dr}{c} \times \frac{r^2 + b^2}{\sqrt{(4r^4 + (r^2 + b^2)^2 \times \frac{d^2}{c^2})}}} \times \frac{r \times (cd + r_F)}{\sqrt{(d^2 e^2 \mp 2cdr_F + r^2 F^2)}} =$$

$$\frac{2r^3}{\sqrt{(4r^4 + (r^2 + b^2)^2 \times \frac{d^2}{c^2})}} \text{ and } c + \frac{r_F}{d} = \frac{2r^2 c}{r^2 + b^2}.$$

Which equations, respect being had to the conclusions in art. 8 and 9, indicate that, whether b be less or greater than r , if an immoveable circle DL , whose radius is $= \frac{2r^3}{\sqrt{(4r^4 + (r^2 + b^2)^2 \times \frac{d^2}{c^2})}}$ be conceived to be described in a plane incli-

ned to the plane of the equator of the spheroid (before the impulse) in an angle whose sine (to the radius r) is $= \frac{dr}{c} \times \frac{r^2 + b^2}{\sqrt{(4r^4 + (r^2 + b^2)^2 \times \frac{d^2}{c^2})}}$, so that the said cir-

cle touch the said equator in the point D in the section $opDAE$; the spheroid after the impulse will so revolve, that its equator will always touch and roll along the said immoveable circle (DL), the velocity of the point of contact (along that circle) being $= \frac{2r^2 c}{r^2 + b^2}$, while the spheroid turns about its proper axis (oq) with the primitive angular velocity c , and the poles o and q (by the said rolling of the equator) describe circles, whose radii are each $= \frac{bd}{c} \times \frac{r^2 + b^2}{\sqrt{(4r^4 + (r^2 + b^2)^2 \times \frac{d^2}{c^2})}}$, pa-

rallel to the said circle DL , with the angular velocity d (or their proper velocity $\frac{bd}{r}$) which we supposed given to them by the impulse.* Thus the motion of the spheroid consequent to the impulse appears to be remarkably regular.

* Other ways of solving the problem are also suggested by the preceding articles.—Orig.

And in the very same manner may be explained the motion of a cylinder, whose primitive motion about its proper axis may be disturbed by some percussive force, in like manner as we supposed the spheroid disturbed; only (instead of the former substitution for F) substituting for the accelerative force arising from the centrifugal force of the particles of the revolving cylinder its proper value $\frac{cd}{r} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2}$ (computed in the Appendix) and afterwards proceeding as we have done with regard to the spheroid, b denoting half the length of the cylinder, and r the radius of any section at right angles to its proper axis.

Seeing that $(\frac{cd}{r} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2})$ the expression for the said accelerative force respecting the cylinder vanishes when b is $= \frac{1}{2}r\sqrt{3}$, it is manifest that the cylinder in that case will (with respect to its own particles) undisturbedly revolve about any axis whatever passing through its centre of gravity, as will a sphere. Which remarkable property of that particular cylinder I believe has not before been taken notice of. And there are also bodies of other forms having the like property.

The preceding articles lead us to consider the motion of the earth's axis in a light more clear and satisfactory than any in which it has before been considered; but I must, for want of leisure, defer making the application till some future opportunity: only observing here, that by what is done above it appears, that from the action of the sun and moon on the earth, its axis has a diurnal motion which I have no where seen explained. Which motion is not much unlike that of the axis of the revolving spheroid just now considered, when $(2b)$ this last mentioned axis is many times longer than $(2r)$ the equatorial diameter of the said spheroid, and $\frac{d}{c}$ very small.

APPENDIX.—*Showing how the joint centrifugal force of the particles of a spheroid or cylinder, having a rotatory motion about any momentary axis, is computed.*

—1. In fig. 20, let p be a particle of matter firmly connected with the plane $DOEFQG$, in which the line ocq is situated; and pq being a perpendicular from p to the said plane, let the distance pq be denoted by u ; also, the line ql being at right angles to $olcq$, let the distance pl be denoted by h . Then the said plane with the particle p being made to revolve about $olcq$ as an axis, with the angular velocity e measured at the distance a from the said axis, the velocity of p will be $= \frac{he^2}{a}$, and its centrifugal force from l will (by a well known theorem) be $= \frac{he^2}{a}$ to make it a^2 the expression being $\frac{he^2}{a^2} \times p$. Whence, by resolving that force into 2 others, one in the direction qp , and the other in a direction parallel to lq , it appears that the force urging p from the plane $DOEFQG$, will be $= \frac{ue^2}{a} \times p$, let the distance lq be what it will.

2. The particle p being connected with the plane $DOEFQG$ as mentioned in

the preceding article, and the distance cl being denoted by v ; if p be urged directly from the said plane by a force $fu \times p$, the efficacy of that force to turn the said plane about the line hcl , drawn in it at right angles to ocq , will (by the property of the lever) be equivalent to the force $\frac{fuv \times p}{g}$, acting on the said line ocq at right angles to the said plane at the distance g from the point c . It is also obvious that, *cæteris paribus*, the efficacy will be the same let the distance of q from l be what it will.

In fig. 21, let q coincide with l ; and let ch be a line in the plane clp continued (which plane will be at right angles to the plane $doefqg$;) also, pk being at right angles to ch , let those lines pk and ch be denoted by w and x respectively. Then the sine and cosine of the angle hco to the radius r , being respectively denoted by m and n , the force $\frac{fuv \times p}{g}$ will be $= \frac{f \times p}{g} \times (mn \times (w^2 - x^2) + (m^2 - n^2) \times wx)$. Consequently, if each particle of any solid body, through which a line hcl and a plain $doefqgh$ may be conceived to pass, be urged from that plane by a force expressed by $fu \times p$ as above; the force which, acting on the line ocq at the distance g from c , would be equivalent to the efficacy of all the forces acting on the several particles of that body to turn the same about the line hcl , will be obtained by computing the sum of all the forces $\frac{f \times p}{g} \times (mn \times (w^2 - x^2) + (m^2 - n^2) \times wx)$ acting on the said body.

The computation of such equivalent force will in most cases be abridged by observing that, if pk be continued to p'' , so that hp'' be $= hp$, the efficacy of the force on the particle p'' , to turn the body about the line hcl in opposition to the force on the particle p , will be represented by the equivalent force $\frac{f \times p''}{g} \times (mn \times (x^2 - w^2) + (m^2 - n^2) \times wx)$ acting on the line ocq at the distance g from c ; and that therefore the efficacy of the two forces on p and p'' , to turn the body about hcl , will be represented by the equivalent force $\frac{2f \times p}{g} \times mn \times (w^2 - x^2)$ acting on the line ocq , at right angles to the plane $doefqgh$, at the distance g from c .

3. In fig. 22, 23, if the body be a cylinder, a spheroid, or the like, and its proper axis be situated in the line ch , the ordinates corresponding to the abscissæ hp , hp'' , in the circular section hi whose centre is h , will each be parallel to that diameter passing through c , about which the body will be urged to turn; and each of those ordinates will be $= \sqrt{(y^2 - w^2)}$, y being the radius of such section. Therefore, writing $2\sqrt{(y^2 - w^2)}$ instead of p , it follows that $\frac{4Af}{g} \times mnx' \times (\frac{y^4}{4} - x^2y^2)$, the whole fluent of $\frac{4f \times \sqrt{(y^2 - w^2)}}{g} \times mnx' \times (w^2 - x^2) \times \dot{w}$, generated while w ($= hp = hp''$) from 0 becomes equal to the radius y (both x and y being considered as invariable) will express the value of the force which,

acting on the line ocq at the distance g from c , would be equivalent to the force of all the particles in the said section, whose thickness is denoted by the indefinitely small quantity x' ; the distance ck being denoted by x , and A being put for (.78539) the area of a quadrant of a circle whose radius is 1.

4. Fig. 22, in the cylinder whose length is $2b$ and diameter $2r$; y being $= r$, $\frac{1}{4}y^4 - x^2y^2$ will be $= r^2 \times (\frac{1}{4}r^2 - x^2)$: consequently the fluent of $(\frac{1}{4}r^2 - x^2) \times \dot{x}$, generated while x from o becomes $= b$, being $\frac{1}{4}br^2 - \frac{1}{3}b^3$, we have $\frac{8Afr^3}{g} \times mn \times (\frac{br^2}{4} - \frac{b^3}{3}) = \frac{f_{mn}}{12g} \times (3r^2 - 4b^2) \times M$ for the force which, acting as above at the distance g from (c) the centre of gravity of the cylinder, would be equivalent to the efficacy of the forces acting as above on all the particles of the cylinder to turn it about a diameter passing through c , M being the mass or content of the cylinder.

5. Fig. 23, in the spheroid whose proper axis is $2b$ and equatorial diameter $2r$, y^2 being $= \frac{r^2}{b^2}(b^2 - x^2)$, $\frac{1}{4}y^4 - x^2y^2$ will be $= r^2 \times (\frac{r^2}{4} - \frac{r^2x^2}{2b^4} + \frac{r^2x^4}{4b^4} - x^2 + \frac{x^4}{b^2})$: consequently the fluent of $\frac{r^2\dot{x}}{4} - \frac{r^2x^2\dot{x}}{2b^4} + \frac{r^2x^4\dot{x}}{4b^4} - x^2\dot{x} + \frac{x^4\dot{x}}{b^2}$, generated while x from o becomes $= b$, being $\frac{r^2b}{4} - \frac{r^2b}{6} + \frac{r^2b}{20} - \frac{b^3}{3} + \frac{b^3}{5} = \frac{2}{15} \times (r^2b - b^3)$, we have $\frac{16Afr^2}{15g} \times mn \times (r^2b - b^3) = \frac{f_{mn}}{5g} \times (r^2 - b^2) \times s$ for the force which, acting at the distance g from c the centre of the spheroid, would be equivalent to the efficacy of the forces acting as above on all the particles of the spheroid to turn it about a diameter of its equator, s being the mass or content of the spheroid.—These equivalent forces are distinguished by the name of motive forces; the correspondent accelerative forces are computed in the following articles.

6. Fig. 24, the body being a spheroid whose centre is c , and whose proper axis PN is $= 2b$, and equatorial diameter $AB = 2r$; let F be the accelerative force of a particle at the distance g from the axis about which the body is urged to turn, which axis is supposed to be a diameter of its equator. Denote ck by x ; ki by y ; and let the abscissa ko and its correspondent ordinate (parallel to the last mentioned axis) in the circle whose radius is ki , be denoted by s and t respectively. Then, considering the body as urged to turn about that diameter of its equator which is at right angles to AB , the accelerative force of every particle in the said ordinate will be $= \frac{\sqrt{(s^2 + x^2)}}{g} \times F$, and the motive force of all the particles in the same ordinate will be $= \frac{\sqrt{(s^2 + x^2)}}{g} \times Fts' = \frac{\sqrt{(s^2 + x^2)}}{g} \times Fs' \sqrt{(y^2 - s^2)}$; to which (by the property of the lever) a motive force $= \frac{s^2 + x^2}{g^2} \times Fs' \sqrt{(y^2 - s^2)}$, acting at the distance g from the centre at right angles to a ray from it, would be equivalent. Therefore, considering x and y as invariable, and

s only as variable, $\frac{4Fx'}{g^2} \times$ the whole fluent of $s \sqrt{(y^2 - s^2)} \times (s^2 + x^2)$ will denote a force which, acting at the distance g from c , would be equivalent to the motive force of all the particles in the section hi whose radius is hi and thickness x' . Which fluent is $= Ay^2 \times (x^2 + \frac{y^2}{4}) = \frac{Ar^2}{b^2} \times (b^2 - x^2) \times (x^2 + \frac{r^2}{4b^2} \times (b^2 - x^2))$. Consequently $\frac{8Ar^2F}{b^2g^2} \times$ the whole fluent of $x \times (b^2 - x^2) \times (x^2 + \frac{r^2}{4b^2} \times (b^2 - x^2))$ will denote a motive force which, acting at the distance g from c at right angles to a ray from it, would be equivalent to the whole motive force urging the spheroid to turn as above mentioned. Such equivalent force will therefore be $= \frac{16Ar^2F}{15g^2} \times (r^4b + r^2b^3) = \frac{F}{5g^2} \times (r^2 + b^2) \times s$; and this being put $= \frac{fmn}{5g} \times (r^2 - b^2) \times s$ (the value of the same force found in art. 5,) we find $F = fgm n \times \frac{r^2 - b^2}{r^2 + b^2}$; which will be $= \frac{gmne^2}{a^2} \times \frac{r^2 - b^2}{r^2 + b^2}$, if f be $= \frac{e^2}{a^2}$, its value computed in art. 1.

Or F will be denoted by $\frac{cd}{r} \times \frac{r^2 - b^2}{r^2 + b^2}$; if r be to e as m to d , and as n to c ; and a and g be each $= r$.

7. Fig. 25, the body being a cylinder whose centre of gravity is in c , and whose proper axis PN is $2b$, and diameter $2r$; the accelerative force (F) at the distance g from c , will in like manner be found $= \frac{gmne^2}{a^2} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2}$; the cylinder being considered as urged to turn about a diameter passing through c .

If $r : e :: m : d :: n : c$, and a and g be each $= r$, F will be $= \frac{cd}{r} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2}$.

XVI. Directions for making the Best Composition for the Metals of Reflecting Telescopes; with a Description of the Process for Grinding, Polishing, and giving the Great Speculum the True Parabolic Curve. By Mr. John Mudge. p. 296.*

As the method of casting, grinding, and polishing the specula of reflecting telescopes, by Mess. Molyneux and Hadley, published in Dr. Smith's Optics, is generally followed by, and well known to the workmen, I shall avoid a repetition of the general directions there given, and only remark on such parts of that process as I think are essentially defective, and supply them by a method of my own, which, from repeated trials, I have found completely to answer the purpose. After therefore referring to the above account for the manner of making the gages, patterns, the method of casting, as well as a great many other particulars, I will begin with

* For this paper Mr. Mudge was honoured with the Society's prize gold medal.

The best composition for the specula of reflecting telescopes.—The perfection of the metal of which the speculum should be made, consists in its hardness, whiteness, and compactness; for on these properties the reflective powers and durability of the speculum depend. And first of the hardness and whiteness of the metal. There are various compositions recommended in Smith's Optics, all which have however their several defects. Three parts copper and one part and one-fourth of tin will make, he says, a very hard white metal; but liable to be porous. This however is an imperfection which I shall presently show the method of preventing; but the permanent fault of it, and which I have myself experienced, is, that it is not hard enough. The speculum of a reflecting telescope ought to have the utmost possible hardness, compatible with its being operated on by the tool.

It is to be observed, that the smallest quantity of tin added to melted copper destroys its perfect malleability, and at the same time produces a metal whiter and harder than copper. As the quantity of tin is increased, suppose to a 5th or 4th part, the metal becomes whiter, still harder, and consequently more friable. If the quantity of tin be further increased, as to a 3d of the whole composition, it will then have its utmost whiteness; but at the same time will be rendered so exceedingly hard and brittle, that the finest washed emery on lead or brass will not cut it, without breaking up its surface; and the common blue stones used in grinding the speculum will not touch it. Mr. Jackson, a mathematical instrument maker, and a most excellent workman, told me, that the tin was increased to the above proportion in his metals; but that they were so exceedingly hard, that it cost him an infinite deal of pains, and a journey of 200 miles, to find out a stone of sufficient hardness to cut it, and whose texture at the same time was fine enough not to injure its surface. I have seen several of his finished metals; they were indeed perfectly hard and white; but the kind of stone with which he ground them he kept a secret.

After many experiments with various proportions of tin and copper, by gradually increasing the former, I at last found that $14\frac{1}{2}$ ounces of grain-tin to 2 lb. of good Swedish copper, made a beautiful white and very hard metal; so hard indeed, that the stones would but barely cut it, and washed emery on brass or tin but just grind the surface without breaking it up; whereas the proportion of tin being increased by the addition of only another half ounce, the former inconvenience immediately took place. This therefore is the maximum in point of hardness.

This composition, though complete in the former respects, was, as well as Dr. Smith's, subject every now and then to be porous; sometimes indeed I succeeded in casting a single metal, or perhaps 2 or 3 without this imperfection; at other times, and most frequently indeed, they were attended with this defect,

without being at all able to form a probable conjecture at the cause of my success or disappointment. The pores were so very small, that they were not discoverable when the metal had received a good face and figure on the hones, nor till the last and highest polish had been given; and then it frequently appeared as if dusted over with millions of microscopic pores, which were exceedingly prejudicial in 2 respects; for first, they became in time a lodgment for a moisture which tarnished the surface; and 2dly, on polishing the speculum, the putty necessarily rounded off the edges of the pores, so as to spoil a great part of the metal, by the loss of as much light and sharpness in the image as there were defective points of reflection in the metal. Besides the trouble of a great number of experiments, in order to get rid of this mischief, and to ascertain the cause to which it was owing, there was this additional inconvenience attending it, that the fault was not discovered, as before observed, till a great deal of trouble had been taken in grinding and even polishing the metal, the whole of which was rendered useless by the mortifying discovery of this defect.

I was extricated at last from this difficulty, and in some measure by accident. Having one day made a great number of experiments, and having melted down all the good copper I had or could procure; though puzzled and fatigued, yet not caring to give it up, I recollected that I had some metal which was reserved out of curiosity, and was a part of one of the bells of St. Andrew's which had been re-cast. Expecting however very little from this gross and uncertain composition, I was nevertheless determined to see what could be made of it by enriching the composition with a little fresh tin. Accordingly casting a metal with it, it turned out perfectly free from pores, and in every respect as fine a metal as ever I saw. I could not at first conceive to what this success was owing; but at last I hit upon the real cause of that defect which had given me so much embarrassment and trouble, during a course of near 100 experiments, and in consequence fell upon a method which ever after prevented it.

I had hitherto always melted the copper first, and when it was sufficiently fused, I used to add the proportional quantity of tin; and as soon as the two were mixed, and the scoria taken off, the metal was poured into the moulds. I began to consider that putty was calcined tin, and strongly suspected, that the excessive heat which the copper necessarily undergoes before fusion, was sufficient to reduce part of the tin to this state of calcination, which therefore might fly off from the composition in the form of putty, at the time the metal was poured into the flasks. On this idea, after I had furnished myself with some more Swedish copper and grain-tin, both which I had always before used, I melted the copper, and having added the tin as usual to it, cast the whole into an ingot; this was, as I expected, porous. I then melted it again, and as in this mixed state it did not acquire half the heat which was before necessary to

melt the copper alone, so it was not sufficient to calcine the tin; the speculum was then perfectly close, and free from this fault; nor did I ever after, in a single instance, meet with the abovementioned imperfection.

All that is necessary therefore to be done, to procure a metal which shall be white, and as hard as it can be wrought, and also perfectly compact, is to melt 2 lb. of Swedish copper, and when so melted, to add $14\frac{1}{2}$ oz. of grain-tin to it; then, having taken off the scoria, to cast it into an ingot. This metal must be a 2d time melted to cast the speculum; but as it will fuse in this compound state with a small heat, and therefore will not calcine the tin into putty, it should be poured off as soon as it is melted, giving it no more heat than is absolutely necessary. It is to be observed however that the same metal, by frequent melting, loses something of its hardness and whiteness: when this is the case, it becomes necessary to enrich the metal by the addition of a little tin, perhaps in the proportion of half an ounce to a pound. And indeed when the metal is first made, if instead of adding the $14\frac{1}{2}$ oz. of tin to the 2 lb. of melted copper, about one ounce of the tin were to be reserved and added to it in the succeeding melting, before it is cast off into the moulds, the composition would be the more beautiful, and the grain of it much finer: this I know by experience to be the case. The best method for giving the melted metal a good surface is this: the moment before it is poured off, throw into the crucible a spoonful of charcoal dust; immediately after which the metal must be stirred with a wooden spatula, and poured into the moulds.

The metal being cast, there will be no occasion for the complicated apparatus directed by Dr. Smith, for grinding and polishing it. Four tools are all that are necessary, viz. the rough grinder to work off the rough face of the metal; a brass convex grinder, on which the metal is to receive its spherical figure; a bed of hones which is to perfect that figure, and to give the metal a fine smooth face; and a concave tool or bruiser, with which both the brass grinder, and the hones are to be formed. A polisher may be considered as an additional tool; but as the brass grinder is used for this purpose, and its pitchy surface is expeditiously and easily formed by the bruiser, the apparatus is therefore not enlarged.

Of rough-grinding the speculum.—The tool by which the rough surface of the metal is rendered smooth and fit for the hones, is best made of lead, stiffened with about a 5th or 6th part of tin. This tool should be at least a 3d more in diameter than the metal to be ground; and for one of any size, not less than an inch thick. It may be cemented on a block of wood, to raise it higher from the bench. This leaden tool being cast, it must be fixed in the lathe, and turned as true as it is possible, by the gage, to the figure of the intended speculum, making a hole or pit in the middle, as a lodgment for the emery, of about an inch diameter for a metal of 4 inches: when this is done, deep grooves must be

cut across its surface with a graver, in the manner represented in fig. 1, pl. 2. These grooves will serve to lodge the emery, and enable the tool to cut a great deal faster. There is no occasion to fear any alteration in the convexity of this tool by working the metal on it, for the emery will bed itself in the lead, and so far arm its surface, that it will preserve its figure and cut the metal very fast. Any kind of low handle, fixed on the back of the metal with soft cement, will be sufficient; but it should cover two-thirds of its back to prevent its bending. This way of working will cut the metal faster, and with more truth, than the method described by Dr. Smith; for should the surface and rough parts be attempted to be ground off by a common grind-stone by hand, though you did it as near the gage as possible, yet the metal would be so much out of truth when applied to the succeeding tool, that no time would be saved by it. I used to employ a common labourer for this purpose, who soon acquired such a dexterity at working on this tool, that in two hours time he would give a metal of 4 inches diameter so good a face and figure, as even to fit it for the hones. When all the sand-holes and irregularities on the face of the metal are ground off, and the whole surface is smooth and regularly figured, the speculum is then ready for the brass grinder, and must be laid aside for the present.

The manner of forming the brass-grinding tool.—The following is the method I have always pursued. Procure a round stout piece of Hamburgh brass, at most a 6th part larger than the metal to be polished; and let it be well hammered into a degree of convexity (by the assistance of the gage) suitable to the intended speculum. Having done this, scrape and clean the concave side so thoroughly, that it may be well tinned all over; then cast on it, after it has been pressed a proper depth into the sand, the former composition of tin and lead, in such quantity, that it may, for a speculum of 4 inches diameter, be at least an inch and a half thick, and with a base considerably broader than the top, that may stand firmly on the bench in the manner hereafter to be described. It must then be fixed and turned in the lathe with great care, and of such a convexity as exactly to suit the concave gage, which we suppose already made. It will be necessary to be more careful in forming this than the former tool, and especially that no rings be left from the turning; then the succeeding hone tool will not require so much exactness, as any defects in turning, will, by a method hereafter mentioned, be easily remedied; but any inequality or want of truth in the brass tool will occasion a great deal of trouble before it can be ground out by the emery. This tool must have a hole, somewhat less than that in the metal to be worked on it, in the middle, quite through to the bottom. When this tool is finished off in the lathe, its diameter should be an 8th wider than the metal.

How to form the bed of hones, or the third tool.—Having chosen the kind of

hones, and the best too of the sort recommended in Smith's Optics; they should be cemented in small pieces, in a kind of pavement agreeably to his directions, on a thick round piece of marble, or metal made of lead and tin like the former composition (which is what I have always used) in such a manner, that the lines between the stones may run straight from one side to the other; so that, placing the teeth of a fine saw in each of these divisions, they may be cleared from one end to the other of the cement which rises between the stones. This bed of hones should be at least a 4th part larger than the metal to be ground on it. The surface of the metal on which the hone pavement is to be cemented may or may not, as you please, be turned of a convexity suitable to the gage, though I have never taken that trouble. As soon as the hones are cemented down, and the joints cleared by the saw, this tool must be fixed in the lathe, and turned as exactly true to the gage as possible; which done, it must be laid aside for the present. The next tool to be made is the bruiser.

The manner of forming the bruiser, the fourth and last tool.—This tool should be likewise made of thick stout brass, like the former, perfectly sound, about a quarter of an inch thick, and hammered as near to the gage as possible. It should be then scraped, cleaned, and tinned on the convex side, as the former tool was on the concave, and the same thickness of lead and tin cast on it. The general shape of this should differ from the former; for as that increased in diameter at the bottom for the sake of standing firmly, so this should be only as broad at bottom as at top, as it is to be used occasionally in both those positions. When this tool is fixed in the lathe, and turned off concave to the convex gage with great truth also, its diameter ought to be the middle size between the hones and the polisher. Having with the lathe roughly formed the convex brass-grinder, the bed of hones, and the concave bruisers, the convex and concave brass tools and the metal must be wrought alternately and reciprocally on each other with fine emery and water, so as to keep them as nearly to the same figure as possible, in order to which some washed emery must be procured. This is best done by putting it into a phial, which must be half filled with water and well shaken up, so that, as it subsides, the coarsest may fall to the bottom first, and the finest remain at the top: and whenever fresh emery is laid on the tools, the best method (which we should also observe with the putty in polishing) will be, to shake gently the bottle, and pour out a small quantity of the turbid mixture.

Of grinding the speculum, the brass tool, and the bruiser together.—All the tools being ready, on a firm post in the middle of a room, begin to grind the brass convex tool with the bruiser on it, working the latter crosswise, with strokes sometimes across its diameter, at others a little to the right and left, and always so short that the bruisers may not pass above half an inch within the surface

of the brass tool either way, shifting the bruiser round its axis at about every half dozen strokes. Also every now and then shift your own position, by walking round, and working at different sides of the brass tool; at times the strokes should be carried round and round, but not much over the tool: in short, they must be directed in such a way, and the whole grinding conducted in such a manner and with such equability, that every part of both tools may wear equally. This habit of grinding, as well as the future one of polishing, will be soon acquired. When you have wrought in this manner about a quarter of an hour with the bruiser on the tool, it will be then necessary to change them, and, placing the bruiser on its bottom, to work the convex tool on that in the same manner.

When by working in this equable manner, alternately with the bruiser and tool, and occasionally adding fresh emery, you have nearly got out all the vestiges of the turning tool, and brought them both nearly to a figure, it will be then time to give the same form to the metal. This must be done by now and then grinding it on the brass tool with the same kind of emery, taking care however, by working the two former tools frequently together, to keep all 3 exactly in the same curve. The best kind of handle for the metal is made of lead, a little more than double its thickness, and somewhat less in diameter, of about 3 lb. weight, with a hole in the middle (for reasons to be shown hereafter) a little larger than that in the metal: this handle should be cemented on with pitch. The upper edge of this weight must be rounded off, that the fingers may not be hurt; and a groove, about the size of the little finger, be turned round just below it, for more conveniently holding and taking the metal off the tools.

The manner of figuring the metal on the hones.—When the bruiser, brass tool, and metal are all brought to the same figure, and have all a true good surface, the next part of the process is to give a correct spherical figure, and a fine face to the metal on the hones. It will be necessary to premise however, that the hones should be placed in a vessel of water, with which they should be quite covered for at least an hour before they are used, otherwise they will be perpetually altering their figure when the metal comes to be ground on them. The same precaution is also necessary, if you are called off from the work while you are grinding the metal; for if they be suffered to get dry, the same inconvenience will arise.

In order to give a proper figure to the hones, and exactly suitable to that of the brass tool, bruiser, and metal, when the hones are fixed down to the block, some common flour emery (unwashed) with a good deal of water, must be put on them, and the bruiser being placed on the hones, and rubbed on them with a few strokes and a light hand, the inequalities of the stone will be quickly worn off; but as a great deal of mud will be suddenly generated, it must be washed

off every quarter of a minute with a great deal of water. By a repetition of this 2 or 3 times, the hones (being of a very soft and friable substance) will be cut down to the figure, without wearing or altering the bruiser at all. Though this business may be quickly done, and can be continued but for a few strokes at a time, I need not say that it is necessary that those strokes be carried in the same direction, and with the same care, which was observed in grinding the former tools together. As soon as the hones have received the general figure of the bruiser, and all the turning strokes are worn out from them, the emery must be carefully washed off; in order to which, it will be necessary to clear it from the joints with a brush under a stream of water. The bruiser and metal must also be cleared in the same manner, and with equal care, from any lurking particles of emery.

The hones being fixed down to the block, you now begin to work the bruiser on them with very cautious, regular, short strokes, forward and backward, to the right and left, turning the axis of the bruiser in the hand while you move round the hones, by shifting your position, and walking round the block. Indeed the whole now depends on a knack in working, which should be conducted nearly in the following manner. Having placed the bruiser on the centre of the hones, slide it in an equable manner forward and backward, with a stroke or two directly across the diameter, a little on one side, and so on the other; then shifting your position an 8th part round the block, and having turned the bruiser in your hand about as much, give it a stroke or two round and round, but not far over the edges of the hones, and then repeat the cross strokes as before: those round strokes (which ought not to be above 2 or 3 at most) are given every time you shift your own position and that of the metals, previous to the cross ones, in order to take out any stripes either in the hones or bruiser, which may be supposed to be occasioned, by the straight cross strokes. During the time of working, no mud must be suffered to collect on the hone, so as to destroy the perfect contact between the two tools; and therefore they must every now and then be washed clean by throwing some water on them. When by working in this manner all the emery strokes are ground off from the bruiser, and it has acquired a good figure and clean surface, you may then begin with the metal on the hones, in the same cautious manner, washing off the mud as fast as it collects, though that will be much less now than when the bruiser was ground on them. Every now and then however the bruiser must be rubbed gently and lightly on the hones, which will as it were, by sharpening them and preventing too great smoothness, occasion them to cut the metal much faster.

When, after having some time cautiously wrought in the manner before described, the hone-pavement has uniformly taken out all the emery strokes, and given a fine face and true figure to the metal, which will be pretty well

known by the great equality there is in the feel while you are working, and by which an experienced workman will form a pretty certain judgment; having proceeded thus far, you may then try your metal, and judge of its figure by this more certain manner. Wash the hone-pavement quite clean; then put the metal on the centre of it, and give 2 or 3 light strokes round and round only, not carrying however the edges of the metal much over the hones; this will take out the order of straight strokes: then having again washed the hones, and placed the speculum on their centre, with gentle pressure, slide it towards you till its edge be brought a little over that of the hones, then carry it quite across the diameter as far on the other side, and having given the metal a light stroke or two in this direction, take it off the tool. The metal being wiped quite dry, place it on a table at a little distance from a window; stand yourself as near the window, at some distance from the metal; and looking obliquely on its surface, turn it round its axis, and you will see at every half turn the grain given by the last cross strokes flash on your eye at once over the whole face of the metal. This is as certain a proof of a true spherical figure as the operose and difficult method described in Dr. Smith's Optics; for as there is nothing soft or elastic, either in the metal or in the hones, this glare is a certain proof of a perfect contact in every part of the two surfaces; which there could not be if the spheres were not both perfect and precisely the same.

Indeed there is one accidental circumstance which necessarily affords its aid in this and every business of the like sort, and that is, that a concave and convex surface ground together, though ever so irregular at first, will (if the working be uniform and proper, consisting, especially at last, of cross strokes in every possible direction across the diameter) be formed into portions of true and equal spheres; had it not been for this lucky necessity, it would have been impossible to have produced that correctness which is essential in the speculum of a good reflecting telescope by any mechanic contrivance whatever. For when it is considered, that the errors in reflection are 4 times as great as in refraction, and that the least defect in figure is magnified by the powers of the instrument, any thing short of perfection in the figure of the speculum would be evidently perceived by a want of distinctness in the performance. It must however be observed that both in forming the tools and at last figuring the metal (and indeed the same must be observed in the future process of polishing) that no kind of pressure is used that may endanger the bending or irregularly grinding them; they should therefore be held with a light hand, and loosely between the fingers, and the motion given should be in a horizontal direction, with no more pressure than their own dead weight.

The polishing of the speculum is the most difficult and essential part of the whole process; for every experienced workman knows, to his vexation, that the

most trifling error here will be sufficient to spoil the figure of his metal, and render all his preceding caution useless. I have however discovered a method which I shall explain, not only of giving the metal a parabolic figure, but also of recovering it when it happens to be injured; both to be effected in the act of polishing, and the former as certainly as the spherical figure is given on the hones. Indeed, if we consider rightly, polishing will be perceived to be but a kind of grinding with a finer order of strokes, and with a power infinitely finer than was before used in what is commonly called the grinding. But before describing this method, which was the result of many years experience, I will take the liberty of making some few strictures on that of Messrs. Hadley and Molyneux, which is followed by the generality of workmen.

First, then, the tool itself used by them for polishing the metal, is formed with infinite difficulty. The first described polisher is directed to be made by covering the tool with sarsenet, which is to be saturated with a solution of pitch in spirit of wine, by successive applications of it with a brush, till it is covered, and by the evaporation of the spirit of wine filled with this extract of pitch; the surface is then to be worked down and finished with the bruiser. This is all very easy in imagination; but whoever has used this method (which I have myself unsuccessfully several times) must have found it attended with infinite labour, and at last the business done in a very unsatisfactory manner; for the pitch by this process will be deprived of an essential part of its composition. The spirit of wine dissolves none but the resinous parts of its substance, which is hard and untractable; and if you use soap or spirit of wine to soften or dissolve it, it will equally affect the whole surface, the lower as well as higher parts of it. And suppose that with infinite labour with the bruiser, it is at last reduced to a fine uniform surface, it is nevertheless too hard ever to give a good polish with that lustre which is always seen in Mr. Short's, and indeed all other good metals. Nor will it give a good spherical figure; for a perfect sphere is formed, as before observed, by that intimate accommodation arising from the wear and yielding of both tool and metal; whereas in this method, there is such a stubbornness in the polisher, that the figure of the metal, good or bad, must depend on the truth of the former, which is very seldom perfect.

If the polisher be made in the 2d manner proposed, by straining the pitch through an outer covering, which is afterwards to be stripped off, the superficies of pitch and sarsenet is so very thin, that the putty, working into them, forms a surface hard and untractable, so that it is impossible to give the speculum a fine polish. Accordingly all those metals which are wrought that way have an order of scratches instead of polish, discovering itself by a greyish visible surface. Besides, supposing this tool perfectly finished, and answering its purpose ever so well, it is possible it can produce in the speculum any other than a spherical

figure; and indeed nothing else is expected from this method, as very evidently appears by the experiment recommended to ascertain the truth of the figure. You are directed to place a small luminous object in the centre of the sphere of which the metal is a segment, and then having adjusted an eye-glass at the distance of its own focal length from the object, and so situated that the image of the object formed by the speculum may be visible to the eye, you are to judge of the perfect figure of the metal by the sharpness and distinctness with which the image appears. Hence it is very evident, that as the object and image are both distant from the metal by exactly its radius, nothing but a true spherical figure of the speculum can produce a sharp distinct image; and that the image could not be distinct if the figure of the speculum were parabolic. Consequently, if the same speculum used in a telescope were to receive parallel rays, there would necessarily be a considerable aberration produced, and a consequent imperfection in the image. Accordingly, there never was a good telescope made in this manner; for if the number of degrees, or the portion of the sphere of which the great metal is a part, were as considerable as it ought to be, or as great as Mr. Short allowed in his metal, the instrument would bear but a very low charge, unless a great part of the circumference of the metal were cut off by an aperture, and the ill effects of the aberration by that means in some measure prevented. If ever a finished metal turned out without this defect, and has been found perfectly sharp and distinct, it must have been owing to an accidental parabolic tendency, no ways the natural result of the process, and therefore quite unexpected, and most probably unknown, to the workman.

Having made many efforts in the former method, which by no means pleased me for the reasons abovementioned; and having observed, from some of Mr. Short's telescopes which fell into my hands, that the high lustre of the polish could never have been produced in the manner above described, but by some softer and more tender substance; and at the same time recollecting that Sir Isaac Newton had given an account in his Optics of his having finished some metals, and considerably mended the object-glass of a refractor, by working both on a tool whose surface had been covered with common pitch about the thickness of a groat; reflecting on these matters, coarse and uncertain as this method appeared at first sight, I was determined to try whether I could not get rid of my embarrassment, by a mode of operation somewhat similar. Accordingly, shortening Dr. Smith's process, I made a set of tools in the manner before described, except that I was obliged to make some subsequent alteration in the polisher which I shall presently describe. Having given a good spherical figure to the brass tool and the bruiser, as well as to the metal on the hones, and made the brass convex tools so hot as just not to hurt the finger, I tied a lump of common pitch (which should be neither too hard nor too soft) in

a rag, and holding it in a pair of tongs over a still fire where there was no rising dust, till it was ready to strain through the linen, I caused it to drop on the several parts of the convex tool, till I supposed it would cover the whole surface about double the thickness of a shilling; then spreading the pitch as equally as I could, I suffered the polisher (by which name I shall for the future call this tool) to grow quite cold. I then warmed the bruiser so hot as almost to burn my fingers, and having fixed it to the bench with its face upwards, I suddenly placed the polisher on it, and quickly slid it off; by this means rendering the surface of the pitch more equal. The pitch is then to be wiped off from the bruiser with a little tow; and by touching the surface with a tallow candle, and wiping it a 2d time, it will be then perfectly clean and fit for a 2d process of the same sort, which must again be performed as quickly as possible; and this is ordinarily sufficient to give a general figure to the surface of the pitch. The bruiser and polisher are then suffered to get perfectly cold, when the pitch, considering what has been taken off, will be about the thickness of a shilling.

It is however here necessary to observe, that the pitch should be neither very hard and resinous, nor too soft; if the former, it will be so untractable as not to work kindly; and if too soft, it will in working alter its figure faster than the metal, and too readily fit itself to the irregularity of its figure, if it have any. When both tools were perfectly cold, I gave the polisher a gentle warmth, and then fixed the bruiser to the block with its face upwards; and (having with a large camel's-hair brush spread over the face of the polisher a little water and soap, to prevent sticking) with short, straight, and round strokes I worked it on the bruiser, every now and then adding a little more water and soap, till the pitch on the polisher had a fine surface, and the true form of the bruiser; and this I continued to do till they both got perfectly cold together: in this manner the polisher was perfectly formed in about a quarter of an hour. But here a difficulty arose: when I began to polish the metal, I found that the edge of the hole in the metal collected the pitch towards the middle of the polisher; and though in this method of working I could give an exquisite polish, as the putty lodged itself in the pitch exceedingly well, yet the figure of the metal was injured in the middle, nor did indeed the work go on with that equability which is the inseparable attendant on a good figure. In order to obviate this difficulty, I cast some metals with a continued face, the holes not going quite through, within perhaps the thickness of a six-pence. I finished 2 or 3 metals of this sort, and the work promised and went on very well; but when I came to open the holes, which I did with the utmost caution, I found the metals short of perfection; which I attributed to an alteration of the figure from the removal of even that small portion of metal after the speculum had been finished. This I suppose was in some measure the reason why I spoiled a very distinct and perfect

2 foot metal, which bore a charge of 200 times, only by opening the sharp part of the edge of the hole, because I thought it bounded the field: so essentially necessary is an exquisite correctness of figure in the speculum of a perfect reflector.

This experiment not succeeding, instead of casting the metal without a hole, I made one quite through the middle of the polisher, a little less than that in the speculum. This perfectly answered the purpose; no more inconvenience arose from the gathering of the pitch, for it had now no greater tendency to collect at the centre than the sides; and I finished several metals successively, excellent both in point of figure and polish; one of those of 2 inches diameter and 7.5 focal length, bore a charge of 60 times, and upwards. This telescope underwent Mr. Short's examination, who was pleased to remark only, that he thought he had made one more distinct.

I must observe that, in this method of working, the polishing goes on in an agreeable, uniform, and smooth manner; and that the small degree of yielding in the pitch, which is actually not more than the wearing of the metal, produces that mutual accommodation of surfaces so necessary to a true figure. In the beginning of the polish, and indeed for some time during the progress of it, always remembering now and then to move the metal round its axis, I worked round and round, not far from and always equally distant from the centre, except that every time, previous to the shifting the metal on its axis, I used a cross stroke or two; and when the polish was nearly completed, I mostly used cross strokes, giving a round stroke or two likewise every time I turned the metal on its axis. I observed in this method of working, that the metal always polished fastest in the middle; insomuch, that half or $\frac{2}{3}$ of it would be completely polished when its circumference was scarcely touched by the tool. Observing this in some of the first metals, and not considering that this way of polishing was in fact a species of grinding, and as perfect as that on the hones, I went on reluctantly with the work, almost despairing of being able to produce a good figure. However I always found myself agreeably deceived; for when the polish was extended to the edge, or within the 10th of an inch of it, I almost constantly found the figure good, and the performance of the metal very distinct. But this same circumstance of apparent defect in the metals, was in fact that to which their perfection was owing; for they all, contrary to expectation, turned out parabolic. However I did not for a great while know any certain way of giving that degree of parabolic tendency which was just necessary, and which will be described hereafter. It was a long time before I got rid of my prejudice against this apparent imperfection in the process, or could reconcile myself to the irregular manner in which the polish proceeded; for I considered it as a certain source of error, and though I saw it eventually succeed,

yet whenever I chanced to find that a metal, when first applied to the polisher, took the polish equally all over, and consequently the whole business did not take up above 10 minutes; under those circumstances, I always used to please myself with the expectation of a correct figure, at least as much so as the metal had received from the hones, where the surface was but just and equally taken off by the putty; but in this I constantly found myself deceived, and the metal turned out good for nothing. In short, at this time, though I speculatively knew that a parabolic figure was necessary to a perfect image, I yet considered it as of little practical consequence.

From the foregoing experiments, and a number of succeeding trials, I at length discovered a certain way of giving a correct parabolic figure, and an exquisite polish at the same time. This, which I have strong reasons to believe was Mr. Short's method, I will now describe in as few words as I can.

How to polish the speculum.—It is first necessary to observe that, in order to avoid the detrimental intrusions of any particles of emery, it would not be right to polish in the same room where the metal and tools were ground, nor in the same clothes which were worn in the former process; at least it would be necessary to keep the bench quite wet, to prevent any dust from rising. Having then made the polisher, by coating the brass convex tool equally with pitch, which we suppose smoothed and finished with the brass tool in the manner before described, and which is a very easy process, the whole operation is begun and finished in the following manner. The leaden weight or handle on the back of the metal should be divided into 8 parts, by so many deep strokes of a graver on the upper surface of the lead, marking each stroke with the numbers 1, 2, 3, 4, and so on, that the turns of the metal in the hand may be known to be uniform and regular. To prevent any mischief from coarse particles of putty, I always wash it immediately before using. In order to this, put about half an ounce of putty into an ounce phial, and fill it $\frac{2}{3}$ with water; then having shaken the whole, let the putty subside, and stop the bottle with a cork. In a tea-cup with a little water, there should be a full-sized camel's hair brush, and a piece of dry clean soap in a galley-pot: a soft piece of sponge will also be necessary. These, as well as the metal bruiser and polisher, should be constantly covered from dust.

The polisher being fixed down, and the camel's hair brush, being first wetted and rubbed a little over the soap, let every part of the tool be brushed over with it; then work the bruiser with short, straight, and round strokes, lightly on the tool, and continue to do so, now and then turning it, till the polisher have a good face, and be fit for the metal. Then having shaken up the putty in the phial, and touched the polisher in 5 or 6 places with the cork wetted with that and the water, place the bruiser on the tool, and give a few strokes on the putty, to rub down any gritty particles; after which, having removed it, work

the metal lightly on the polisher round and round, carrying the edges of the speculum however, not quite half an inch over the edge of the tool, and now and then with a cross stroke.

The first putty, and indeed all the succeeding applications of it, should be wrought with a considerable while; for if time be not given for the putty to bed itself in the pitch, and any quantity of it lie loose on the polisher, it will accumulate into knobs, which will injure the figure of the metal: and therefore as often as ever such knobs arise, they must be carefully scraped off with the point of a penknife, and the loose stuff taken away with the brush. After the putty is well wrought into the pitch, some more may be added in the same manner, but never much at a time, and always remembering to work on it first with the bruiser, for fear any gritty particles may find their way on the polisher. If the bruiser be apt to stick, and do not slide smoothly on the pitch, the surface of either tool may be occasionally brushed over with the soap and water, but it must be remembered that the wet brush must be but lightly rubbed on the soap.

In the beginning of this process little effect is produced, and the metal does not seem to polish fast, in some measure owing to its taking the polish in the middle, and perhaps because neither that nor the bruiser move evenly on the polisher: but a little perseverance will bring the whole into a good temper of working; and, when the pitch is well defended by the coating of the putty, the process will advance apace, and the former acquiring possibly some little warmth, the metal moves more agreeably over it, with a uniform and regular friction. All this while the metal must have no more pressure than that which it derives from its own weight and that of the handle; and the polisher must never be suffered to get dry, but as often as it has any tendency to do so, the edges of it must be moistened with the hair pencil; and now and then, even when fresh putty is not laid on, the surface of the polisher should be touched with the brush to keep it moist.

When the polish of the metal nearly reaches the edge (for it always, as before said, begins in the middle) you must alter the method of working; for now the round strokes must be gradually altered for the short and straight ones. Supposing then you are just beginning to alter them; after having put on fresh putty, and gently rubbed it with 2 or 3 strokes of the bruiser, you place the metal on the tool, and after a stroke or two round and round, give it a few forward and backward, and from side to side, but with the edges very little over the tool; then having turned the metal $\frac{1}{8}$ round in your hand, and moved yourself as much round the block (which must be remembered throughout the whole process) you go on again with a stroke or two round, to lead you only to the cross strokes, which are now to be principally used, and with more boldness.

After this has been done some time, the metal will begin to move stiffly as the friction now increases, and the speculum polishes very beautifully and fast; and the whole surface of the polishing tool will be equally covered over with a fine metallic bronze. The tool even now must not be suffered to become dry; a single round stroke in each of your stations and turnings of the metal will be sufficient, and the rest must all be cross ones, for we are completing a circular figure. You must now be very diligent, for the polisher drying, and the friction increasing very fast, the business of the spherical figure is nearly at an end. As the metal wears much, its surface must be now and then cleaned, with a piece of shammy leather, from the black stuff which collects on it; and the polisher likewise from the same matter, with a soft piece of wet sponge. You will now be able to judge of the perfect spherical figure of the metal and tool, when there is a perfect correspondence between the surfaces, by the fine equable feel there is in working, which is totally free from all jerks and inequalities. Having proceeded thus far, you may put the last finishing to this figure of the metal by bold cross strokes, only 3 or 4 in the directions of each of the 8 diameters, turning the metal at the same time: this must be done quickly, for it ought, in this part of the process particularly, to be remembered, that if you permit the tool to get quite dry, you will never be able, with all your force, to separate that and the metal, without destroying the polisher by heat. The metal has now a beautiful polish and a true spherical figure, but will by no means make a sharp distinct image in the telescope: for the speculum (if it be tried in the manner hereafter recommended) will not be found to make parallel rays converge without great aberration; indeed the deviation will be so great, as to be very sensibly perceived by a great indistinctness in the image.

How to give the parabolic figure to the metal.—In order then to give the speculum the last and finishing figure, which is done by a few strokes, it must be particularly remarked, that by working the metal round and round, the sphere of the polisher by this means becoming less, it wears fastest in the middle: and as a segment of a sphere may become parabolic, by opening the extremes gradually from within outwards, so it may be equally well done by increasing the curvature in the middle, in a certain ratio, from without inwards. Supposing then the metal to be now truly spherical, stop the hole in the polisher by forcing a cork into it underneath, about an inch, so that it do not reach quite to the surface; and having washed off any mud that may be on the surface of the tool with a wet soft piece of sponge, while its surface is a little moist, place the centre of the metal on the middle of the polisher; then having, with the wet brush, lodged as much water round the edge of the metal as the projecting edge will hold, fill the hole of the metal and its handle with water, to prevent the evaporation of the moisture, and the consequent adhesion between the speculum and

polisher, and let the whole rest in this state 2 or 3 hours: this will produce an intimate contact between the two, and by parting, with any degree of warmth they may have acquired by the vicinity of the operator, they will become perfectly cold together. By this time you may push out the cork from the polisher, to discharge the water, and give the metal the parabolic figure in the following manner. Move the metal gently and slowly at first, a very little round the centre of the polisher (indeed after this rest it will move stiffly) then increasing by degrees the diameter of these strokes, and turning the metal frequently round its axis, give it a larger circular motion, and this without any pressure but its own weight, and holding it loosely between the fingers: this manner of working may safely be continued about 2 minutes, moving yourself as usual round the block, and carrying the round strokes in their increased and largest state, not more than will move the edge of the metal half an inch or $\frac{5}{8}$ over the tool. The speculum must not all this while be taken off from the polisher; and consequently no fresh putty can be added. It will not be safe to continue this motion longer than the time abovementioned; for if the parabolic tendency be carried the least too far, it will be impossible to recover a true figure of that kind but by going through the whole process for the spherical one in the manner before described, by the cross strokes on the polisher, which takes a great deal of time. However, when there is occasion, it may be done; and I have myself several times recovered the circular figure, when I had inadvertently gone too far with the parabolic; and ultimately finished the metal on the polisher without the use of the hones.

To try the true figure of the metal.—It will now be proper to try the figure of the speculum, and that is always best done by placing it in the telescope it is intended for. In order to this, I use the instrument as a kind of microscope, placing the object however at such a distance that the rays may be nearly parallel. At about 20 yards a watch-paper, or some such object, on which there are some very fine hair strokes of a graver, is fixed up. The lead must be then taken off from the back of the speculum; which is best done by placing the edge of a knife at the junction of the lead and metal, when, by striking the back of it with a slight blow, the pitch immediately separates, and the handle drops off; the remaining pitch may be scraped off with a knife, taking care that none of the dust stick to the polished face of the metal.

Having placed the speculum in the cell of the tube, and directed the instrument to the object, make an annular kind of diaphragm with card-paper, so as to cover a circular portion of the middle part of the metal between the hole and the circumference, equal in breadth to about an 8th part of the diameter of the speculum: this paper ring should be fixed in the mouth of the telescope, and remain so during the whole experiment, for the part of the metal covered by it

is supposed to be perfect, and therefore unemployed. There must be 2 other circular pieces of card-paper cut out, of such sizes, that one may cover the centre of the metal by completely filling the hole in the last described annular piece; and the other, such a round piece as shall exactly fit into the tube, and so broad as that the inner edge may just touch the outer circumference of the middle annular piece. It would be convenient to have these last 2 pieces so fixed to an axis that they may be put in their places, or removed so easily as not to displace or shake the instrument. All these pieces therefore together will completely shut up the mouth of the telescope.

Let the round piece which covers the centre of the metal, or that which has no hole in it, be removed; and, by a nice adjustment of the screw, let the image, which is now formed by the centre of the mirror, be made as sharp and distinct as possible. Then, every thing else remaining at rest, replace the central piece, and remove the outside annular one, by which means the circumference only of the speculum will be exposed, and the image now formed will be from the rays reflected from the outside of the metal. If there be no occasion to move the screw and small metal, and the 2 images formed by these 2 portions of the metal be perfectly sharp and equally distinct, the speculum is perfect, and of the true parabolic curve; or at least the errors of the great and little speculum, if there be any, are corrected by each other. If, on the contrary, under the last circumstance, the image from the outside of the metal should not be distinct, and it should become necessary, in order to make it so, that the little speculum be brought nearer, it is plain that the metal is not yet brought to the parabolic figure; but if, on the other hand, in order to procure distinctness, you be obliged to move the little speculum farther off, then the figure of the great speculum has been carried beyond the parabolic, and has assumed an hyperbolic form. When the latter is the case, the circular figure of the metal must be recovered, after having fixed on the handle with soft pitch, by bold cross strokes on the polisher, finishing it again in the manner above described. If the speculum be not yet brought to the parabolic form, it must cautiously have a few more round strokes on the polisher; indeed a very few of them in the manner before described make in effect a greater difference in the speculum than would be at first imagined. If a metal of a true spherical figure were to be tried in the abovementioned manner in the telescope, which I have frequently done, the difference of the foci of the 2 segments of the metal would be so considerable, as to require 2 or 3 turns of the screw to adjust them; so very great is the aberration of a spherical figure of the speculum, and so improper to procure that sharpness and precision so necessary to a good reflecting telescope.

This is by no means the case with the object glasses of refractors; for besides that they are in fact never so distinct as well finished reflectors, the apertures of

them are so exceedingly small, compared to the latter, and the number of degrees employed so very small, that the inconvenience of a spherical figure is not so much perceived. Accordingly we observe in the generality of reflectors (whose specula, unless by accident, are always spherical) that the only true rays which form the distinct image arise from the middle of the metal: and unless the defect be remedied by a considerable aperture, which destroys much light, the false reflection from the inside of the metal produces a greyish kind of haziness, which is never seen in Mr. Short's, or indeed in any good telescopes.

Supposing that the 2 foci of the different parts of the metal perfectly coincide, and that, by the union of them when the apertures are removed, the telescope shows the objects very sharp and distinct, you are not however even then to conclude that the instrument is not capable of further improvement; for you will perceive a sensible difference in the sharpness of the image, under different positions of the great speculum with respect to the little one, by turning round the great metal in its cell, and opposing different parts of it to different parts of the little metal, correcting by this means the error of one by the other. This attempt should be persevered in for some time, turning round the great speculum about $\frac{1}{8}$ at a time, and carefully observing the most distinct situation each time the eye-piece is screwed on: when, by trying and turning the great metal all round, the distinctest position is discovered, the upper part of the metal should be marked with a black stroke, that it may always be lodged in the cell in the same position. This is the method Mr. Short always used; and the caution is of so much consequence, that he thought it necessary to mention it very particularly in his printed directions for the use of the instrument. Mr. Short also frequently corrected the errors of the great by the little metal in another way. If the great speculum did not answer quite well in the telescope, he cured that defect sometimes by trying the effect of several metals successively, by this means correcting the errors of one by the other; for in several of his telescopes which have passed through my hands, when the sizes and powers have been the same, I have found that the great metals, though very distinct in their proper telescopes, yet have, when taken out and changed from one to the other, spoiled both telescopes, rendering them exceedingly indistinct, which could arise from no other circumstance. For this reason I suppose it was, that he kept ready finished, a great many large metals of the same focal length, so that, when he wanted to mount a telescope, he might from a great choice, be able to combine those metals which suited each other best. I am strongly inclined to believe this was the case, not only from the above observation, but because he showed me himself a box of finished metals, in which I am sure there were a dozen and a half of the same focal length.

There is another circumstance, and a material one, which must not be omitted;

it is this: for the very same reason that the pitch should not be too hard or soft, the work will not proceed well in the heat of summer, or the cold of winter: in the latter, it may be possible to remedy the defect by having the room warmed with a stove; and in the summer, the other inconvenience may perhaps be avoided by using a harder kind of pitch; but I much doubt in either case whether the work will go on so kindly: I have myself always wrought in spring and autumn. The process of polishing, and indeed grinding on the hones, will not go on so well if it be not continued uninterruptedly from beginning to end; for if the work of either kind be left but for a quarter of an hour, and you then return to it again, it will be some time before the tool and metal can get into a kindly way of working; and till they do, you are hurting what was done before.

Magnifying very minute objects, and particularly reading at a distance, have been generally considered as the surest tests of the goodness of a telescope; and indeed when the page is placed at a great distance, so that the letters subtend but a very small angle at the eye, if then they appear with great precision and sharpness, it is most probable that the instrument is a good one. But yet we are sometimes apt to be deceived by this method; nor is it always possible to determine on the different merits of 2 instruments of equal power, by this mode of examination; for when the letters are removed to the utmost extent of the powers of the 2 instruments, the eye is apt to be prejudiced by the imagination. If 2 or 3 words can be here and there made out, all the rest are guessed at by the sense; insomuch that an observer, zealous for the honour of his instrument, is very apt to deceive himself in spite of his intentions. The surer test is by figures, where you can procure no aid from this sort of deception. In order to examine my reflecting telescopes, I made, on a piece of copper and on a black ground, 6 lines consisting of about 12 pieces of gold figures, and each line of figures differing in magnitude, from the smallest that could be distinctly made, to those of about $\frac{2}{10}$ of an inch long; besides, the figures in the several lines were differently disposed, and the sum of each line also differed. It is evident that by this method all guess is precluded; and that, of 2 instruments, of the same powers, that which can make out the least order of figures, which will be known by the sum, is the best telescope. Such a plate I caused to be fixed up for experiments against the top of a steeple, about 300 yards north of my house; and it will serve to give some idea of the distinctness with which very small figures could be made out at that distance, by saying, that in a clear state of the air, and with the sun behind me, with a telescope of 18 inches focal length. I have seen the legs of a small fly, and the shadows of them, with great precision and exactness.

I cannot conclude without indulging myself in an observation on the amazing sagacity of Sir Isaac Newton, in every subject on which he thought fit to

employ his attention. It was he who first proposed, and indeed practised, the polishing with pitch; a substance which at first sight perhaps every one but himself would have thought very improper, from its softness, to produce that correctness of figure so necessary on these occasions; and yet I believe that it is the only substance in nature that is perfectly well calculated for the purpose; for at the same time that it is soft enough to suffer the putty to lodge very freely on its surface, and for that reason to give a most tender and delicate polish; it is likewise totally inelastic, and therefore never, from that principle, suffers any alteration in the figure you give it. If the first makers of the instrument therefore had given proper credit to, or had simply followed the hint Sir Isaac gave, it would have saved them infinite trouble, and they would have produced much better instruments; but the pretended refinement, of drawing a tincture from pitch with spirits of wine, affords you only the resinous, hard, and untractable part of the pitch, divested of all that part of its original substance which is necessary to give it that accommodating pliability in which its excellence consists.

It is needless to swell this account with a detail of the process for polishing the little speculum, as it must be conducted in the same manner which has been already described in that of the large one; only observing, that as the little metal has an uninterrupted face, without a hole, so there is no occasion for one in the polisher; and also that, as a spherical figure is all that need here be practically attempted, so the difficulty in finishing is infinitely short of that of the other.

As it is always necessary to solder to the back of the little speculum a piece of brass, as a fixture for the screw to adjust its axis, I shall just hint a safe and neat method of doing it, which may be very useful to the optical or mathematical instrument maker on other occasions. Having cleaned the parts to be soldered very well, cut out a piece of tin foil the exact size of them; then dip a feather into a pretty strong solution of sal ammoniac in water, and rub it over the surfaces to be soldered; after which place the tin foil between them as fast as you can (for the air will quickly corrode their surfaces so as to prevent the solder taking) and give the whole a gradual and sufficient heat to melt the tin. If the joints to be soldered have been made very flat, they will not be thicker than a hair: though the surfaces be ever so extensive, the soldering may be conducted in the same manner, only care must be taken, by general pressure, to keep them close together. In this manner, for instance, a silver graduated plate may be soldered on to the brass limb of a quadrant, so as not to be discernible by any thing but the different colour of the metals. This method was communicated to me by the late Mr. Jackson, who during his life kept it a secret, as he used it in the construction of his quadrants, and is, I believe, not as yet known to any workman.

P. S. It was some time after I had written the above account that I saw Mr. Short's method of polishing object glasses for refracting telescopes, published in the Transactions. By that paper I find that what I before strongly suspected,

is really the case, viz. that he knew how well pitch was calculated for purposes of this kind. Only it may be remarked, that as glass is much harder, polishes much slower, and consequently does not wear away and alter its figure so soon as the metal of which the speculum is made; and as at the same time (on account of the very small apertures allowed to telescopes of this sort) nothing more than a spherical figure is proposed; he is therefore obliged to use pitch in a hard, friable, and stubborn state; whereas, considering the delicate substance of the metal speculum, and the figure intended to be given to it, the soft pitch of the common sort, by suffering the putty to bed itself in its substance, produces the most beautiful polish; and by its pliability is better calculated for that mutual accommodation between polisher and metal, so necessary to the figure proposed.

Explanation of the Figures — Pl. 2, fig. 1 shows the grinder for working off the rough face of the metal; the black strokes represent deep grooves made with a graver. Fig. 2 the bed of hones, to complete the spherical figure of the speculum, and to render its surface fit for the polisher. Fig. 3 an apparatus for examining the parabolic figure of the speculum. *AA* The mouth of the telescope, or edge of the great tube. *BB* A thin piece of wood fastened into, and flush with the end of the tube; to which is permanently fixed the annular piece of paste-board *CC*, intended to cover, and to prevent the action of the corresponding part of the speculum. *DD* Another piece of paste-board, fixed by a pin to the piece of wood *BB*, on which it turns as on a centre; so that the great annular opening *HH* may be shut up by the ring *FF*, or the aperture *GG* by the imperforate piece *E* in such manner that, in the first instance, the reflexion may be from the centre, and in the latter from the circumference, of the great speculum.

XVII. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1776. By Thomas Barker, Esq. p. 350.

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the House.			Abroad.			
					High.	Low.	Mean.	High.	Low.	Mean.	
Jan.	Morn.	29.65	28.88	29.27	42½	23	32½	42	10	26	2.511
	Aftern.				43	26	33	45	16	29	
Feb.	Morn.	29.50	28.24	28.89	44½	24	39	44	11	35	3.195
	Aftern.				45	25	40	45½	27	41½	
Mar.	Morn.	29.95	28.50	29.46	53½	38½	44½	47½	27	38	1.518
	Aftern.				55	40	46	62½	37	48½	
Apr.	Morn.	29.94	29.00	29.58	54½	43	50	52	31	42½	0.887
	Aftern.				57½	44	51½	64½	42	55	
May	Morn.	29.99	28.73	29.57	62½	47	51½	58	38	47	1.627
	Aftern.				66	48	53	76	38	57	
June	Morn.	29.87	29.04	29.42	66	55½	59	62	48½	54	2.485
	Aftern.				69½	57½	61	75	54	64½	
July	Morn.	29.86	29.03	29.54	65	57	62	63½	53	58	1.850
	Aftern.				68½	59½	64	80	65	69½	
Aug.	Morn.	29.83	29.00	29.41	70	57	61	63½	46	54	5.200
	Aftern.				75	59	62½	82½	58½	66	
Sept.	Morn.	29.93	28.75	29.40	62	52½	57½	60	37	49½	2.452
	Aftern.				64	54½	58½	69	52½	61	
Oct.	Morn.	29.87	29.06	29.56	57½	48	52½	53	36	45½	2.061
	Aftern.				58	48	53½	62	48	54	
Nov.	Morn.	29.85	28.60	29.42	52	37	45½	54	26½	38½	2.823
	Aftern.				52	37	46	54	32½	44	
Dec.	Morn.	29.94	28.73	29.43	48½	34	42½	49	21½	37	1.233
	Aftern.				49	34	43	52	26½	40½	
Mean of all				29.40			50½			49	27.842

*XVIII. Extract of a Meteorological Journal for the Year 1776, kept at Bristol.
By Samuel Farr, M. D. p. 353.*

An abridged Table of the Winds, &c. for Bristol, for the Year 1767.

Mths.	Barometer.			Rain.											Frosty days.	Fair days.	Thunder, &c.
	High.	Low.	Mean.	Inches.	N	E	W	S	NW	SE	NE	SW					
Jan. .	29.93	29.04	29.53	3.993	Jan. .	0	1	0	0	0.5	3	23.5	3	25	6		
Feb. .	29.74	28.66	29.21	5.538	Feb. .	0	0	0	1.5	1	3.5	2.5	20.5	1	5.5	5. S.W.	
March	30.28	28.80	29.53	1.643	Mar ch	0	1.5	0	1.5	1.5	3	7.5	17	2.5	13.5		
April	30.28	29.26	29.85	0.438	April	1.5	1	0.5	2	4	2	10.5	8.5	1	18		
May .	30.30	29.10	29.88	1.149	May .	2.5	2	0.5	2.5	4	1	11.5	7	0	13.5		
June..	30.20	29.38	29.88	2.554	June..	0.5	0.5	0	3	2	5	5.5	13.5	0	13.5	13. S.	
July..	30.16	29.30	29.76	2.332	July..	0	0	0.5	3	3	5	1	18.5	0	13	5. S.E. 19 S.E.	
Aug..	30.08	29.30	29.68	4.747	Aug..	0.5	0.5	0.5	1	2	6.5	3	14	1	11.5	6. N.W. 16. S.W. 30. S.W.	
Sept..	30.20	20.17	29.72	3.270	Sept..	0	0	0	0.5	4	3	10	12.5	5	15.5	4. N.W. 8. N.W.	
Oct...	30.18	29.60	29.87	1.686	Oct. .	0	1.5	0	1.5	3.5	4	13	7.5	4	6		
Nov...	30.15	28.90	29.74	2.283	Nov..	1	0	0.5	0	2	13	1.5	12	5	10.5	30. S.E.	
Dec...	30.28	29.26	29.76	1.422	Dec...	3.5	0.5	0.5	0.5	3.5	10.5	2	10	9	11	1. S.E.	
Mean of all				29.70 31.055		9.5	8.5	3.0	17.0	31.0	59.5	91.5	144.0	53.5	137.5		

*XIX. Meteorological Journal kept at the House of the R. S., by order of the
President and Council. p. 357.*

1776.	Thermometer without.			Thermometer within.			Barometer.			Rain. Inches.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
January ..	44.5	13.5	29.3	43.5	20.5	31.8	30.14	29.21	29.687	1.167
February ..	49.5	14.5	42.6	46.0	19.0	42.4	29.97	28.84	29.408	3.510
March	52.0	30.5	45.9	56.0	37.0	46.4	30.43	28.98	29.91	1.541
April.	56.5	34.5	50.3	58.0	41.0	51.2	30.44	29.46	29.542	0.320
May	62.0	40.0	54.3	66.0	47.0	54.9	30.46	29.14	29.999	1.594
June	66.0	32.5	62.4	70.5	54.5	64.2	30.34	29.48	29.87	2.028
July	70.0	55.0	65.9	70.0	60.0	68.3	30.31	29.55	29.913	1.752
August	67.0	53.0	64.2	72.5	58.0	66.1	30.16	29.53	29.546	2.540
September	61.5	43.0	57.2	64.5	52.0	66.1	30.35	29.26	29.83	2.712
October ..	58.0	42.0	53.8	65.0	51.0	53.4	20.32	29.65	30.00	0.734
November	57.0	28.0	45.0	58.0	37.0	49.9	30.39	29.11	29.878	2.191
December	52.0	25.0	42.0	57.5	30.5	45.0	30.42	29.16	29.882	1.275
Whole year			51.1			52.9			29.789	21.364

Of the magnetic needle, the mean declination or variation was $21^{\circ} 47'$; and the mean inclination or dip $72^{\circ} 30'$.

XX. Of a Volcanic Hill near Inverness. By Thomas West, Esq. p. 385.

The hill is about a mile and a half from Inverness, and is called by the inhabitants Creck Faterick, or Peter's Rock: the lower part of it is chiefly ploughed up: the upper part is extremely steep, craggy, and very difficult of access, and appears to have evident marks of having been a volcano, as by much the greatest part of the largest rocks on it have been either strongly calcined or fused, as will plainly appear by some specimens picked up on the sides of the hill, others broken off from the solid rock with a pick-axe, though not without great difficulty, on

account of the extreme hardness of the rock; and others dug out from the summit of the hill, at the depth of 4 feet, lying in a rich, light, black mould, which, after having been exposed to the air for some time, turned to a whitish ash-colour. On the summit of this hill is a small plane, 90 paces long, by 27 wide, surrounded by rocks, from 6 to 8 feet high, like a breast-work, extremely craggy, and difficult of access on the outside, but rising from the centre, in the inside, to the top of the breast-work, with a gentle slope of turf, equal to any of the finest on the sheep downs.*

XXI. New Electrical Experiments and Observations; with an Improvement of Mr. Canton's Electrometer. By Mr. Tib. Cavallo. p. 388.

See Mr. Cavallo's Treatise on Electricity, vol. 2, for this paper.

XXII. Barometrical Observations on the Depth of the Mines in the Hartz. By John Andrew de Luc, F. R. S. p. 401. From the French.

These mines are extremely deep; and Mr. de L. was desirous to try in them his rules for measuring heights by the barometer, that he might know whether in those pits, where exhalations of all kinds spread themselves, the condensations of the air follow the same laws that they do out of them. And he had the satisfaction to find that they answered in the Hartz, just as they had done on the mountains in the neighbourhood of Geneva, where they took their origin. A remarkable circumstance which relates to the barometer itself is as follows. Having occasion for corresponding observations in some places of his route, he applied to observers who had good barometers; among which was one of Mr. Dollond's. These barometers he compared with his own, being well assured beforehand that he should find a difference in the heights indicated, from the circumstance of their having cisterns at the bottom, which makes the barometrical column always shorter in these than it is in a plain tube in the form of a siphon. Accordingly this was the case in all these barometers; they all stood lower than Mr. de Luc's, but varied from each other according to particular circumstances, depending chiefly on the diameter of the tube, and the figure of the cistern.

The first of his observations was made in 3 mines near Clausthal, called the Dorothea, the Caroline, and the Benedict; the depths of which were found to be as follows, viz.

The depth of the Dorothy pit between two fixed points, 168.96 French toises; that of the Caroline, relatively to the same point at the mouth, 170.74; that of the lowest searching gallery of the Benedict, relatively to the same point, 143.96: all agreeing within 1 or 2 toises of the geometrical measurements.

* The specimens sent with the above letter to the R. S., having been examined by some members well acquainted with volcanic productions, were by them judged to be real lava; and it was their opinion, that if a great quantity of the like substance be found on the hill from which these pieces were taken, the hill most probably owes its origin to a volcano. J. P.—Orig.

Mr. de L. was surprised to have come so near to the geometrical measures, as he had imagined, that the exhalations of all kinds in the mines must in some measure affect the common laws of the air's elasticity in different degrees of heat, if not its absolute elasticity. On reflecting however on this singular conformity of the air in mines with the external air, he soon found the cause of it in the extreme care taken to procure a circulation of external air in the mines, in order to prevent the pernicious effects of the exhalations; so that the same means, which really preserve the health of the miners in their subterraneous abodes, seem to give the air which circulates in them, and more especially that of the pits in which are the principal currents, the properties of the external air as to barometrical measurements. Doubtless this is the cause of that interesting phenomenon, as satisfactory for the security it gives to the lives of the miners, as for the application of the laws of aërometry: this was again confirmed by observations Mr. de L. made some days after in other mines, where indeed he met with some irregularities, but not such as might have been expected from barely considering the local circumstances.

These mines are in the Ramelsberg near Goslar. The ore that is chiefly extracted from them, as well as from those of Clausthal, is lead; but they are worked in another manner. The vein of ore, which is near 18 toises broad, is extremely impregnated with pyrites: insomuch that, when it is heated, the vapour of the sulphur, which disengages itself, bursts the stone, which falls down in large fragments. The method therefore is to light great fires against the rock; and, when they are extinguished, the miners assist with their instruments the fall of the stones, that may still remain suspended. The following are the results of his observations taken there, viz.

The height of the gallery of Breitling, above the bottom of the pit of Kaunkuhl, 44.41 French toises; the height of the entry of the mines, above the gallery of Breitling, 27.04; the height of the top of the pit of Kaunkuhl, above the entry of the mines, by external observations, 41.27; therefore the depth of the pit of Kaunkuhl, measured in 3 parts, one of them without the mines, 112.72; and the depth of the same pit, determined by immediate observations made at the top and the bottom, 113.13; agreeing within a toise of the geometrical measurement.

Having made these experiments within the mines, Mr. de L. was desirous of making some in the open air, which he had soon a very agreeable opportunity of doing; for the principal of the subterraneous geometers having had occasion to determine most accurately the height of 2 external points of the Hartz, relatively to the mines of Clausthal and Zellerfeld; nothing more was required but to observe the barometer at the entry of a certain mine, which was a fixed point, and to observe it again at these 2 external points; one of which was about 3000 toises horizontal distance, beyond a small hill; and the other 5000 toises off, entirely without the Hartz. On carrying this project into execution, Mr. de L. found the following heights by his calculations, viz.

Height of the entry of the mine, called Alte Seegen, above a certain point in the valley of Bremeke, 102.18 French toises; height of the entry of the same mine, above another point near Lasfelde, in the valley of Osterode, 173.81.

One of these barometrical measures, taken in open air, was found to agree very nearly with the geometrical measure; and the other differed only $1\frac{1}{2}$ toise in excess.

A twisted brass wire 5 toises long, 2 puncheons, a semicircle, and a compass, are all the instruments made use of by the subterraneous geometer. By means of the 2 puncheons, he extends his wire in the direction of the way which he is measuring; and by practice he acquires a habit of always stretching it to the same degree. His semicircle, which is very light, being suspended at the middle of the wire, shows him its inclination. By this means he has a right-angled triangle, of which the hypotenuse and angle at the base are known. He has consequently the vertical height and horizontal distance gone over. After this he suspends his compass to the wire, to find out its declination, and consequently the direction of his horizontal line. It is in this manner that he draws the plan and section of these subterraneous labyrinths. It is likewise by this means that he goes over hills and vales, in order to determine points corresponding to his pits and galleries. But is this a method that may safely be depended on? The fact answers, and saves the trouble of long reasonings. A miner, solely on the faith of his geometer, and in the absolute obscurity of the entrails of the earth, undertakes a labour that is to cost him years, in daily boring through a rock. Another miner sets out to meet him from some other mine, or from without. At the end of a determined measure, the gnomes begin to hear each other, and at length they meet. Having observed some of these points of rencounter in the galleries; it is sometimes difficult to perceive the small winding which has been necessary for their meeting end to end.

Mr. de L. communicates some other barometrical observations, not verified by geometrical survey; by which he determined the height of some points of the Hartz relatively to the plain, and chiefly the highest point. This greatest elevation, called the Blocksberg or Brocken, is situated in the estate of Count de Verniguerode. And the result of the observations was as follows, viz.

The small hut, at the summit of the Brocken, above Oder-brucke, 172.93 French toises; Oder-brucke above Clausthal, 91.39; Clausthal above Gottingen, 210.21; Gottingen above Hanover, 56.45; total elevation of the Brocken above Hanover, 530.98.

It will be easy to come at the elevation of Hanover above the level of the sea, in order to complete this measurement; corresponding observations of the barometer will be sufficient for that purpose. In the mean time it is easy to estimate, either by the barometer itself (the mean height of which during the month of October was 30.1 English inches on a 2d floor) or by the small declivity of the rivers between Hanover and the sea, that the elevation of Hanover above that level is not very considerable.

XXIII. The General Mathematical Laws which regulate and extend Proportion Universally; or, a Method of Comparing Magnitudes of any Kind together, in all the possible Degrees of Increase and Decrease. By James Glenie, A.M. and Lieut. in the Artillery. p. 450.

The doctrine of proportion laid down by Euclid, and the application of it given in his Elements, form the basis of all the geometrical reasoning used by mathematicians, both ancient and modern. But the reasonings of geometers with regard to proportional magnitudes have seldom been carried beyond the triplicate ratio, the proportion that similar solids have to each other, when referred to their homologous linear dimensions. This boundary however comprehends but a very limited portion of universal comparison, and almost vanishes into nothing when referred to that endless variety of relations, which must necessarily take place between geometrical magnitudes, in the infinite possible degrees of increase and decrease. The first of these takes in but a very contracted field of geometrical comparison; whereas the last extends it indefinitely. Within the narrow compass of the first, the ancient geometers performed wonders, and their labours have been pushed still further by the ingenuity and industry of the moderns. But no author, that I have been able to meet with, gives the least hint or information with regard to any general method of expressing geometrically, when any two magnitudes of the same kind are given, what degree of augmentation or diminution any one of these magnitudes must undergo, in order to have to the other any multiply or sub-multiply ratio of these magnitudes in their given state; or any such ratio of them as is denoted by fractions or surds; or, to speak still more generally, a ratio which has, to the ratio of the first-mentioned of these magnitudes to the other, the ratio of any two magnitudes whatever of the same, but of any kind. Neither have I been able to find that any author has shown geometrically in a general way, when any number of ratios are to be compounded or decompounded with a given ratio, how much either of the magnitudes in the given ratio is to be augmented or diminished, in order to have to the other a ratio, which is equal to the given ratio, compounded or decompounded with the other ratios. To investigate all these geometrically, and to fix general laws in relation to them, is the object of this paper; which, as it treats of a subject as new as it is general, I flatter myself, will not prove unacceptable to this learned Society. It would be altogether superfluous to mention the great advantages that must necessarily accrue to mathematics in general, from an accurate investigation of this subject, since its influence extends more or less to every branch of abstract science, when any data can be ascertained for reasoning from. I shall, in a subsequent paper, take an opportunity of showing how, from the theorems afterwards delivered in this, a method of reasoning with finite magnitudes, geometrically, may be derived, without any

equal to LR, KN has to NO a ratio compounded of the ratios of LN to NO and NR to OP; that is, of the ratios A to B, C to D, and E to F. Therefore a magnitude of the same kind with A and B, which has to B the ratio compounded of these ratios, is expressed by $A + A \cdot \frac{C-D}{D} + A \cdot \frac{E-F}{F} + A \cdot \frac{C-D}{D} \cdot \frac{E-F}{F}$.

Again, if NR, OP, be supposed to represent G, H, respectively, and KV a 4th proportional to OP, KN, and QR; VQ will be equal to KR (14 E 6) and consequently VN will have to NO a ratio compounded of the ratios of KN to NO and NR to OP; that is, of the ratios A to B, C to D, E to F, G to H. But VK is by construction equal to $A \cdot \frac{G-H}{H} + A \cdot \frac{C-D}{D} \cdot \frac{G-H}{H} + A \cdot \frac{E-F}{F} \cdot \frac{G-H}{H} + A \cdot \frac{C-D}{D} \cdot \frac{E-F}{F} \cdot \frac{G-H}{H}$.

And this added to KN, above found, gives $A + A \cdot \frac{C-D}{D} + A \cdot \frac{E-F}{F} + A \cdot \frac{C-D}{D} \cdot \frac{E-F}{F} + A \cdot \frac{C-D}{D} \cdot \frac{E-F}{F} + A \cdot \frac{C-D}{D} \cdot \frac{G-H}{H} + A \cdot \frac{E-F}{F} \cdot \frac{G-H}{H} + A \cdot \frac{C-D}{D} \cdot \frac{E-F}{F} \cdot \frac{G-H}{H}$, for the magnitude of the same kind with A and B, which has to B, the ratio compounded of the ratios A to B, C to D, E to F, G to H; whence the law of continuation is manifest. The same conclusions may be derived from (E. 5); so that no principle can be simpler or more geometrical than that here made use of.

Thus then these magnitudes will stand:

1. $A + A \cdot \frac{C-D}{D}$, when two ratios are compounded.
2. $A + A \cdot \frac{C-D}{D} + A \cdot \frac{E-F}{F} + A \cdot \frac{C-D}{D} \cdot \frac{E-F}{F}$, when three are compounded.
3. $A + A \cdot \frac{C-D}{D} + A \cdot \frac{E-F}{F} + A \cdot \frac{G-H}{H} + A \cdot \frac{C-D}{D} \cdot \frac{G-H}{H} + A \cdot \frac{E-F}{F} \cdot \frac{G-H}{H} + A \cdot \frac{C-D}{D} \cdot \frac{E-F}{F} \cdot \frac{G-H}{H}$, when four ratios are compounded, &c. &c.

By continuing this operation much further, I found on examination that the number of terms in which A is connected with the differences C - D, E - F, G - H, &c. taken 1 by 1, 2 by 2, 3 by 3, &c. if p denote the number of ratios compounded, is expressed respectively by $\frac{p-1}{1}$, $\frac{p-1}{1} \cdot \frac{p-2}{2}$, $\frac{p-1}{1} \cdot \frac{p-2}{2} \cdot \frac{p-3}{3}$, &c. Thus if the ratio of A to B be supposed equal to the ratios of C to D, E to F, G to H, &c. respectively, these expressions will give the following ones:

$$1. A + \frac{2-1}{1} \cdot A \cdot \frac{A-B}{B}.$$

$$2. A + \frac{3-1}{1} \cdot A \cdot \frac{A-B}{B} + \frac{3-1}{1} \cdot \frac{3-2}{2} \cdot A \cdot \frac{(A-B)^2}{B^2}.$$

$$3. A + \frac{4-1}{1} \cdot A \cdot \frac{A-B}{B} + \frac{4-1}{1} \cdot \frac{4-2}{2} \cdot A \cdot \frac{(A-B)^2}{B^2} + \frac{4-1}{1} \cdot \frac{4-2}{2} \cdot \frac{4-3}{3} \cdot A \cdot \frac{(A-B)^3}{B^3};$$

for magnitudes of the same kind with A and B, which have to B respectively, the duplicate, triplicate, and quadruplicate ratio of A to B; where p is successively equal to 2, 3, and 4. And universally, by the same geometrical reasoning, it is found, that $A + \frac{p-1}{1} \cdot A \cdot \frac{A-B}{B} + \frac{p-1}{1} \cdot \frac{p-2}{2} \cdot A \cdot \frac{(A-B)^2}{B^2} + \&c. A \cdot \frac{(A-B)^{p-1}}{B^{p-1}}$,

has to B such a multiply ratio of A to B, as is expressed by the number p .

In the reasoning above, I fixed on B as the magnitude to which the rest were to be referred; but I might as well have fixed on A, or any of the other magnitudes. Thus, for instance $B + \frac{p-1}{1} \cdot B \cdot \frac{B-A}{A} + \frac{p-1}{1} \cdot \frac{p-2}{2} \cdot B \cdot \frac{(B-A)^2}{A^2} + \&c. B \cdot \frac{(B-A)^{p-1}}{A^{p-1}}$ has to A, such a multiply ratio of B to A, as is expressed by the number p ; or A has to $B + \frac{p-1}{1} \cdot B \cdot \frac{B-A}{A} + \frac{p-1}{1} \cdot \frac{p-2}{2} \cdot B \cdot \frac{(B-A)^2}{A^2} + \&c. B \cdot \frac{(B-A)^{p-1}}{A^{p-1}}$, the ratio of $A + \frac{p-1}{1} \cdot A \cdot \frac{A-B}{B} + \frac{p-1}{1} \cdot \frac{p-2}{2} \cdot A \cdot \frac{(A-B)^2}{B^2} + \&c. A \cdot \frac{(A-B)^{p-1}}{B^{p-1}}$ to B; that is, such a multiply ratio of A to B, as is expressed by the number p . Each of these indeed is demonstrated separately from the same sort of geometrical reasoning; but for the sake of brevity these separate demonstrations are omitted, as they are both contained in the general reasoning above, which furnishes likewise a great variety of other expressions, according as certain numbers of the ratios C to D, E to F, G to H, &c. are supposed to be respectively equal to, greater or less than, the ratio of A to B.

XXIV. The Case of Ann Davenport. By Mr. Fielding Best Fynney, Surgeon, at Leek, Staffordshire. p. 458.

May 16, 1775, Mr. F. being desired to visit Ann Davenport, a native of Leek, he beheld a truly miserable object, with the most cadaverous countenance he had ever seen, emaciated to the last degree by a hectic fever, and profuse colliquative sweats. She had a continual thirst, her appetite was totally gone, and she was always in the extremes of being too loose or too bound. Her mother said, she was then in her 21st year; and that she had been a strong and sprightly child from her birth, till she was about 5 years of age, from which time she had been a stranger to health, and every now and then had been seized with excruciating fits of the colic, especially whenever she ate or drank any thing the least acid. The young woman said, that about a year before she had first perceived a swelling on the right side of her belly just above the groin; which, if at any time she attempted to stretch out her thigh, gave her inexpressible pain, as if something stabbed her in that part: that therefore she was always obliged to keep up her knees, more or less, towards her breast, by which means she had, in some degree, lost the power of extending her limbs.

Mr. F. ordered her to take $\frac{1}{2}$ dr. of powdered bark in a little port wine every 4 hours; and, as matter had already formed within the tumor, he desired that a maturing poultice might be applied every night and morning; for he imagined that nature, without such assistance, could never bring the abscess to a head in her weak condition. July 10th, the matter pointing at the upper end of the tumor very near the os ilium, he made a large opening, from which was dis-

charged an amazing quantity of pus; but, as the tension was still great, he applied a linseed poultice over the common dressings: yet, in a few days a 2d abscess began to form towards the vertebræ of the loins, between the false ribs and the os ilium, which was rapid in its progress, for it was brought to maturation, and opened on the 26th. On the 31st he was alarmed with a gangrenous appearance of the whole integuments of the abdomen: for this she took 1 dr. of powdered bark in red port every 3 hours; but, as vesications and every symptom of a sphacelus continued to increase, he also used the bark externally, in embrocations and cataplasms morning and evening.

This treatment soon stopped the mortification, and the parts sloughing off largely, left 3 holes at nearly equal distances from one another, between the first opening and the left os ilium, besides several others in different parts of the belly; but as the discharge was immoderate, he thought the patient in the utmost danger. However, the same course was persevered in, and at the latter end of August another abscess appeared lower down, towards the right groin; he ordered it to be poulticed, and left it to open of itself, which it did on the 21st of September. He was immediately called to her; and, on carefully examining the part, he found a hard substance deeply seated, which he directly extracted.* It was making its way towards the integuments from the extremity of the appendix vermiformis of the cæcum, which probably, and fortunately, by former inflammations had adhered to the peritonæum. The large end came first, and the small end was within the appendix vermiformis of the cæcum at the time he took it out; for, immediately on the extraction, some excrements followed, and among them some dark brown particles which he discovered to be filings of iron, which the patient had formerly taken in a large quantity, as she had never been regular like other women. On a careful examination he found some of these filings quite reduced to rust, but still retaining their form as they came from under the file. Some fæces came through this last wound daily, frequently most copiously; and sometimes, though the external orifice was larger, by being confined with the dressings, they insinuated

* In pl. 2, fig. 4, 5, are different views of the external surface of this irregular substance, and of so much of its nucleus as projects out of the round part, exactly as both appeared on being taken out of the body. The whole was of a dusky brown colour, and had a great resemblance to a small shrivelled pear. Fig. 6, is a section of the round part, which seemed to be formed of fine fibrous substances, closely cemented together by an earthy matter, and of the peg of crab-tree wood, its nucleus. This figure also shows how far the peg went in, and also an incrustation of stony matter on it. The nucleus, Mr. F. believed to be the smaller end of that part of a silk engine called a star, at which machine the patient had been employed before she was 5 years of age; therefore it must have been lodged at least 16 years within the appendix vermiformis of the cæcum, as she remembered nothing of swallowing it, and as during that course of years she had frequently been afflicted with the severe colics beforementioned.—Orig.

themselves between the integuments of the abdomen, and came through the other openings. About the middle of February 1776, the discharge of the excrements by these openings was sensibly diminished; and the wounds were all healed, except one, by the latter end of the year, through which a small quantity of excrements still continued to pass now and then. Her health had surprisingly improved; she was at the above date very fleshy and strong, had the catamenia, and Mr. F. had the greatest reason to expect that she would be perfectly cured. Strict regard was all along paid to the non-naturals.

Subjoined to this account is the attestation of the clergyman of the parish.

XXV. An Account of the Kingdom of Thibet. By John Stewart, Esq., F. R. S.
p. 465.

The kingdom of Thibet, though known by name ever since the days of Marco Paolo and other travellers of the 12th and 13th centuries, had never been properly explored by any European till the present period. It is true, some straggling missionaries of the begging orders had, at different times, penetrated into different parts of the country; but their observations, directed by ignorance and superstition, placed in a narrow sphere, could give no ideas but what were false and imperfect. Since them, the Jesuits have given the world, in Duhalde's History of China, a short account of this country, collected with their usual pains and judgment, from Tartar relations, which, as far as it goes, seems to be pretty just.

This country commonly passes in Bengal under the name of Boutan. It lies to the northward of Hindostan, and is all along separated from it by a range of high and steep mountains, properly a continuation of the great Caucasus, which stretches from the ancient Media and the shores of the Caspian sea, round the north-east frontiers of Persia, to Candahar and Cassamire, and thence, continuing its course more easterly, forms the great northern barrier to the various provinces of the Mogol empire, and ends, as we have reason to believe, in Assam or China. This stupendous Tartar bulwark had ever been held impassable by the Mogols, and all other Mussulams conquerors of India; and though in the vallies lying between the lower mountains, which run out perpendicular to the main ridge, where reside various Indian people, whom they had occasionally made tributary to their power, they never had attempted a solid or permanent dominion over them. It was on occasion of a disputed succession between the heirs of one of the Rajah's or petty sovereigns of those people, that the Boutaners were called down from their mountains to the assistance of one of the parties; and our government engaged on the opposite side. The party assisted by us did not fail in the end to prevail; and in the course of this little war two people became acquainted who, though near neigh-

hours, were equally strangers to each other. At the attack of a town called Gooch Behar, our troops and the Boutaners first met; and nothing could exceed their mutual surprize in the rencounter. The Boutaners, who had never met in the plains any other than the timid Hindoos flying naked before them, saw, for the first time, a body of men, uniformly clothed and accoutred, moving in regular order, and led on by men of complexion, dress, and features, such as they had never beheld before: and then the management of the artillery, and incessant fire of the musketry, was beyond any idea which they could have conceived of it. On the other hand, our people found themselves on a sudden engaged with a race of men unlike all their former opponents in India, uncouth in their appearance, and fierce in their assault, wrapped up in furs; and armed with bows and arrows and other weapons peculiar to them. The place was carried by our troops; and a great many things taken in the spoil; such as arms, clothing, and utensils of various sorts. Images in clay, in gold, in silver, and in enamel, were sent down to Calcutta; all which appeared perfectly Tartar, as we have them represented in the relations and drawings of travellers; and there were besides several pieces of Chinese paintings and manufactures. While those things continued to be the subject of much conversation and curiosity to us in Bengal, the fame of our exploits in the war had reached the court of Thibet, and awakened the attention of the Tayshoo Lama, who (the Delai Lama being a minor) was then at the head of the state. The Dah Terriah, or Deb Rajah as he is called in Bengal (who rules immediately over the Boutaners, and had engaged them in the war) being a feudatory of Thibet, the Lama thought it proper to interpose his good offices, and in consequence sent a person of rank to Bengal, with a letter and presents to the governor, to solicit a peace for the Dah, as his vassal and dependent.

Mr. Hastings, the governor, did not hesitate a moment to grant a peace at the mediation of the Lama, on the most moderate and equitable terms; and, eager to seize every opportunity which could promote the interest and glory of this nation, and tend to the advancement of natural knowledge, proposed in council to send a person in a public character to the court of the Tayshoo Lama, to negotiate a treaty of commerce between the two nations, and to explore a country and people hitherto so little known to Europeans. Mr. Bogle, an approved servant of the company, whose abilities and temper rendered him every way qualified for so hazardous and uncommon a mission, was pitched on for it. It would be foreign to my purpose to enter into a detail of his progress and success in this business: it will be sufficient to say, that he penetrated, across many difficulties, to the centre of Thibet; resided several months at the court of the Tayshoo Lama; and returned to Calcutta, after an absence of 15

months on the whole, having executed his commission to the entire satisfaction of the administration.

Mr. Bogle divides the territories of the Delai Lama into 2 different parts. That which lies immediately contiguous to Bengal, and which is called by the inhabitants Docpo, he distinguishes by the name of Boutan; and the other, which extends to the northward as far as the frontiers of Tartary, called by the natives Pû, he styles Thibet. Boutan is ruled by the Dah Terriah or Deb Rajah, as already remarked. It is a country of steep and inaccessible mountains, whose summits are crowned with eternal snow; they are intersected with deep vallies, through which pour numberless torrents that increase in their course, and at last, gaining the plains, lose themselves in the great rivers of Bengal. These mountains are covered down their sides with forests of stately trees of various sorts; some, such as pines, &c. which are known in Europe; others that are peculiar to the country and climate. The valley and sides of the hills which admit of cultivation are not unfruitful, but produce crops of wheat, barley, and rice. The inhabitants are a stout and warlike people, of a copper complexion, in size rather above the middle European stature, hasty, and quarrelsome in their temper, and addicted to the use of spirituous liquors; but honest in their dealings, robbery by violence being almost unknown among them. The chief city is Tassei Seddein situated on the Patchoo. Thibet begins properly from the top of the great ridge of the Caucasus, and thence extends in breadth to the confines of Great Tartary, and perhaps to some of the dominions of the Russian empire. Having once attained the summit of the Boutan mountains, you do not descend in an equal proportion on the side of Thibet; but, continuing still on a very elevated base, you traverse valleys which are wider and not so deep as the former, and mountains that are neither so steep, nor apparently so high. On the other hand, Mr. Bogle represents it as the most bare and desolate country he ever saw. The woods, which every where cover the mountains in Boutan, are here totally unknown; and, except a few straggling trees near the villages, nothing of the sort is to be seen. The climate is extremely severe and rude. At Chamnànning, where he wintered, though it be in latitude $31^{\circ} 39'$, only 8° to the northward of Calcutta, he often found the thermometer in his room at 29° under the freezing point by Fahrenheit's scale; and in the middle of April the standing waters were all frozen, and heavy showers of snow perpetually fell. This must doubtless be owing to the great elevation of the country, and to the vast frozen space over which the north wind blows uninterruptedly from the pole, through the vast desarts of Siberia and Tartary, till it is stopped by this formidable wall.

The Thibetians are of a smaller size than their southern neighbours, and of

a less robust make. Their complexions are also fairer, and many of them have even a ruddiness in their countenances unknown in the other climates of the east. Those whom I saw at Calcutta appeared to have quite the Tartar face. They are of a mild and cheerful temper; and Mr. Bogle says, that the higher ranks are polite and entertaining in conversation, in which they never mix either strained compliments or flattery. The common people, both in Boutan and Thibet, are clothed in coarse woollen stuffs of their own manufacture, lined with such skins as they can procure; but the better orders of men are dressed in European cloth, or China silk, lined with the finest Siberian furs. The ambassador from the Deb Rajah, in his summer-dress at Calcutta, appeared exactly like the figures we see in the Chinese paintings, with the conical hat, the tunic of brocaded silk, and light boots. The Thebetian who brought the first letter from the Lama was wrapped up from head to foot in furs. The use of linen is totally unknown among them. The chief food of the inhabitants is the milk of their cattle, prepared into cheese and butter, or mixed with the flour of a coarse barley or of peas, the only grain which their soil produces; and even these articles are in a scanty proportion; but they are furnished with rice and wheat from Bengal and other countries in their neighbourhood. They are supplied with fish from the rivers in their own and the neighbouring provinces, salted and sent into the anterior parts. They have plenty of animal food, from the cattle, sheep, and hogs, which are raised on their hills; and are not destitute of game. They have a singular method of preparing their mutton, by exposing the carcase entire, after the bowels are taken out, to the sun and bleak northern winds, which blow in the months of August and September, without frost, and so dry up the juices and parch the skin, that the meat will keep uncorrupted for the year round. This they generally eat raw, without any other preparation. Mr. Bogle was often regaled with this dish, which, however unpalatable at first, he says, he afterwards preferred to their dressed mutton just killed, which was generally lean, tough, and rank. It was also very common for the head men, in the villages through which he passed, to make him presents of sheep so prepared, set before him on their legs as if they had been alive, which at first had a very odd appearance.

The religion and political constitution of this country, which are intimately blended together, would make a considerable chapter in its history. It suffices here to say, that at present, and ever since the expulsion of the Eluth Tartars, the kingdom of Thibet is regarded as depending on the empire of China, which they call Cathay; and there actually reside 2 mandarines, with a garrison of 1000 Chinese, at Lahassa the capital, to support the government; but their power does not extend far: and in fact, the Lama, whose empire is founded on the surest grounds, personal affection and religious reverence, governs every

thing internally with unbounded authority. It is well known that the Delai Lama is the great object of adoration for the various tribes of heathen Tartars, who roam through the vast tract of continent which stretches from the banks of the Volga to Corea on the sea of Japan, the most extensive religious dominion perhaps on the face of the globe. He is not only the sovereign Pontiff, the vicegerent of the Deity on earth: but, as superstition is ever the strongest where it is most removed from its object, the more remote Tartars absolutely regard him as the Deity himself. They believe him immortal, and endowed with all knowledge and virtue. Every year they come up from different parts, to worship and make rich offerings at his shrine; even the emperor of China, who is a Mantchou Tartar, does not fail in acknowledgements to him in his religious capacity, and actually entertains at a great expence, in the palace of Pekin, an inferior Lama, deputed as his Nuncio from Thibet. Mr. Bogle says, that the Lama often distributes little balls of consecrated flour, like the pain benit of the Roman catholics, which the superstition and blind credulity of his Tartar votaries may afterwards convert into what they please. The orthodox opinion is, that when the Grand Lama seems to die, either of old age or of infirmity, his soul in fact only quits an actual crazy habitation, to look for another or better, and it is discovered again in the body of some child, by certain tokens known only to the Lamas or Priests, in which order he always appears. The present Delai Lama is an infant, and was discovered only a few years ago by the Tayshoo Lama, who in authority and sanctity of character is next to him, and consequently, during the other's minority, acts as chief. The lamas, who form the most numerous as well as the most powerful body in the state, have the priesthood entirely in their hands; and besides fill up many monastic orders, which are held in great veneration among them. Celibacy is not positively enjoined to the lamas; but it is held indispensable for both men and women, who embrace a religious life: and indeed their celibacy, their living communities, their cloysters, their service in the choirs, their strings of beads, their fasts, and their penances, give them so much the air of christian monks, that it is not surprizing an illiterate capuchin should be ready to hail them brothers, and think he can trace the features of St. Francis in every thing about them. It is an old notion that the religion of Thibet is a corrupted christianity. The truth is, that the religion of Thibet, whence-ever it sprung, is pure and simple in its source, conveying very exalted notions of the Deity, with no contemptible system of morality; but in its progress it has been greatly altered and corrupted by the inventions of men. Polygamy, at least in the sense we commonly receive the word, is not in practice among them; but it exists in a manner still more repugnant to European ideas; viz. in the plurality of husbands, which is firmly established and highly respected there. In a country where the means of sub-

sisting a family are not easily found, it seems not impolitic to allow a set of brothers to agree in raising one, which is to be maintained by their joint efforts. In short, it is usual in Thibet for the brothers in the family to have a wife in common, and they generally live in great harmony and comfort with her; not but sometimes little dissensions will arise, an instance of which Mr. Bogle mentions in the case of a modest and virtuous lady, the wife of half a dozen of the Tayshoo Lama's nephews, who complained to the uncle, that the two youngest of her husbands did not furnish that share of love and benevolence to the common stock which duty and religion required of them. In short, however strange this custom may appear to us, it is an undoubted fact that it prevails in Thibet, in the manner here described.

The manner of bestowing their dead is also singular: they neither put them in the ground like other Europeans, nor burn them like the Hindoos; but expose them on the bleak pinnacle of some neighbouring mountain, to be devoured by wild beasts and birds of prey, or wasted away by time and the vicissitudes of weather in which they lie. The religion of Thibet, although it be in many of its principal dogmata totally repugnant to that of the Bramins or of India, yet in others it has a great affinity to it. They have, for instance, a great veneration for the cow; but they transfer it wholly from the common species to that which bears the tails, spoken of hereafter. They also highly respect the waters of the Ganges, the source of which they believe to be in Heaven; and one of the first effects which the treaty with the Lama produced, was an application to the governor-general, for leave to build a place of worship on its banks. This it may be imagined was not refused. On the other hand, the Sunniasses, or Indian pilgrims, often visit Thibet as a holy place, and the Lama always entertains a body of 2 or 300 in his pay.

The residence of the Delai Lama is at Pateli, a vast palace on a mountain near the banks of the Barampooter, about 7 miles from Lahassa. The Tayshoo Lama has several palaces or castles, in one of which Mr. Bogle lived with him five months. He represents the Lama as one of the most amiable as well as intelligent men he ever knew; maintaining his rank with the utmost mildness of authority, and living in the greatest purity of manners, without affectation. Every thing within the gates breathed peace, order, and dignified elegance. The castle is of stone or brick, with many courts, lofty halls, terraces, and porticos; and the apartments are in general roomy, and highly finished in the Chinese stile, with gilding, painting, and varnish. There are two conveniencies to which they are utter strangers, stair-cases and windows. There is no access to the upper rooms but by a sort of ladders of wood or iron; and for windows they have only holes in the ceilings, with penthouse covers, contrived so as to shut up on the weather-side. Firing is so scarce, that little is used but for culi-

nary purposes; and they trust altogether for warmth in their houses to their furs and other clothing. The Lama, who is completely conversant in what regards Tartary, China, and all the kingdoms in the east, was exceedingly inquisitive about Europe, its politics, laws, arts, and sciences, government, commerce and military strength; on all which heads Mr. Bogle endeavoured to satisfy him, and actually compiled for his service a brief state of Europe in the Hindostan language, which he ordered to be translated into that of Thibet. The Russian empire was the only one in Europe known to him: he has a high idea of its riches and strength, and had heard of its wars and success against the empire of Rome, for so they call the Turkish state, but could not conceive it could be in anywise a match for Cathay. Many of the Tartar subjects of Russia come to Thibet; and the Czar has even, at various times, sent letters and presents to the Lama. Mr. Bogle saw many European articles in his hands; pictures, looking-glasses, and trinkets of gold, silver, and steel, chiefly English, which he had received that way, particularly a Graham's repeating watch, which had been dead, as they said, for some time. While he was there, several Mongols and Calmucs arrived from Siberia, with whom he conversed.

The city of Lahassa, which is the capital, is of no inconsiderable size, and is represented as populous and flourishing. It is the residence of the chief officers, of government, and of the Chinese mandarins and their suite. It is also inhabited by Chinese and Cassemirian merchants and artificers, and is the daily resort of numberless traders from all quarters, who come in occasional parties, or in stated caravans. The waters of the Great River, as it is emphatically called in their language, wash its walls. Father Duhalde, with great accuracy, traces this river, which he never suspects to be the Barampooter, from its origin in the Cassemirian mountains (probably from the same spring which gives rise to the Ganges) through the great valley of Thibet, till, turning suddenly to the southward, he loses it in the kingdom of Assam; but still, with great judgment and probability of conjecture, supposes it reaches the Indian sea somewhere in Pegu or Aracan. The truth is however, that it turns suddenly again in the middle of Assam, and traversing that country westerly, enters Bengal towards Rangamatty, under the abovementioned name, and thence bending its course more southerly, joins the Ganges, its sister and rival, with an equal, if not more copious stream; forming at the conflux a body of running fresh water, hardly to be paralleled in the known world, which disembogues itself into the Bay of Bengal. Two such rivers uniting in this unhappy country, with all the beauty, fertility, and convenience which they bring, well entitles it to the name of the Paradise of Nations, always bestowed on it by the Moguls.

The chief trade from Lahassa to Peking is carried on by caravans, that employ full 2 years in the journey thither and back again; which is not surprising, when

we consider that the distance cannot be less than 2000 English miles: and yet an express from Lahassa reaches Pekin in 3 weeks, a circumstance much to the honour of the Chinese police, which knows to establish so speedy and effectual a communication through mountains and deserts for so long a way. The trade with Siberia is carried on by caravans to Seling, which is undoubtedly the Selinginsky of the Russian travellers on the borders of Baykal Lake. The Indians have an admirable method of turning godliness into great gain, it being usual for the Faquiers to carry with them, in their pilgrimages from the sea coasts to the interior parts, pearls, corals, spices, and other precious articles, of small bulk, which they exchange on their return for gold-dust, musk, and other things of a similar nature, concealing them easily in their hair and in the clothes round their middle, and carrying on, considering their numbers, no inconsiderable traffic by these means. The Gosseigns are also of a religious order, but in dignity above the Faquiers; and they drive a more extensive and a more open trade with that country.

Besides their less traffic with their neighbours in horses, hogs, rock-salt, coarse cloths, and other articles, they enjoy 4 staple articles, which are sufficient in themselves to procure every foreign commodity of which they stand in need; all of which are natural productions, and deserve to be particularly noticed. The first, though the least considerable, is that of the cow-tails, so famous all over India, Persia, and the other kingdoms of the east. It is produced by a species of cow or bullock different from what is found in any other country. It is of a larger size than the common Thibet breed, has short horns, and no hump on its back. Its skin is covered with whitish hair of a silky appearance; but its chief singularity is in its tail, which spreads out broad and long, with flowing hairs, like that of a beautiful mare, but much finer and far more glossy. Mr. Bogle sent down 2 of this breed to Mr. Hastings, but they died before they reached Calcutta. The tails sell very high, and are used, mounted on silver handles, for chowras, or brushes, to chase away the flies; and no man of consequence in India ever goes out, or sits in form at home, without two chowrawbadars, or brushers, attending him, with such instruments in their hands.

The next article is the wool from which is made the shaul, the most delicate woollen manufacture in the world, so much prized in the east, and now so well known in England. Till Mr. Bogle's journey, our notions on that subject were very crude and imperfect. As the shauls all come from Cassemire, we concluded the material from which they were fabricated to be also of that country's growth. It was said to be the hair of a particular goat, the fine under hair from a camel's breast, and a thousand other fancies; but we know it for certain to be the produce of Thibet sheep. Mr. Hastings had one or two of these in his paddock when I left Bengal. They are of a small breed, in figure nothing differing from our sheep, except in their tails, which are very broad; but their

fleeces, for the fineness, length, and beauty of the wool, exceed all others in the world. The Cassemirians engross this article, and have factors established for its purchase in every part of Thibet, whence it is sent to Cassemire, where it is worked up, and becomes of great wealth to that country, as well as it is originally to Thibet.

Musk is another of their staple articles, of which it will be needless to say much, as the nature, quality, and value, of this precious commodity are so well known in Europe. The deer which produces it is common in the mountains; but being excessively shy, and frequenting solely the places the most wild and difficult of access, it becomes a trade of great trouble and danger to hunt after. We have the musk sent down to Calcutta in the natural bag, not without great risk of its being adulterated; but still it is far superior to any thing of the kind that is to be met with in sale in Europe.

The last of the articles which I reckon staple, is gold, of which great quantities are exported from Thibet. It is found in the sands of the Great River, as well as in most of the small brooks and torrents that pour from the mountains. The quantity gathered in this manner, though considerable with respect to national gain, pays the individual but very moderately for the labour bestowed on it. But, besides this, there are mines of that metal in the northern parts, which are the reserved property of the Lama, and rented out to those who work them. It is not found in ore, but always in a pure metallic state (as I believe it to be the case in all other mines of this metal) and only requires to be separated from the spar, stone, or flint, to which it adheres. Mr. Hastings had a lump sent to him at Calcutta, of about the size of a bullock's kidney, which was a hard flint veined with solid gold. He caused it to be sawed in two, and it was found throughout interlarded, as it were with the purest metal. Though they have this gold in great plenty at Thibet, they do not employ it in coin, of which their government never strikes any; but it is still used as a medium of commerce, and goods are rated there by the purse of gold, as here by money. The Chinese draw it from them to a great amount every year, in return for the produce of their labour and arts. I hope the society will accept as a rarity the translation of the original letter which the Tayshoo Lama wrote to Mr. Hastings, by the envoy whom he sent to solicit a peace for the Deb Rajah. It came into my hands in the course of my office, and by the permission of the governor-general I retained a copy.

Translation of a Letter from the Tayshoo Lama to Mr. Hastings, Governor of Bengal, received the 29th of March, 1774.

“The affairs of this quarter in every respect flourish: I am night and day employed for the increase of your happiness and prosperity. Having been informed, by travellers from our quarter, of your exalted fame and reputation,

my heart, like the blossom of spring, abounds with satisfaction, gladness, and joy. Praise God that the star of your fortune is in its ascension. Praise him, that happiness and ease are the surrounding attendants of myself and family. Neither to molest or persecute is my aim: it is even the characteristic of our sect to deprive ourselves of the necessary refreshment of sleep, should an injury be done to a single individual; but in justice and humanity, I am informed you far surpass us. May you ever adorn the seat of justice and power, that mankind may, in the shadow of your bosom, enjoy the blessings of peace and affluence! By your favour I am the Rajah and Lama of this country, and rule over a number of subjects; a particular with which you have no doubt been acquainted by travellers from these parts. I have been repeatedly informed, that you have been engaged in hostilities against the Dah Terria, to which it is said the Dah's own criminal conduct, in committing ravages and other outrages on your frontiers, gave rise. As he is of a rude and ignorant race, past times are not destitute of the like misconduct which his avarice tempted him to commit. It is not unlikely but he has now renewed those instances, and the ravages and plunder which he may have committed on the skirts of the Bengal and Bahar provinces, have given you provocation to send your vindictive army against him. However, his party has been defeated, many of his people have been killed, three forts have been taken from him, and he has met with the punishment he deserved. It is as evident as the sun that your army has been victorious; and that, if you had been desirous of it, you might in the space of two days have entirely extirpated him, for he had not power to resist your efforts. But I now take upon me to be his mediator; and to represent to you, that, as the said Dah Terria is dependent on the Dela Lama, who rules in this country with unlimited sway (but, on account of his being in his minority, the charge of the government and administration for the present is committed to me) should you persist in offering further molestation to the Dah's country, it will irritate both the Lama and all his subjects against you. Therefore, from a regard to our religion and customs, I request you will cease all hostilities against him; and in doing this you will confer the greatest favour and friendship on me. I have reprimanded the Dah for his past conduct; and I have admonished him to desist from his evil practices in future, and to be submissive to you in all things. I am persuaded he will conform to the advice which I have given him; and it will be necessary that you treat him with compassion and clemency. As to my part, I am but a Faquier; and it is the custom of my sect, with the rosary in our hands, to pray for the welfare of mankind, and for the peace and happiness of the inhabitants of this country; and I do now, with my head uncovered, intreat that you may cease all hostilities against the Dah in future. It would be needless to add to the length of this letter, as the bearer of it, who is a Goseign, will

represent to you all particulars; and it is hoped you will comply therewith. In this country, worship of the Almighty is the profession of all. We poor creatures are in nothing equal to you; having however a few things in hand, I send them to you by way of remembrance, and hope for your acceptance of them."

XXVI. Of the Degrees and Quantities of Winds requisite to Move the Heavier kinds of Wind Machines. By John Stedman, M. D. p. 493.

The irregularity and uncertainty of winds in this country have been found a considerable discouragement to erect wind machines. These machines are seldom erected with us, unless where a considerable moving power is necessary. This is always the case where the larger kind of pump-work is to be kept in motion, or where water is to be extracted from deep pits. Having inquired of many people concerned in such works, what may be the proportion of time in which wind machines may be kept in motion, to that in which they cannot move from a defect of wind, it is found that these people differ widely in their conjectures. The only method of bringing the matter to a proper estimate is, by comparing the quantities of winds sufficient for moving these machines, with those of winds below that degree, and calms. This computation can only be drawn from journals in which the degrees of winds are noted. In the meteorological register of the Medical Essays of the Edinburgh Society, there is a column of winds, and 4 degrees are noted. This division is sufficient for the purposes for which that register was intended; but when we consider the wind as a power acting on machines, that number of degrees will be found too small. Thus, from the 2d in that register to a hurricane, there is but 1 intermediate degree. As the 2d degree, which is very moderate, is insufficient for moving these machines; the 3d is more than just enough for that purpose. A degree therefore which is a mean between these 2, will be found to be the lowest that is sufficient for moving machines of the heavier kinds, particularly such as are used for pumping water out of coal-pits. These 3 degrees of wind, that is the 2d and 3d of the Edinburgh register, and an intermediate degree, are very distinguishable even by the senses, and without the assistance of any instrument, by those who are attentive, and have been accustomed to make observations of this nature.

To ascertain proportions of this nature, a longer term of years would have been more satisfactory; but in case others should afterwards pursue this kind of computation, the proportions are digested in a table at the end of this essay, and may be consulted occasionally. In making up this table, viz. of the 2d degree and above, and of the 3d and upwards, hurricanes are included, though that degree of wind be too high for any machine. But as the observations were taken twice in 24 hours, and as winds sufficient to move these machines may be sup-

posed to have happened sometimes between the times of observation, though at these times the wind might have been below the mean; to compensate this defect, hurricanes are included in the computation. From the table then we have the following proportions of the 2 degrees of winds and upwards, to those below; and also of the mean between those 2 degrees.

	Days.
Winds of the 2d degree and upwards in each week	4.283
Calms and winds below the 2d degree	2.717
Winds of the 3d degree and upwards	0.902
Calms and winds below the 3d degree	6.098
Winds of a mean proportion between the 2 preceding degrees ..	2.592
Calms and winds below the mean degree	4.408

From this computation we have 2.592 days in a week, or 19.307 weeks in a year, in which wind machines of the heavier kinds, and of considerable friction, may be supposed to be kept in motion; which, to the times in which they cannot go, is as 10 to 17. It may be observed, that the resistance to the machine, or its weight and friction, being diminished, though in a small degree, will add considerably to the frequency and length of times in which it can go; since it often happens that there are winds immediately below the lowest degree in the preceding estimate, sufficient to keep the lighter machines in motion. Hence those who have machines which are not absolutely of the heaviest kind, will be apt to conclude this computation erroneous. Besides, there are few who make allowance for, or attend to the universal law which obtains in mechanics, that in larger machines, their power doth not increase in a proportion so high as their bulk and the resistance arising from their friction. The table, for each month, on an average of 5 years, is annexed.

A Table, showing the Proportion of Winds of the 2d degree and upwards, to those of the 1st and below; also of the 3d degree and upwards, to those of the 2d and below; for each month on a medium of 5 years.

	Proportion of winds of the 2d degree to those of the 1st, &c.		Proportion of winds of the 3d degree to those below it for 5 years.	
	Wind.	Calms, &c.	Winds.	Calms, &c.
June	153	147	7	233
July	162	148	25	285
August	130	180	48	252
September ..	162	138	11	299
October	141	169	35	275
November ..	157	141	46	254
December ..	197	113	46	264
January	236	74	49	261
February ...	228	56	87	197
March	228	82	51	259
April	212	88	36	264
May	230	80	30	280
Sum of yrs. & months }	2236	1416	471	3123

XXVII. Description of the Jesuits Bark Tree of Jamaica and the Caribbees.

By Wm. Wright, M.D., Member of the Philos. Society of America, and Surgeon-General in Jamaica. p. 504.

This species of Jesuits bark grows on stony lands near the sea-shore, in the

parishes of St. James and Hanover, on the north side of Jamaica. The tree is called the sea-side beech, and rises only to 20 feet. The trunk is not thick in proportion, but hard, tough, and of a yellowish-white colour in the inside. The branches and leaves are opposite; the leaves are of a rusty green, and the young buds of a blueish green hue. It blossoms in November, and continues in flower till February, having on the same tree or sprig, flowers and ripe pods. The flowers are of a dusky yellow colour, and the pods black: when ripe they split in two, and are, with their flat brown seeds, in every respect similar to those of the *Cinchona officinalis* as depicted in a plate sent out by Mr. Banks.

The bark of this tree in general is smooth and grey on the outside, though in some rough and scabrous. When well dried, the inside is of a dark brown colour. Its flavour at first is sweet, with a mixture of the taste of horse-radish and of aromatics of the east; but, when swallowed, of that very bitterness and astringency which characterizes the Peruvian bark. It yields these qualities strongly to water both when cold and in decoction. Half an ounce, boiled from 2 lb. to 1 lb. of water, made as strong a decoction as 3 times its weight of the *cinchona vera*. The colour was brown, but not turbid.

I have had many opportunities (says Dr. W.) of trying its effects, especially in remittents, which are the most common and fatal fevers in these climes. A vomit or gentle purge, if necessary, was first given; and then immediately this bark as soon as they operated. It strengthened the stomach, checked retching and vomiting, corrected morbid humours in *primæ viæ*, and conquered speedily the disease. My success in such a dangerous malady leaves not a doubt but that it will prove equally efficacious in every other case where a tonic and antiseptic medicine is indicated.

LEAVES ovate, perfectly entire, nerveless, opposite.

FLOWERS single, axillary.

CALYX. *Perianth* one-leaved, superior, five-cleft, very small, permanent, bell-shaped, very obscurely five-toothed.

COROL. one-petalled, funnel-form: *Tube* cylindric, very long: *Border* five-parted, equal to the tube: *Divisions* ovate, oblong, reflex, sometimes pendulous.

STAM. *Filaments* five, filiform, erect from the middle of the tube, of the length of the corol. *Anthens* very long, obtuse, erect above the exterior base, affixed in the mouth of the corol.

CAPSULE. Bipartite, with a parallel dissepiment, gaping on the under side.

SEEDS. Very many, compressed, margined, oblong.

XXVIII. *Description and Use of the Cabbage-bark Tree of Jamaica.* By Wm. Wright, M.D. p. 507.

The cabbage-bark tree, or worm-bark tree, grows in most parts of Jamaica, and particularly abounds in the low Savannahs of St. Mary and St. George. It rises to a considerable height, but no great thickness, sending off branches near the top of a straight, smooth trunk. The leaves are, when young, of a light

green hue; when full-grown, of a dark green colour; and before they drop, of a rusty appearance. The flower-spike is long and beautifully branched. The flowers are numerous; their calyces of a dark purple; their petals of the colour of the pale rose; the nectaria must contain much honey, as thousands of bees, beetles of various kinds, butterflies, and humming-birds, are continually feeding on them. The pericarpium is a green, hard fruit, of the size of the smaller plum. The skin is of the thickness of a crown piece; and tastes very austere. The kernel is covered with a brown skin like that of other nuts; it is very hard, and tastes astringent.

The wood is hard, and takes a good polish. It is however fit only for rafters or other parts of small buildings; but this tree is valued chiefly for its bark, which externally is of a grey colour, and the inside black and furrowed. Fresh cabbage-bark tastes mucilaginous, sweet, and insipid. Its smell however is rather disagreeable, and it retains it in the decoction; hence by some called the bulge-water tree. Mr. Peter Duguid, formerly of this island, seems to have been the first that gave any account of the virtues of this bark, in the Edinburgh Essays, Physical and Literary, vol. 2. The experiments he promised have never yet appeared. It is certain it has powerful effects, and its anthelmintic quality is established by the experience of several ages. It is at present in general use here, and begins to be known in Europe. No description having yet appeared, I have supplied that defect as far as my abilities in botany reached. It remains now to proceed to its exhibition, and the purposes it is meant to answer as a medicine. Cabbage-bark may be given in different forms; as in decoction, syrup, powder, and extract. I have used them all, and shall speak of them separately.

The decoction. Take fresh dried or well-preserved cabbage-bark, 1 oz. Boil it in a quart of water, over a slow fire, till the water is of an amber colour, or rather of deep coloured Madeira wine; strain it off, sweeten it with sugar, and let it be used immediately, as it does not keep many days.—*Syrup of Cabbage-bark.* To any quantity of the above decoction add a double portion of sugar, and make a syrup. This will retain its virtues for years.—*The extract* of cabbage-bark is made by evaporating the strong decoction in balneo mariæ to the proper consistence; it must be continually stirred, as otherwise the resinous part rises to the top, and on this probably its efficacy depends.—*The powder* of well dried bark is easily made, and looks like jalap, though not of equal specific gravity.

This bark, like most other powerful anthelmintics, has a narcotic effect; and on this account it is always proper to begin with small doses, which may be gradually increased till a nausea is excited, when the dose for that patient is ascertained. But by frequent use we can in common determine the dose, though we

chuse to err rather on the safe side. A strong healthy grown person may, at first, take 4 table spoonfuls of the decoction or syrup, 3 grains of the extract, or 30 grains of the powder, for a dose. A youth, 3 table spoonfuls of the decoction or syrup, 2 grains of extract, or 20 grains of powder. A person of 10 years of age, 2 table spoonfuls of the decoction or syrup, $1\frac{1}{2}$ grain of extract, or 15 grains of the powder. Children of 2 or 3 years old, a table spoonful of the decoction or syrup, 1 grain of extract, or 10 grains of the powder. Children of a year old, half the quantity.

These may be increased, as above observed, till a nausea is excited, which will depend on the strength, sex, and habit of body of the patient. Care must be taken that cold water be not drank during the operation of this medicine, as it is in this case apt to occasion sickness, vomiting, fever, and delirium. When this happens, or when an over large dose has been given, the stomach must be washed with warm water: the patient must speedily be purged with castor-oil, and use plenty of lime-juice beverage for common drink; vegetable acid being a powerful antidote in this case, as well as in an over dose of opium. The decoction is what is mostly given here, and seldom fails to perform every thing that can be expected from an anthelmintic medicine, by destroying worms in the intestines, and bringing them away in great quantities. By frequent use however these animals become familiarized, and we find it necessary to intermit it, or have recourse to others of inferior merit.

The writers of the Edinburgh Medical Commentaries take notice, that the decoction of cabbage-bark always excites vomiting. We find no such effect from it here, and may account for it by their receiving it in a mouldy state. A syrup therefore is given there with better effect. They observe also that it has a diuretic virtue, which we have not noticed here. This bark purges pretty briskly, especially in powder, 30 or 40 grains working as well as jalap by stool; but in this way it does not seem to kill worms so well as in decoction. Five grains of the extract made a strong man sick, and purged him several times; but, by frequent use, he took 10 grains to produce at length the same effect.

It must not be concealed that fatal accidents have happened from the imprudent administration of this bark, chiefly from over-dosing the medicine. But this cannot detract from the merit of the cabbage-bark, since the best medicines when abused, become deleterious; and even our best aliments, in too great quantity, prove destructive. On the whole, the cabbage-bark is a most valuable remedy, and I hope will become an addition to the materia medica.

LEAVES opposite, oblong-ovate, ternate, acuminate, smooth above, beneath nerveless, with short footstalks.

CALYX. *Perianth* one-leaved, bell-shaped, very slightly five-parted, with short, ovate *Divisions*.

COROL. papilionaceous; *Standard* roundish, concave: *Wings* obtuse, concave, length of standard. *Keel* ovate, spreading, very slightly divided into two parts.

STAM. diadelphous, ten, filiform, inserted into calyx, length of wings. *Anthers* roundish.

PIST. subulate, filiform. *Stigma* none. *Germ* ovate-oblong, compressed.

PER. Drupe subovate large.

SEED. *Nut* subovate, subligneous, with a longitudinal furrow on each side, bivalve.

XXIX. *Observations made in Savoy, to Ascertain the Height of Mountains by means of the Barometer; being an Examination of Mr. De Luc's Rules, delivered in his Recherches sur les Modifications de l'Atmosphere. By Sir George Shuckburgh,* Bart. F.R.S. p. 513.*

In the course of his tour into Italy in the years 1775 and 1776, Sir G. made some stay at Geneva; which being in the neighbourhood of the Alps, and on that account a convenient home, induced him to make some observations on those mountains, which have been deservedly objects of attention to the most incurious traveller. He was particularly desirous of verifying the experiments with the barometer, in taking heights of different situations; a method that has been long known to the ingenious, though but rarely practised, and capable of but little precision till within these few years; and perhaps at present not so generally known as the convenience and utility of the method seems to require.

His first series of observations he proposed to be on Mont Saleve, one of the Alps, situated about 2 leagues south of Geneva, and precisely on the same point where Mr. De Luc had made his highest or 15th station. The place where he measured a base was in a field near the villages of Archamp and Neidens, not

* Sir George A. W. Shuckburgh Evelyn, Bart. died at his seat in Warwickshire, in Sept. 1804, in the 54th year of his age. He had represented that county in 3 successive parliaments; where his integrity and independent conduct as a British senator, procured him the approbation and respect of all wise and good men. Sir G. was an elegant classical scholar, and had improved his knowledge of men and manners by profitable travels through Europe. He was no mean mathematician and philosopher, and was very well skilled in astronomy, both theoretical and practical; in which sciences his deep and laborious researches gave him a distinguished character in the Royal and Antiquarian Societies, whose publications are adorned with several of his learned and ingenious compositions, of which, however, the present paper, on the Barometrical Measurement of Altitudes, is the chief. Sir Geo. carried his mathematical and logical habits into every purpose in life, in every circumstance of which he was one of the most correct and methodical of men; every part of his household and other concerns being conducted in the most punctual and orderly manner. Of men and motives of action Sir Geo. was a most accurate judge, and was always attentive to guard himself against the impositions of the designing. But his philosophical labours are those on which must chiefly rest the fabric of his intellectual fame. Here no man was more wary of making hasty inferences, or of forming general conclusions from partial or inaccurate observations. Truth was his darling object; which he endeavoured to discover, and to detect error, by the most patient and unremitted vigilance. Had Sir Geo. devoted more of his time to those pursuits, he would probably have had few superiors in philosophical celebrity. The pains he took to adjust a regular and uniform standard of weights and measures, the tardy cautiousness of his experiments, the accuracy of his calculations, and the practicability of his schemes, entitle him to the highest praise, among such as have laboured for the public benefit.

quite 3 miles in a horizontal line from the top of the rock whose height was to be determined. At the end A, of the base AB, he intended to place one of the barometers; and the other at the top of the rock, called the Pitton, at c the highest point of Mount Saleve; and with proper instruments to measure the triangle ABC. The angles were taken both on the horary circle, which was brought parallel to the horizon, and also on the azimuth circle of the equatorial instrument; this made it, as it were, 2 different instruments independant of each other. The angles were also doubled, tripled, and quadrupled, on each arch; by this means the error of the centre or axis of the instrument vanished; the possible error in the divisions, in the reading off, and in the coincidence of the wires in the telescope (which magnified 40 times) with the signals placed at each angle of the triangle, was lessened in proportion to the number of times the observation was repeated; and finally the mean of all was taken. The same was done with each angle at A, B, and c, horizontal as well as vertical, viz. the elevation of c above A and B was taken; and also the depression of A and B below c. The advantage of this method was, that the error of the line of collimation, the effect of refraction, and of the curvature of the earth's surface, all became equal and contrary; by these means the little errors were diminished, and great errors absolutely avoided.

From this measured base, and the angles taken at its extremities, by trigonometrical calculation Sir Geo. S. determined the sides of the triangle ABC, with the relative altitudes of its angular points, and consequently the height of the mountain at its highest point c; which were as below, viz. the side AB 2760.8 feet, Ac 15286.4, Bc 14041.7; and the height of c, the highest point, above A the lowest, 2831.3 feet, which is probably within 3 or 4 feet of the truth, or about 1 foot in 1000.

Having thus the perpendicular height very accurately ascertained by geometry, it remained to take the altitude of the barometer at each station A and c, and if possible with equal precision. These observations it would be too tedious to set down at length. The following particulars however may be noticed. Every observation of the barometer was triple; that is, the height was read off 3 different times, and the mean taken; but from once reading only Sir G. could be sure of the height to $\frac{2}{1000}$ of an inch, exclusive of the error of the divisions, which in some places might amount to that quantity; this the nonius would itself discover and even correct by estimation. At every series of observations the float at the bottom was readjusted, so that he could constantly be sure of an alteration of the weight of the atmosphere expressed by 0.002 inch of quicksilver, if not of half that quantity. Finally, the difference of the two barometers was constantly taken, after being left $\frac{3}{4}$ of an hour or more in the same place, to acquire the true temperature of the air, and this before and after every expedition. Hence

he concludes that the weight of any column of air may be measured with these barometers to .008 inch, though all the errors should lie the same way. The barometrical operations are related at considerable length; and the computations are made according to Mr. De Luc's method, or rather according to Dr. Horsley's reduction of it to the scales and measures of this country (vide vol. 13, p. 530,) with this difference, that Sir G. reckoned the equation for the expansion of quicksilver = .00323 inch for every degree of Fahrenheit's thermometer in a column of 30 inches, instead of .00312 which Mr. De Luc used; as Sir G. had collected from some of his own experiments made at Oxford in the beginning of the year 1773: this difference will not however occasion an alteration in the result of any one of the observations of more than 5 inches, and may therefore be considered as of no account. The barometrical computation was thus made from 3 different sets of observations, the 3 results from which were as below, for the height of the rock at c, above the point A in the plain below: viz. by the 1st set, it was 2775.2 feet, or 56.1 feet less than 2831.3 the height by the geometrical method; by the 2d set it came out 2763.2, or 68.1 feet too little; and by the 3d method 2759.3, or 72 feet too little.

These observations then seem to prove that the barometrical rules were a little defective as to the true ratio between the gravities of air and quicksilver, viz. in the value of an inch of quicksilver in the Torricellian tube, expressed in inches of the atmosphere with a given temperature. The first comparison gives for this error in defect — 19.8 feet in every 1000 feet; the second, 24.0 feet; and the last, 25.4 feet: the mean of the three is 23.1 feet; and by so much we may conclude that these rules, in greater heights also, will give the difference of elevation too little, viz. by $\frac{1}{43}$ nearly. But it will be fair to make the experiment.

The Mole is a convenient, insulated mountain, situated about 18 miles east of Geneva, and rising near 5000 feet above the lake, much higher than any person had ever made these experiments at, with the required precision. On this summit Sir G. determined to confirm or correct his discovery. After a tedious and difficult ascent up this mountain, its height was taken by the mean of the barometers and thermometers, and found to be 4120 feet; whereas the geometrical measurement, by means of a base measured at the foot of the mountain, after the manner of the former operation at Mount Saleve, gave full 4211.3 feet, for the geometrical height. Hence the mean error of the barometrical method, is 21.7 feet on every 1000. This result then justifies the former conclusion, and proves that either the proportional gravity of air and quicksilver is now different from what it was, when M. De Luc made his experiments, viz. from 1756 to 1760; or that his or Sir G.'s observations are defective. That my trigonometrical measurements were sufficiently exact, says Sir G. viz. to within 2 or 3 feet, I think I have already shown; and even that Mr.

D.'s were also. Within what limits my barometrical errors are to be found, is not difficult to determine from what has been before premised. That the scale of Mr. De Luc's barometer was less accurate than mine, is, I think, without a doubt; and indeed he never attempted a division less than $\frac{1}{16}$ of a French line, or about $\frac{5}{16000}$ of an inch English: and yet when I consider the number of his observations, and the unexampled diligence and care with which he made them, I am obliged to attribute the difference of our results to some other cause than that of inaccuracy. If then future experience should demonstrate, that the density of the atmosphere with a given heat is invariable, or nearly so; while the pressure of a whole column of it continues the same, we may perhaps search for the cause of our disagreement from hence, viz. the barometers of Mr. De Luc were not sufficiently near each other in an horizontal direction: mine were separated from 2 to 3 miles; and his, I believe, at double or triple that distance. It may be suspected, I am well aware, that the syphon construction of Mr. De Luc's barometer might occasion this difference: let us see whether this be the case. Mr. De Saussure (whose instrument was of Mr. De Luc's construction, and made as I understood, under his inspection) observed at the top of the Mole, or at least nearly on the same level with my barometer, as follows: after relating and calculating the observations by Mr. Saussure's observations, it turns out that the mean of 2 sets gives a difference of about 10 inches only in deducing the height of the mountain, a quantity wholly to be neglected. Finally, the mean of Mr. De Saussure's observations gives the defect of Mr. De Luc's rules 21.9 in 1000. The construction of the barometer had therefore no influence on this difference. But further, while Mr. De Saussure observed the height of the barometer on the Mole, Mr. De Luc, the brother, made a corresponding observation with a similar instrument at Geneva; the result of which, computed after Mr. De Luc's manner, gives, for the summit of the Mole above the lake of Geneva 4814 English feet; whereas, by a mean of Sir Geo. S.'s trigonometrical operations, this height was 4883 feet.

This last observation serves at least to show, that the error I am contending for is on the defective side, though it gives the quantity of it somewhat less, but by no means deserves that confidence which the other comparisons do; for, besides that this single observation may be concluded less decisive, the trigonometrical measurement is also less accurate from the distance; and lastly, to suppose the state of the atmosphere precisely the same with respect to weight in two places 20 miles asunder, is, I am afraid, a postulatum too hazardous to grant. I therefore say, that all these observations confirm the same truth, that the atmosphere is lighter than Mr. De Luc presumed it. What had already been done may seem sufficient for the establishment of this fact; for I have always held, that a few observations, well made and faithfully related, do more

in the interpretation of nature, than a multitude of crude, careless, and immethodical experiments. But I have not done: I wished to put this matter out of all doubt, and accordingly undertook another expedition to the summit of Mont Saleve, on the 18th of September, and in a colder temperature: the experiments then made, with their results, were as follow:

The difference of real height was 2828.9 feet. But, a mean of 4 series of barometrical observations, gives for the error on 1000 feet, 26.8. I think I have now shown, that the error actually exists; it remains that we determine precisely the quantity of it. For this purpose it will be proper to collect all the preceding observations in one point of view.

Table of the Result of all the Barometrical Experiments.

Place of observation.		True height trigonomet- rically.	Height by the barome- ters.	Mean heat.	Error in feet.	Error in 1000 feet.	Mean error in 1000 feet.
Mont Saleve,	1	2831.3	2775.2	69°.4	— 56.1	—19.8	} —23.1
	2	—	2763.2	68 .5	— 68.1	—24.0	
	3	—	2759.4	67 .2	— 71.9	—25.4	
At the Mole,	1	4211.3	4132.7	58 .3	— 78.6	—18.6	} —21.7
	2	—	4140.1	58 .9	— 71.2	—16.9	
	3	—	4115.1	59 .5	— 96.2	—22.8	
	4	—	4111.9	60 .0	— 99.4	—23.5	
	5	—	4113.7	60 .5	— 97.6	—23.1	
	6	—	4104.9	60 .3	—106.1	—25.2	
Mont Saleve,	1	2828.9	2755.6	57 .5	— 73.3	—25.9	} —26.8
	2	—	2754.9	58 .9	— 74.0	—26.2	
	3	—	2748.9	59 .6	— 80.0	—28.2	
	4	—	2752.8	59 .8	— 76.1	—26.9	

Mean of all, 23.6, and the temperature 61°.4.

The Mole, from two observations of Mr. De Sausure,	}	4211.3	—	—	— 92.	—21.8	}	—16.2
—								
The same by Mr. De Sausure, and Mr. De Luc, at Geneva,	}	4883.	4814.	—	— 69.	—14.		
—								
According to Mr. De Luc's own observations, see Recherches sur l'Atmosphere.	}	the Mole,	4882.8	4860.	— 22.8	— 4.7		
		the Dole,	4292.7	4210.	— 82.7	—19.5		
		the Buet,	8893.6	8770.	—123.7	—13.9		
		M ^t .Blanc,	14432.5	14093.	—339.5	—23.5		

The titles of the columns are sufficiently clear to make a farther explanation of this table unnecessary; and it appears, I think, incontestably, on taking a mean of my 13 observations on Mont Saleve and the Mole, that this error is about $23\frac{1}{2}$ feet on every 1000; that is, the rules of Mr. De Luc give the height by so much too little. At the bottom of the foregoing table I have subjoined 6 other comparisons, some of them from Mr. De Luc's own observations, as recorded in his valuable work; which however I must add, are certainly of less

authority in this inquiry, as they were made with barometers a great way distant from each other, viz. near 30 miles: besides which, the geometrical heights are, for the same reason, not so accurately ascertained. I have, however, ventured to make what use I could of them, viz. to show that these too give a result on the same side, though not exactly the same; and to urge the necessity of a certain vicinity in those observations from which a theory is to be deduced.

Shall I be permitted to adduce another proof, in confirmation of what has been advanced? When I first took up the consideration of measuring altitudes in the atmosphere with the barometer, and had heard only of Mr. De Luc's labours, it occurred to me that there was a much more simple method of arriving at this theory, than either he or I have since pursued. It was this; to determine hydrostatically the specific gravities of air and quicksilver, with a given temperature and pressure; the increase of volume, or change of gravity, with a given increase of heat being supposed to be known by the experiments of Boerhaave and Hawkesbee, which might be further examined by similar ones; and presuming that the geometrical ratio in the air's density, in advancing upwards from the earth's surface, had been sufficiently demonstrated. For the proportional gravity of quicksilver to air will express inversely the length of two equi-ponderant columns of these fluids, that is, when the columns are taken infinitely small.* With these ideas I made the following experiment. I caused a glass vessel to be blown something like a Florence flask, or rather larger; to the neck of this was adapted a brass cap with a valve opening outwards, and made to screw on or off, with a male screw, by which it was fixed to an excellent pump of Mr. Nairne's construction, and exhausted of its air, or at least rarefied to a known degree: the vessel was then carefully weighed with a sensible balance, and again after the air was re-admitted; the difference gave the weight of the air that had been exhausted. After having repeated this 2 or 3 times, the vessel was exactly filled with water as far as the valve, which had been the term of capacity for the air; this was done by screwing on the cap till the superfluous water oozed all out, and on inverting the vessel there appeared not the least sign or bubble of air; I therefore concluded, that the volume of water was precisely the same as had been the volume of air, a circumstance that should be accurately attended to. It was then carefully weighed, and compared with its weight when full and deprived of its air. It will readily be seen, that I had then the specific gravity of the two fluids, on supposition that the figure of the glass had not altered by pressure during the experiment; and this effect may be presumed to have been

* I am not sorry (says Sir G.) to anticipate the reader's remark here, that this observation is not new; since I find that I have been treading the same steps with Mr. Boyle and Dr. Halley, who both made use of this method; the one with a view to determine the limits of the atmosphere; and the other the height of Snowden.—Orig.

the most sensible, when the vessel was filled with water, the pressure at that time being from within. To assure myself of this, I let in a small quantity of air, which formed a bubble of about $\frac{1}{3}$ of an inch in diameter, and on immersing the glass in another vessel of water, by which the pressure within was counterpoised by a pressure without, the bubble seemed to contract itself by a quantity, as I found afterwards, equal to about 2 grains in weight, or $\frac{1}{8000}$ of the whole contents. I therefore concluded, that this correction was hardly worth taking notice of, and still less the effect from external pressure when the glass was exhausted. At every operation the height of the barometer and thermometer (placed close to the vessel when the air was weighed) was noted down, together with the height of the pump-gage, which, compared with the barometer in the room, showed the quantity exhausted. The result of the experiment was as follows, the barometer in the room standing at 29.27 inches, and the heat of the room 53° .

	Grains.
The bottle empty or exhausted till the gage stood at 29.15 inches weighed (determined from 4 different trials, and the balance turning with $\frac{1}{15}$ of a grain).....	2657.40
Increase of weight when filled with air, from 4 trials certain to $\frac{1}{10}$ of a grain.....	+ 16.13
Bottle filled with water, whose heat was 51°	16220.00
Weight of the water, exclusive of the bottle	13562.60

But the bottle was exhausted only in the proportion of 29.15 inches to 29.27 inches; therefore if a perfect vacuum could have been made, the difference of weight would have been 16.22 grains instead of 16.13 grains. Again, the water was colder than the air by 2° ; the one being 53° , and the other only 51° : now water, from former experiments, I find to expand about $\frac{3}{10000}$ with 2° of heat; therefore, if the water had been of the same temperature with the air that was examined, the weight of an equal volume would have been only 13558.5 grains; and lastly, 13558.5 divided by 16.22 gives 836,* and by so much is water heavier than air in these circumstances.

By former experiments I find the specific gravity of the quicksilver of my barometers, compared with rain-water in 68° of heat, as,	13.606 to 1
And $68^{\circ} - 53^{\circ} = 15^{\circ}$, correct therefore for 15° of expansion of quicksilver	+ .018
Correct for 15° of expansion of air	— .031.

True specific gravity of quicksilver, with 53° of heat	13.594
Which multiplied by the specific gravity of air	$\times .836$

Gives for the comparative gravity of quicksilver and air, when the barometer is at 29.27, and the thermometer 53° 11364.6

* Hawkesbee's experiments made the air 850 lighter than water, the barometer being at 29.7; and Dr. Halley supposed it about 800. Mr. Cavendish, in weighing 50 grains of air, when the barometer was at 29 $\frac{3}{4}$, and the thermometer at 50° , concluded the specific gravity of air to be about 800 also. Now my experiment, reduced to the same circumstances with his, would give 817 for this gravity, no great difference in an affair of such delicacy.—Orig.

And lastly, $\frac{1}{10}$ of an inch of quicksilver, when the barometer stands at 29.27 inches (viz. from 29.22 inches to 29.32 inches) with the temperature 53° , is equal to a column of the atmosphere of	91.7
This quantity, according to my barometrical observations, is	93.83
But according to Mr. De Luc's rules	91.66

We see here then that the statical experiment agrees with the result of my barometrical ones to within about 11 inches in 100 feet, and I am not sure that it is not still capable of much further precision; and though perhaps alone it might carry with it, to some persons, a less conclusive testimony, who reluctantly reason from the little to the great, yet, in conjunction with what has been before shown, I think it has considerable weight; and I am the less inclined to reject such an indirect method of proof, as I have the great authorities of Halley and Newton on my side.

I have thus endeavoured to show that the error of the theory is — $\frac{2.36}{1000}$ when the temperature of the air is $61^{\circ}.4$. It remains therefore finally, that we deduce a rule, the error of which shall be nothing, viz. to find the temperature in which the difference of the logarithms of the height of the barometer, taken to 4 places of figures, will give the true difference of elevation in English fathoms. Previous to this investigation, with which I intend to conclude this paper, it will be necessary to remark, that by repeated experiments with the manometer, I find a small difference in the equation for the expansion of air by a change of temperature, and even in that of quicksilver from the same cause, from what Mr. De Luc's observations have given it. I shall not here trouble the reader with the experiments at large, too simple in themselves to deserve such a detail, unless a future occasion should render that necessary, as the methods here used may be met with among Hawkesbee's or Mr. Boyle's experiments; and content myself with relating only the result of the different trials.

1000 parts of air of the temperature of freezing, and pressure of $30\frac{1}{2}$ inches, increased in volume by an addition of 1 degree of heat on Fahrenheit's thermometer as follows: by a mean of 8 experiments with one manometer 2.44; and by a mean of 5 with a second 2.42. The mean of these two sorts of observations made with different instruments, is 2.43, viz. 1000 parts of the air at freezing become by expansion from 1° of heat equal to 1002.43 parts, or 1002.385 parts with the standard temperature $39^{\circ}.7$. Mr. De Luc's experiments reduced give this quantity equal to 1002.23.*

* It has generally been supposed, that air expands $\frac{1}{400}$ with each degree of the thermometer, commencing from the mean temperature 55° ; and, in consequence of this, astronomers have computed tables for correcting their mean refractions; but, on reducing the result of my observations to the temperature 55° , we shall have the correction of $1^{\circ} = \frac{2.43}{1055.89}$ or $\frac{1}{435}$. Now according to Mr. De Luc this equation is $\frac{2.23}{1033.45} = \frac{1}{463}$, which would produce a difference of about 4" in the corrected refraction, on an altitude of 5° , with the temperature 35° . If my numbers may be sup-

It has been suspected, in consequence of some experiments made by a very ingenious member of this society, that air does not expand uniformly with quicksilver; or that the degrees of heat shown by a quicksilver-thermometer would be expressed on a manometer, or air-thermometer, by unequal spaces in a certain geometrical ratio. I do not deny this proposition; but I have also very little reason to assent to it, if I may trust my own experiments, which certainly evince that this ratio, if not truly arithmetical, is so nearly so as to occasion no sensible error in the measuring of heights with the barometer; and that is all I contend for. The small differences that are seen in the above table of this expansion, deduced from a mean of 14° or of 40° , I would attribute rather to the errors of observation than to any actual irregularity in nature. If however this progression be insisted on, it should seem that the degree of the air's expansion increases with an increase of heat; and that the difference of volume or density from 1° of heat, any where within the limits abovementioned, would be about one part in 5000 from what I take it at a mean. I should not have insisted so long on this circumstance, but in respect to the known accuracy of the author of this hypothesis. Neither do I find any reason to believe, that the expansion of air varies with its density. I have tried air whose density or pressure was equal to $23\frac{3}{4}$ inches, and also to 40 inches; but the dilatation, with equal volumes and equal degrees of heat, was very nearly the same in both cases. I might add a great deal more on these manometrical experiments, but am afraid it would be more tedious than useful. I proceed therefore to the expansion of quicksilver.

This experiment was made with a tube, something like a thermometer, but considerably larger than the ordinary size, and open at one end; it was filled with quicksilver to a certain height, and then exposed to the temperatures of freezing and boiling repeatedly, the barometer being at 30 inches: the difference of the volume in each instance was determined afterwards by accurately weighing the contents. I thus found, that if the quicksilver at freezing be supposed to be divided into 13119 parts, the increase of volume by a heat of boiling water became equal to 208 of these parts $= \frac{1}{637}$, and $\frac{1}{637} \times \frac{1}{180} = \frac{1}{114660}$; and such would be the expansion for each degree of the thermometer, commencing from the freezing point, $= 0.00262$ inch on a column of 30 inches of the barometer, if the glass had suffered no expansion during the experiment. This however has been found to be with 180° of heat $= \frac{1}{400}$ in solidity (viz. the cube of its longitudinal expansion) and $\frac{1}{400} \times \frac{1}{180} = \frac{1}{72000} = 0.00042$

posed to deserve equal confidence, the error of the tables in common use, in the above circumstances, would amount to half that quantity, and therefore probably will be thought scarcely worth correcting. I have mentioned this in order to obviate the conclusions that have been drawn by some persons from Mr. De Luc's theory.—Orig.

inch, for the effect of the expansion of the glass for 1° on a column of 30 inches; this added to the quantity before found, which was only the excess of the greater expansion above the less, gives for the true equation for each degree 0.00304 inch when the barometer stands at 30 inches.* Mr. De Luc's correction in this case was 0.00312; a difference so small that I shall take no notice of it as to its influence on our observations. It may deserve a remark here, that this equation, rigorously taken, is variable according to the height of the thermometer; for 1° , which at freezing is $= \frac{1}{999.1}$ of the whole volume, at the temperature 82° becomes $\frac{1}{999.4}$, a difference indeed that may fairly be neglected, and which I neglect myself; yet I cannot help observing, in justice to Mr. De Luc, that his method of reducing his barometers always to the same standard temperature was free from the error I am speaking of.

To conclude, the defect of Mr. De Luc's rules being supposed $\frac{2.36}{10000}$, or which comes to the same thing, the correction being $+\frac{2.417}{10000}$, when the temperature of the air is $61^{\circ}.4$, and the true expansion of the air for each degree being $\frac{2.29}{10000}$ when the heat is $39^{\circ}.7$; required to find the temperature in which the difference of the logarithms shall give the true height in English fathoms, that temperature, according to Mr. De Luc, being $39^{\circ}.74$, and the expansion $\frac{2.29}{10000}$.

Let t be the temperature $61^{\circ}.4$; s Mr. De Luc's standard temperature; e the expansion for 1° ; e the same, according to Mr. De Luc; α the supposed correction of the rules, and x the temperature sought. We have then the following formula, $\frac{(t-s) \times (e-e) + \alpha}{e} = s - x$; hence, proceeding with the above numbers, $s - x$ comes out $8^{\circ}.50$, and consequently $x = 31^{\circ}.24\frac{1}{2}$ the temperature required; which, if it should be thought convenient, may be considered as the freezing point.

In the whole of the above inquiry I have taken no notice of the effect of

* It has been suspected, and I believe will appear from very good observations, which however never made myself, that the expansion of quicksilver in the barometer is not directly as the heat shown by the thermometer, but in a ratio something different, owing to some of the quicksilver being converted into an elastic vapour in the vacuum that takes place at the top of the Torricellian tube, which presses on the columns of quicksilver, and thus counteracts in a small degree the expansion from heat. It does not however appear to be a considerable quantity, not amounting to above a 16th of the whole expansion in a range of 40° of temperature; I shall therefore venture to consider this equation as truly uniform, since the error on 1000 feet would not amount to 5.—Orig.

† This sign is negative, because the assumed expansion e is less than the true one e , and consequently tended to increase the apparent error of the rules; had it been greater, α would have been +.—Orig.

‡ Very nearly the same conclusion has been since found, in a very easy and simple manner, by one observation only, viz. of the whole pressure of the atmosphere at the surface of the earth. See Dr. Hutton's Dictionary published in 1795, art. atmosphere; also his course of mathematics, vol. 2, p. 235, first published in 1798.

gravity on the particles of the air at different distances from the earth's centre, which should doubtless enter into the account, and which would occasion the density of the atmosphere to decrease in a ratio something greater than the present theory admits of. In a height of 4 English miles Dr. Horsley finds (Phil. Trans., vol. 64) that the diminution of density or volume from the accelerative force of gravity would be only $\frac{1}{5000}$ part of the whole, or about 48 feet; and I may add to this, that this effect will be in the duplicate ratio of the heights, so that at one mile high it becomes only 3 feet. A like effect takes place also below the surface of the earth, as in measure of the depths of mines, &c. with this difference, that here it is but half the quantity in the former instance; gravity within the earth being simply as the distance from the centre; they are both of them however circumstances that deserve no attention in practice.

Sir G. S. has drawn up in a commodious form, the necessary tables and precepts for calculating any accessible heights or depths from barometrical observations, without which he thought the preceding memoir would be incomplete. He has avoided the method of logarithms, proposed by Dr. Halley, and adopted by Mr. De Luc, both because such tables are not in the hands of every body, and because he had perceived that many persons of a philosophical turn, though skilled only in common arithmetic, have been frightened by the very name: a method less popular, however elegant, he thought would have been less generally useful. To these tables is subjoined a list of several altitudes, as determined by the barometer: this served to show the use he had made of the instrument, and at the same time exhibits the level of a great number of places in France, Savoy, and Italy. But first he premises the following short principles as the grounds of the computation.

1st. The difference of elevation of two places may be determined by the weight of the vertical column of the atmosphere intercepted between them. 2d. If then the weight of the whole atmosphere at each place can be ascertained, the weight of this column, viz. their difference, will be known. 3d. But the height of the quicksilver in the barometer expresses the total weight of the atmosphere in the place of observation; the difference therefore of the height of the barometer, observed in two places at the same time, will express the difference of elevation of the two places. 4th. But further, the weight of this column of the atmosphere is liable to some variations, being diminished by heat, augmented by cold; and again, a similar alteration takes place in the column of quicksilver, which is the measure of this weight. 5th. If then the degree of these variations can be determined, and the temperature of the air and quicksilver at the time of observation be known, the weight of this column of air, or the difference of elevation of the two places, will be concluded as certainly as

if the gravity of these two fluids, with all heats, remained invariably the same: this is the whole mystery of barometrical admeasurement.*

XXX. An Account of the Bramin's Observatory at Benares. By Sir Robert Barker, Knt. F. R. S. p. 598.

Benares in the East-Indies, one of the principal seminaries of the Bramins, or priests of the original Gentoos of Hindostan, continues still to be the place of resort of that sect of people; and there are many public charities, hospitals, and pagodas, where some thousands of them now reside. Having frequently heard that the ancient Bramins had a knowledge of astronomy, and being confirmed in this by their information of an approaching eclipse both of the sun and moon, I made inquiry, when at that place in the year 1772, among the principal Bramins, to endeavour to get some information relative to the manner in which they were apprized of an approaching eclipse. The most intelligent that I could meet with however gave me but little satisfaction. I was told, that these matters were confined to a few, who were in possession of certain books and records; some containing the mysteries of their religion, and others the tables of astronomical observations, written in the Skanskirrit language, which few understood besides themselves: that they would take me to a place which had been constructed for the purpose of making such observations as I was inquiring after, and from which they supposed the learned Bramins made theirs. I was then conducted to an ancient building of stone, the lower part of which, in its present situation, was converted into a stable for horses, and a receptacle for lumber; but, by the number of court-yards and apartments, it appeared that it must once have been an edifice for the use of some public body of people. We entered this building, and went up a staircase to the top of a part of it, near the river Ganges, that led to a large terrace, where, to my surprize and satisfaction, I saw a number of instruments yet remaining, in the best preservation, stupendously large, immoveable from the spot, and built of stone, some of them being upwards of 20 feet in height; and though they are said to have been erected 200 years before, the graduations and divisions on the several arcs appeared as well cut, and as accurately divided, as if they had been the performance of a modern artist. The execution in the construction of these instruments exhibited a mathematical exactness in the fixing, bearing, and fitting of the several parts, in the necessary and sufficient supports to the very

* Those are general principles, that apply to all modes of calculation, either with logarithms or otherwise. Most persons make use of logarithms, as the simplest and easiest way. Sir G. S. in avoiding logarithms, employs other tables of his own construction, but less generally useful. Instead of these, reference may be made to the books beforementioned, or to many other popular works on the subject.

large stones that composed them, and in the joining and fastening them into each other by means of lead and iron cramps.

The situation of the two large quadrants of the instrument marked A in pl. 3, whose radius is 9 feet 2 inches, by being at right angles with a gnomon at 25 degrees elevation, are thrown into such an oblique situation as to render them the most difficult, not only to construct of such a magnitude, but to secure in their position for so long a period, and affords a striking instance of the ability of the architect in their construction; for, by the shadow of the gnomon thrown on the quadrants, they do not appear to have altered in the least from their original position; and so true is the line of the gnomon, that, by applying the eye to a small iron ring of an inch diameter at one end, the sight is carried through 3 others of the same dimension to the extremity at the other end, distant 38 feet 8 inches, without obstruction; such is the firmness and art with which this instrument has been executed. This performance is the more extraordinary when compared with the works of the artificers of Hindostan at this day, who are not under the immediate direction of a European mechanic; but arts appear to have declined equally with science in the east.

Liet. Col. Arch. Campbell, at that time chief engineer in the East-India Company's service at Bengal, a gentleman whose abilities do honour to his profession, made a perspective drawing of the whole of the apparatus that could be brought within his eye at one view; but I lament he could not represent some very large quadrants, whose radii were about 20 feet, being on the side whence he took his drawing. Their description however is, that they are exact quarters of circles of different radii, the largest of which I judged to be 20 feet, constructed very exactly on the sides of stone walls built perpendicular, and situated, I suppose, in the meridian of the place: a brass pin is fixed at the centre or angle of the quadrant, from which the Bramin informed me they stretched a wire to the circumference when an observation was to be made; from which it occurred to me, the observer must have moved his eye up or down the circumference, by means of a ladder or some such contrivance, to raise and lower himself, till he had discovered the altitude of any of the heavenly bodies in their passage over the meridian, so expressed on the arcs of these quadrants: these arcs were very exactly divided into 9 large sections; each of these again into 10, making 90 lesser divisions or degrees; and those also into 20, expressing 3 minutes each, of about $\frac{2}{10}$ of an inch asunder; so that it is probable, they had some method of dividing even these into more minute divisions at the time of observation.

My time would only permit me to take down the particular dimensions of the most capital instrument, or the greater equinoctial sun-dial, represented by figure A, plate 3, which appears to be an instrument to express solar time by the

shadow of a gnomon on two quadrants, one situated to the east, and the other to the west of it; and indeed the chief part of their instruments at this place appear to be constructed for the same purpose, except the quadrants, and a brass instrument described hereafter. Figure *b* is another instrument for the purpose of determining the exact hour of the day by the shadow of a gnomon, which stands perpendicular to and in the centre of a flat circular stone, supported in an oblique situation by means of 4 upright stones and a cross piece; so that the shadow of the gnomon, which is a perpendicular iron rod, is thrown on the divisions of the circle described on the face of the flat circular stone. Figure *c* is a brass circle, about 2 feet diameter, moving vertically on 2 pivots between 2 stone pillars, having an index or hand turning round horizontally on the centre of this circle. This instrument appears to be made for taking the angle of a star at setting or rising, or for taking the azimuth or amplitude of the sun at rising or setting.

The use of the instrument, figure *d*, I was at a loss to guess. It consists of 2 circular walls; the outer of which is about 40 feet diameter, and 8 feet high; the wall within about half that height, and appears intended for a place to stand on to observe the divisions on the upper circle of the outer wall, rather than for any other purpose; and yet both circles are divided into 360 degrees, each degree being subdivided into 20 small divisions, the same as the quadrants. There is a door-way to pass into the inner circle, and a pillar in the centre, of the same height with the lower circle, having a hole in it, being the centre of both circles, and seems to be a socket for an iron rod to be placed perpendicularly in it. The divisions on these, as well as all the other instruments, will bear a nice examination with a pair of compasses. Figure *e* is a smaller equinoctial sun-dial, constructed on the same principle as the large one *a*.

This observatory at Benares is said to have been built by the order of the emperor Ackbar; for as this wise prince endeavoured to improve the arts, so he wished also to recover the sciences of Hindostan, and therefore directed that 3 such places should be erected: one at Delhi, another at Agra, and the 3d at Benares. Some doubts have arisen with regard to the certainty of the ancient Bramins having a knowledge in astronomy, and whether the Persians might not have introduced it into Hindostan when conquered by that people: but these doubts I think must vanish, when we know that the present Bramins pronounce, from the records and tables which have been handed to them by their forefathers, the approach of the eclipses of the sun and moon, and regularly as they advance give timely information to the emperor and the princes in whose dominion they reside. There are yet some remains in evidence of their being at one time in possession of this science. The signs of the zodiac, in some of their choultrys on the coast of Coromandel, as remarked by John Call, Esq., F. R. S., in his

letter to the Astronomer Royal, requires little other confirmation. Mr. Call says, that as he was lying on his back, resting himself in the heat of the day, in a choultry at Verdapetah in the Madura country, near Cape Commorin, he discovered the signs of the zodiac on the ceiling of the choultry: that he found one, equally complete, which was on the ceiling of a temple, in the middle of a tank before the pagoda Teppecolum near Mindurah; and that he had often met with several parts in detached pieces. See Philos. Trans., 1772, p. 353, or vol. 13, p. 321, of these abridgments. These buildings and temples were the places of residence and worship of the original Bramins, and bear the marks of great antiquity, having perhaps been built before the Persian conquest. Besides, when we know that the manners and customs of the Gentoo religion are such as to preclude them from admitting the smallest innovation in their institutions; when we also know that their fashion in dress, and the mode of their living, have not received the least variation from the earliest account we have of them; it cannot be supposed they would engrave the symbolical figures of the Persian astronomy in their sacred temples; the signs of the zodiac must therefore have originated with them, if we credit their tradition of the purity of their religion and customs.

Mr. Fraser, in his History of the Mogul Emperors, speaking of time says, “the lunar year they reckon 354 days, 22 gurris, 1 pull; the solar year they reckon 365 days, 15 gurris, 30 pulls, $22\frac{1}{4}$ peels; 60 peels making 1 pull, 60 pulls, 1 gurri, and 60 gurris 1 day. This is according to the Bramins or Indian priests, and what the Moguls and other Mahommedans in India chiefly go by.” Thus far Mr. Fraser; and it serves to strengthen the argument for supposing that the Bramins had a knowledge of astronomy before the introduction of Mahometanism into Hindostan.

Dimensions of the larger equinoctial sun-dial, pl. 3.

	Feet. In.			Ft. In.	
Length of the gnomon at the base bb.	34	8	Thickness gg.	1	0
Oblique length of the gnomon cc.	38	8	Breadth of the gnomon hh.	4	6
Radius of the quadrants aa	9	2	Whole extent of the instrument ii	37	4
Height of the gnomon at d.	22	3	Lat. of the place taken by double alt.	25°	10'
Breadth of the quadrants ff.	5	10			

XXXI. A short Account of Dr. Maty's Illness, and of the Appearances in the Dead Body, which was examined on the 3d of Aug., 1776, the Day after his Decease. By Dr. Hunter and Mr. Henry Watson, FF. R. S. p. 608.

About 2 weeks before he died, Dr. Maty was taken with a violent oppressive pain, just above the pit of the stomach, which made him feel as if he was very near dying. He was bled, and gradually recovered; yet so imperfectly, that any motion of his body, or any pressure on that part with the point of a finger, instantly brought on such pain, that he was convinced the least addition to what

he had several times felt, must have put an end to his life. He had an idea that there might be a collection of matter behind the sternum, which might be discharged by some chirurgical operation. On examining the part, which with the whole body was very much emaciated, there was no protrusion or discoloration. All thoughts of making any perforation were laid aside; and it was thought probable, that there was some inflammation or adhesion of the pericardium, or of the heart itself, at its anterior part, just above the diaphragm. His cough was almost incessant in the night since he had left off the use of opium, to which he had been long accustomed. For 7 or 8 years, he said, he believed he might have had 20 purging stools in every 24 hours, from a complaint in his bowels, the principal seat of which he pointed out so exactly in his emaciated state, that it was observed at the time it must be in the colon, where it passes down on the outside of the lower end of the left kidney. It was therefore thought probable that there was contraction with internal ulceration of the gut at that place: and about 3 years before, with this complaint, which always continued in his bowels and left side, he had a fistula in ano, for which he was cut, and so cured of that disorder; but from that time, he was always sensible that the lower part of the rectum remained in an aukward, uneasy state, so that it was difficult and painful to pass a common glister-pipe into it. His medical friends were of opinion, that no more could be done for him than to palliate, and to procure ease and sleep. He returned to his opium, of which he took one grain twice a day; and at times was much relieved and comforted by it.

The heart and lungs were examined with great care, but there was hardly any appearance of disorder in either, contrary to what was expected. The conjecture that had been formed about the complaint in the bowels proved to be perfectly just. The small intestines were apparently pretty sound; the cæcum and beginning of the colon were much distended with air, but not inflamed. The arch, or transverse turn of the colon, was likewise much distended, and its blood-vessels were so loaded, that there was, at first sight, the outward appearance of an internal inflammation. The enlarged part of the colon terminated at the lower end of the left kidney, where there was an annular stricture on the outside of the gut, and there the gut felt hard and fleshy. The enlarged part being slit up, was much inflamed and superficially ulcerated on the inside, and more in proportion towards the lower end. At the stricture there was but a very small passage left, winding irregularly through an inch and a half of hard ulcerated gut. Below this, where the colon passes over the psoas and iliac vessels, it was in its natural state; but the rectum had been at some former time very much diseased, and for a finger's length to within 2 inches of the anus was contracted to almost a goose-quill size, and of a livid colour. The lower 2 inches were not so much contracted, but of the same livid colour, and the surface of

the gut there was almost as unequal as the fasciculated surfaces in the heart; the effect probably of universal ulceration there, which had been a part of, or a companion to, the fistula, of which he had been cured by the operation; for, on that part, the villous coat of the intestine was destroyed.

To this account, more particularly of the last 2 weeks of Dr. Maty's illness, and of the appearances on opening the body, as drawn up by Dr. Hunter, Mr. W. added the few following remarks.

The heart and lungs were indeed neither of them essentially diseased; yet there was a whitish spot, about the breadth of a sixpence, on the right ventricle of the heart, near its apex; a rough border on the left side of the diaphragm, as if the lungs had been glued to that part and torn off again; a partial adhesion of the lungs to the pleura; and a little purulent fluid within the pericardium. Certainly these were some signs of a slight inflammation having attacked the membranes investing the contents of the thorax. Neither can we suppose such appearances to have existed without occasioning some uneasiness: they were perhaps sufficient to account for that great tenderness and oppressive pain which the doctor felt from the least pressure on the sternum, or on any part of the breast near it.

The principal seat of the disease which proved so tedious, and in the end so fatal, was doubtless confined to the colon only; and it was entirely within the gut. The part first affected must have been that portion of the canal in which we observed the most mischief. The superficial extent of the disease over so large a surface as the whole arch of the colon, and the more formidable appearance of it, in only a few inches of the same gut, distinguished the part where the disease first began, and where it must have had its longest duration. The cause of all this mischief was conjectural with Dr. Maty himself. Had it arisen, as he suspected, from having bruised his side with the hilt of his sword, we then should have found the gut injured from without inwards. But is it not most likely, that a little bit of bone, the stone of fruit, some sharp or hard body, in passing, had injured the gut so much, as to lay a foundation for all the growing complaints? Nearly the same appearances have been observed in the œsophagus from only a hard crust of bread lodging for a time in the passage; which, after being forced down, was succeeded by great soreness, inflammation, ulceration, and at length so complete an obstruction, as to occasion the death of the patient; of which Mr. W. once saw a very deplorable instance.

The ulcerated intestine is a disease generally, as in the case before us, slow in its progress, but certainly fatal. An accumulation of acrid matter, confined air, solid ingesta, in short any thing capable of stretching, irritating, or hardening the gut, will spread and increase the disease. The fasciculated appearance in the rectum is what Mr. W. had once met with in a very sound gut, where the villous

coat was not in the least injured; it is therefore sometimes an original conformation, but apparently unnecessary, as the gut, we may presume, would perform its office much more agreeably without it.

XXXII. An Account of some Experiments made with an Air-pump on Mr. Smeaton's Principle; also some Experiments with a Common Air-pump. By Mr. Edward Nairne, F. R. S. p. 614.

As the following experiments were made principally to try the performance of Mr. Smeaton's pear-gage, it may be proper, says Mr. N., to observe the description of it, in his own words, in the Philos. Trans., for the years 1751 and 1752, vol. 47, p. 420, or p. 247, vol. 10, of these abridgments. The pump with which the chief of Mr. N.'s experiments were made, had the leather of its piston soaked in oil and tallow, with oil in the barrel, and every precaution was taken that no water should get into the working parts of the pump, except what might arise in vapour from the substances under the receiver. In trying some experiments with the pump having Mr. Smeaton's pear-gage, and with the common long barometer-gage, the degrees of exhaustion as indicated by the two were surprizingly different. By the common long gage it appeared that the exhaustion was 300 times, whereas by the new or pear-gage, it appeared to be 6000 times. By which it appeared that the pressure on the surface of the quicksilver in the cistern, and in the tube of the long barometer-gage, was diminished to about a 300th part: the pear-gage being now pushed down till its open end was immersed under the surface of the quicksilver in the cup, the air was then let in, and the pump appeared by that gage to have exhausted all but a 6000th part of the air; or, in other words, the degree of exhaustion by this appeared to be 6000 times.

Finding always this disagreement between the pear-gage and the other gages, Mr. N. tried a variety of experiments; but none of them appeared satisfactory, till one day in April 1776, showing an experiment with one of these pumps to Mr. Cavendish, Mr. Smeaton, and several other gentlemen of the R. S., when the two gages differed some thousand times from one another, Mr. Cavendish accounted for it in the following manner. "It appeared," he said, "from some experiments of his father's, Lord Charles Cavendish, that water, whenever the pressure of the atmosphere on it is diminished to a certain degree, is immediately turned into vapour, and is as immediately turned back into water on restoring the pressure. This degree of pressure is different according to the heat of the water; when the heat is 72° of Fahrenheit's scale, it turns into vapour as soon as the pressure is no greater than that of three quarters of an inch of quicksilver, or about a 40th part of the usual pressure of the atmosphere; but when the heat is only 41°, the pressure must be reduced to that of a quarter of an

inch of quicksilver before the water turns into vapour. It is true, that water exposed to the open air will evaporate at any heat, and with any pressure of the atmosphere; but that evaporation is entirely owing to the action of the air upon it: whereas the evaporation here spoken of is performed without any assistance from the air. Hence it follows, that when the receiver is exhausted to the abovementioned degree, the moisture adhering to the different parts of the machine will turn into vapour and supply the place of the air, which is continually drawn away by the working of the pump, so that the fluid in the pear-gage, as well as that in the receiver, will consist in a good measure of vapour. Now letting the air into the receiver, all the vapour within the pear-gage will be reduced to water, and only the real air will remain uncondensed; consequently the pear-gage shows only how much real air is left in the receiver, and not how much the pressure or spring of the included fluid is diminished, whereas the common gages show how much the pressure of the included fluid is diminished, and that equally whether it consist of air or of vapour."

Mr. Cavendish having explained so satisfactorily the cause of the disagreement between the two gages, Mr. N. considered, that if he were to avoid moisture as much as possible, the two gages should nearly agree: this induced him to make the following experiment.

The plate of the pump being made as clean and as dry as possible, there was then put on it the beforementioned short barometer-gage, also the pear-gage, with a cistern entirely of glass which held the quicksilver; they were then covered with a receiver, round the outside of which was laid a cement which perfectly excluded the outer air; every part, before it was put under the receiver, as well as the receiver itself, being made as clean and as free from moisture as possible.* The pump was then worked for 10 minutes, and the barometer-gages indicated a degree of exhaustion nearly 600; the air was then let into the receiver, the pear-gage indicated a degree of exhaustion, but very little more than 600 also. The near agreement of the pear-gage with the barometer-gages in this last experiment, in which Mr. N. had been so careful to exclude the moisture as much as possible, seemed to prove beyond a doubt, that their disagreeing in the several former experiments must have been owing, as Mr. Cavendish supposed, to the moisture which in them had not been so carefully excluded. But he began now to suspect also, that there might arise a vapour from some moisture that might be contained in the leather soaked in oil and tallow, or in the wooden foot which was cemented to the glass cup, both

* It may be proper here to notice, that the pump in every experiment hereafter mentioned was worked 10 minutes, and the same receiver continued cemented to the pump plate, except where it is otherwise mentioned. The top of this was made to open, in order to put in different things.—Orig.

used in the former experiments: these suspicions induced him to try the following experiments.

A piece of leather dressed in alum, known by the name of white sheep-skin, of about 4 inches diameter, which had been soaked in oil and tallow about a year before (such as was used to place the receiver on in the 1st and 2d experiments) was put into the receiver; the pump was then worked, and the barometer gage indicated a degree of exhaustion of nearly 300; but on the admission of the air the pear-gage indicated a degree of exhaustion of 4000. But the piece of leather being taken out, the pump was then worked, and the degree of exhaustion appeared by both the barometer and pear-gages to be about 600, as before.

Again, a cylinder made of a piece of box-wood, 1 inch in diameter and 3 inches in length, was put into the receiver; the pump was then worked, and the degree of exhaustion appeared by the barometer-gage to be 300, but by the pear-gage 16,000. These experiments have often been repeated, but the result was seldom the same. When leather soaked in oil and tallow has been put into the receiver, the pear-gage has sometimes indicated a degree of exhaustion of 20,000, and sometimes no more than 500; it likewise differs very much from the box-wood, which may perhaps be owing to different degrees of heat and moisture.

From these experiments it is evident, that there arises an elastic vapour from the leather dressed in alum and soaked in oil and tallow, and also from the piece of box-wood, when the weight of the atmosphere has been partly taken off by the action of the pump; and that this vapour presses on the surface of the quicksilver in the tube of the long barometer-gage, and of that in the cistern of the short one; and that consequently the testimony of both these gages must be influenced by this vapour, as well as by the small remainder of common air: but as it is the nature of the pear-gage not to give its testimony till the remaining air contained in it is pressed, so as to become of the same density of the atmosphere; and as this vapour cannot subsist in the form of vapour under that pressure, this gage is not at all influenced by it, but indicates the remaining quantity of permanent air only.

Seeing thus what a considerable quantity of vapour arose from the compound of leather, alum, oil, and tallow, Mr. N.'s next object was to find out from which of those substances it chiefly arose; how far he succeeded appeared by other experiments. From these it appeared that the elastic vapour, which caused so great a difference in the testimony of the gages, arose principally from the leather, and but little from the tallow, oil, or alum: it even appears by one experiment, that it came from the leather, and supplied the place of the exhausted

air so fast, that he could not (at least in the 10 minutes) make the barometer-gage indicate a degree of exhaustion of more than 152.

To determine whether it was the moisture in the leather from which the vapour arose, Mr. N. made other experiments; viz. with a piece of white leather fresh from the leather-sellers; then with the same piece of leather dried by the fire till it would lose no more of its weight; and 3dly, with the same piece of leather held in the steam of hot-water till it had regained the 20 grains weight it had been deprived of by the drying. Hence it appeared that in the 1st and 3d of these 3 states the barometer-gage indicated an exhaustion of only about 140, while the pear-gage showed about 100,000; but in the 2d case they showed nearly alike, the former showing an exhaustion of about 270, and the latter of about 280.

Being now perfectly satisfied, from these and other experiments, that the variation in the testimony of the pear and barometer-gages was occasioned by the moisture contained in the substances which had been put into the receiver assuming the form of vapour; Mr. N. determined next to try what would be the effect of the vapour which might arise from small quantities of different fluids, and from some other substances containing moisture of various kinds. And observing by these experiments that the small quantity of moisture which exhaled from the substances under the receiver prevented the pump from exhausting it to any very considerable degree, he began to suspect that whenever wet leather had been used to connect the receiver with the plate, there must have arisen so great a quantity of vapour as to have prevented the degree of exhaustion being near so great as in some of the foregoing instances. These suspicions induced him to make still further experiments.

The great difference in the testimony of the pear-gage in these last six experiments appeared to him exceedingly astonishing, for the leathers seemed each of them to be as moist at last as at first. By which he was convinced how effectually the use of leather soaked in water, or in water and spirit of wine, prevents the pump from exhausting to any considerable degree. He made a number of experiments of the same kind as these; but never was able to exhaust, under such circumstances, to a greater degree than between 50 and 60, when the heat of the room was about 57° by a thermometer of Fahrenheit's scale.

From these and many other experiments, it evidently appears, says Mr. N., that the air-pump of Otto Guericke, and those contrived by Mr. Gratorix, and Dr. Hooke, and the improved one by Mr. Pappin, both used by Mr. Boyle, also Hauksbee's, S'Gravesande's, Muchenbroeck's, and those of all who have used water in the barrels of their pumps, could never have exhausted to more than between 40 and 50, if the heat of the place was about 57° ; and though Mr. Smeaton, with his pump, where no water was in the barrel, but where leather

soaked in a mixture of water and spirit of wine was used to set the receiver on the pump-plate, may have exhausted all but a 1000th or even a 10,000th part of the common air, according to the testimony of his pear-gage; yet so much vapour must have arisen from the wet leather, that the contents of the receiver could never be less than a 70th or 80th part of the density of the atmosphere: yet it does not seem that any deficiency in the construction of Mr. Smeaton's pump was the cause of his not being able to exhaust beyond the low degrees of 70 or 80. Had he been aware of the bad effects of setting the receiver on leather soaked in water and spirit of wine; and had he made use of the precaution to free all parts of his pump as much as possible from moisture, I make not the least doubt but the air-pump, which he executed himself, would have exhausted to as great a degree, as that pump has been seen to have done with which the chief of these experiments were made. I must here again observe, that if we only wish to know the quantity of permanent air remaining in the receiver after it is as much exhausted as possible, it seems that it is by Mr. Smeaton's gage only that we can know it. Again, when by the assistance of his gage and the barometer-gage together, we have discovered that there is a vapour which arises and occupies the place of the permanent air which is exhausted, it seems that it is by the means of his gage only that we can discover what part of the remaining contents of the receiver consist of this vapour, and what part of permanent air.

By some further experiments, and some of the former repeated, the results were very irregular, sometimes the one gage showing the greater degree of exhaustion, and sometimes the other, in a very extraordinary and unaccountable manner. For, during the course of these experiments on the air-pump it appeared, by the testimony of the pear and barometer-gages, that the remaining contents of a receiver, when exhausted as much as possible, was at different times of different kinds; sometimes it seemed to consist entirely of permanent air, as when a little vitriolic acid, &c. was put in the receiver; and sometimes mostly of vapour arising from moisture, and but a very small proportion of permanent air, as when a bit of damp leather, &c. was in the receiver.

XXXIII. On the Culture of Pine-apples. An extract of a Letter from William Bastard, Esq. of Kitley in Devonshire. p. 649.

In the front part of the hot-house, and indeed any where in the lowest parts of it, the pine-apple plants will not thrive well in water. The way in which I treat them is as follows: I place a shelf near the highest part of the back wall, so that the pine-plants may stand without absolutely touching the glass, but as near it as can be: on this shelf I place pans full of water, about 7 or 8 inches deep; and in these pans I put the pine-apple plants, growing in the same pots

of earth as they are generally planted in to be plunged into the bark-bed in the common way; that is, I put the pot of earth, with the pine plant in it, in the pan full of water, and as the water decreases I constantly fill up the pan. I place either plants in fruit, or young plants as soon as they are well rooted, in these pans of water, and find they thrive equally well: the fruit reared in this way is always much larger, as well as better flavoured, than when ripened in the bark-bed. I have more than once put only the plants themselves without any earth, I mean after they had roots, into these pans of water, with only water sufficient to keep the roots always covered, and found them flourish beyond expectation. In my house, the shelf I mention is supported by irons from the top, and there is an intervening space of about 10 inches between the back wall and the shelf. A neighbour of mine has placed a leaden cistern on the top of the back flue (in which, as it is in contact with the flue, the water is always warm when there is fire in the house) and finds his fruit excellent and large. My shelf does not touch the back flue, but is about a foot above it; and consequently the water is only warmed by the air in the house. Both these methods do well. The way I account for this success is, that the warm air always ascending to the part where this shelf is placed, as being the highest part of the house, keeps it much hotter than in any other part. The temperature at that place is, I believe, seldom less than what is indicated by the 73d degree of Fahrenheit's thermometer, and when the sun shines it is often at above 100°: the water the plants grow in seems to enable them to bear the greatest heat, if sufficient air be allowed; and I often see the roots of the plants growing out of the holes in the bottom of the pot of earth, and shooting vigorously in the water.

My hot-house, the dimensions of which it may be proper to know, is 60 feet long, and 11 feet wide, the flues included; 6 feet high in the front, and 11 feet at the back on the inside of the house. It is warmed by 2 fires. A leaden trough or cistern on the top of the back flue is preferable to my shelf, as in it the pine plants grow much faster in the winter, the water being always warmed by the flue: of this I have seen the great benefit these last 2 months in my neighbourhood. It is not foreign to this purpose to mention that, as a person was moving a large pine plant from the hot-bed in my house last summer, which plant was just showing fruit, by some accident he broke off the plant just above the earth in which it grew, and there was no root whatever left to it: by way of experiment I took the plant, and fixed it upright in a pan of water, without any earth whatever, on the shelf; it there soon threw out roots, and bore a pine-apple that weighed upwards of 2 pounds.

XXXIV. Experiments and Observations made in Britain, in order to obtain a Rule for Measuring Heights with the Barometer. By Colonel William Roy, F. R. S. p. 653.*

Ever since the discovery made by Torricelli, the barometer has been applied, by different persons, in different countries, to the measurement of vertical heights, with more or less success, according to the state of the instruments used, and the particular modes of calculation adopted, by the observers. But of all those who have hitherto employed themselves in this way, none has bestowed so much time and pains, or succeeded so well, as Mr. De Luc, of Geneva, F. R. S. In two quarto volumes, published some years since, that gentleman has given us the history of the barometer and thermometer, with a very curious and elaborate detail of many years experiments, made by him, chiefly on the mountain Saleve. Now the rule, deduced from the observations on Saleve, consists of 3 parts. 1st. The equation for the expansion of the quicksilver in the tube, from the effect of heat, by which the heights of the columns, in the inferior and superior barometers, are constantly reduced to what they would have been in the fixed temperature of $54\frac{1}{4}^{\circ}$ of Fahrenheit, independent of the pressure they respectively sustained. 2d. When the mean temperature of the column of air to be measured, is $60^{\circ}.32$, as indicated by thermometers exposed to the sun's rays at its extremities; then the difference of the common logarithms, of the equated heights of quicksilver in the two barometers, gives the altitude inter-

* Col. Wm. Roy died July 1, 1790, at his house in Argyle-street, after only 2 hours illness: his age not known. At the time of his death he was deputy quarter-master general, colonel of the 30th regiment of foot, surveyor-general of the coasts, major-general in the army, and a very respectable member of the Royal and Antiquarian Societies. General Roy was a native of Scotland, and had formerly been an officer in the royal artillery, but changed on promotion into the line. Besides the present elaborate paper on the measurements of altitudes by the barometer, which is his first in the Philos. Trans. he has 3 other valuable communications in the same Trans. vols. 75, 78, and 80; the former on the measurement of a base on Hounslow-leath, for which he was honoured with the R. S.'s gold medal; and the latter on the relative situations of the observatories of Greenwich and Paris. General Roy had been employed, at different times of his long life, on several important public works: in the winter of 1746 he made an actual survey of Scotland, called the duke of Cumberland's map, which was on a very large scale, and deposited in the Ordnance office; this he afterwards reduced to a smaller size, and had engraven, under the title of the King's Map. In 1784 he was employed on the measurement of the base on Hounslow-leath, abovementioned, as preparatory to the Trigonometrical Survey, afterwards accomplished, for determining the relative situations of the two national observatories of France and England, noticed above. By command of his majesty, he had undertaken and just completed a most curious, accurate, and elaborate set of trigonometrical experiments and observations, to determine the exact latitude and longitude of those two observatories; an account of which, illustrated by tables computed from actual measurements, he had drawn up, and presented to the R. S. and was superintending the printing of it at the time of his death, which happened as before mentioned. This account of the operations was printed in the Phil. Trans. vol. 80, for the year 1790.

cepted between them, in toises and 1000th parts, reckoning the 3 figures to the right hand decimals, and the others integers, the index being neglected. This temperature of $69^{\circ}.32$, when the logarithmic differences give the real height without any equation, is reduced to $39^{\circ}.74$, the new zero of Mr. De Luc's scale, when his formula is adapted to English fathoms and 1000th parts, instead of French toises. And lastly, when the mean temperature of the air is above or below $39^{\circ}.74$, an equation, amounting to $\frac{2}{10000}$ parts of the logarithmic height for each degree of difference, is, in the first case to be added to, and in the last subtracted from, that result, in order to obtain the real altitude.

In Mr. De Luc's book, the experiments for ascertaining the expansion of the quicksilver, are not given in detail; neither are the particular temperatures of the barometers specified. The winter season was however chosen for the purpose; one being left in a cold room, and the other in a closet, heated as high as could conveniently be suffered. The operation having been repeated several times without any essential difference in the results, this general conclusion is drawn, that between the temperatures of melting ice and boiling water, the expansion of the quicksilver is exactly 6 French lines, or .532875 decimal parts of an English inch. But it is to be observed, that the barometer stood then at 28.77525; whereas, if it had stood at 30 inches, it would have been .555556, because the expansion is in proportion to the length of the column. Further, the interval between the freezing and boiling points, in all thermometers, varies with the height of the barometer, or weight of the atmosphere; and it is the custom in England to make thermometers when the barometer stands at 30 inches; that is 1.225 or 13.8 French lines, higher than when Mr. De Luc's boiling point was fixed: and since from his experiments it appears, that each line of additional height in the barometer, raises the boiling point $\frac{1}{13.8}$ part of the interval between that and freezing, it follows that $\frac{1.225}{13.8} = 0.08877$ will denote the number of degrees, that Mr. De Luc's boiling point is lower than that of English thermometers; which reduces it to 209.8 of Fahrenheit, and makes the interval between freezing and boiling only 177.8 degrees. Hence the expansion .532875, above found, must be increased in the proportion of 177.8 to 180, which gives for the total .5624297 or .56243, on a difference of temperature of 180° . Thus the expansion for each degree, supposing it to be arithmetical, or uniformly the same in all parts of the scale, will be .00312461. Having now shewn the expansion of quicksilver in the tubes of barometers resulting from the Geneva observations, I shall next proceed to give some account of those I made for that purpose.

SECT. 1. *Experiments on the expansion of quicksilver.*—The experiments made for this purpose were numerous as well as various, and were therefore subdivided into several classes. To give a minute detail of them all, would be extremely

tedious, and now wholly useless, since it was from those of the 3d class alone, that the rate as well as maximum of expansion was ascertained. Having, in different ways, made a great variety of ingenious and nice experiments, some with tubes of more than $\frac{3}{10}$ of an inch in diameter, of the ordinary length, with a vacuum over the quicksilver of $2\frac{1}{2}$ or 3 inches, part of which reached above the top of the vessel. The mean of 3 experiments gave .5258, for the total dilatation of 30 inches of quicksilver, on 180° between freezing and boiling; that answering to the first 20° , between 32° and 52° , was .0688; that for the 20° in the middle of the scale, between 112° and 132° , was .058; and the rate for the last 20° , between 192° and 212° , was only .041. From this first set of the 3d class of experiments, it appeared evident, that the expansion of 30 inches of quicksilver in the barometer, suffering a heat equal to 180° of Fahrenheit, instead of exceeding Mr. De Luc's, as appeared to be the case from the results of the open tube, really fell short of it: and instead of being arithmetical or uniformly the same, for equal changes of temperature, was actually progressive; the expansion answering to the lower part of the scale, being greater than that corresponding to the middle; which again exceeded that for high temperatures. In these experiments, when the water had acquired a heat 20 or 30 degrees greater than that of the open air, a certain dustiness was perceived in the vacuum of the tube. About 100° of Fahrenheit this appearance had so far increased, as to show clearly, that it could proceed from no other cause than a vapour arising from the surface of the heated quicksilver, quite invisible, till, by its condensation in the cold part of the tube, it was formed into balls, every where adhering to its sides and summit. These globules were very small near the surface of the water, but augmenting gradually as they approached the top of the tube, where they were greatest: their bulk increased with the heat; and when the water was at or near boiling, they would sometimes unite, and descend by their own gravity, along the sides of the tube, into the general mass. Hence the progressive diminution of the rate of expansion of the column of quicksilver in the barometer, is easily accounted for by the resistance of the elastic vapour,* acting against the top of the tube, which was here colder than the rest.

But in the application of the barometer to the measurement of heights, the whole instrument is of the same temperature; therefore, in the 2d set of this 3d class of experiments, the tin vessel was heightened, that tubes of the ordinary length, placed in it, might be wholly immersed in boiling water. The mean of 4 experiments, which agreed very nearly among themselves, gave .5117 for the

* Having mentioned the circumstances to Mr. Ramsden, it first occurred to him, that the resistance of the elastic vapour was the cause of the diminution in the rate of expansion.—Orig.

total expansion between freezing and boiling; for the 20° , between 112° and 132° , it was .059; and for the last 20° , between 192° and 212° , it was .046. In these experiments, the tube being wholly covered with boiling water, no condensation of vapour took place in the vacuum; and therefore no particles of quicksilver were seen adhering to the upper part of the tube. When the water boiled, the resistance of the vapour was greater than in the preceding set, and the total expansion less. These 2 results serve strongly to confirm each other: it is however the last that furnishes the data for constructing the table of equation depending on the heat of the quicksilver in the barometer.

Finding, from the comparison of these two sets of experiments with each other, that the maximum and rate of expansion seemed to vary with the length of the vacuum above the quicksilver, Dr. Blagden advised to try what might be the result, when the vacuum was much longer than in the common barometer. The 3d set of experiments of this class was therefore made with a tube somewhat narrower in the bore than the former, and whose vacuum was $14\frac{1}{2}$ inches in length, of which $11\frac{1}{2}$ reached above the top of the vessel. The mean of 3 observations gave .5443 for the total expansion on 180° ; that for the first 20° was .067; for the 20° in the middle of the scale .058; and for the uppermost 20° , it was .065: whence the mean rate for every 20° , is nearly .0605.* In this set, the condensation in the vacuum of the tube was particularly attended to: it began, as in those of the first set, immediately above the surface of the boiling water, which was always kept an inch or 2 above the top of the column: the lowermost globules were very small, increasing gradually till they got without the lid of the vessel, where they were the largest; thence they diminished uniformly upwards, and disappeared entirely 3 or 4 inches below the top of the tube. Though the rate for the middlemost 20° , in these last experiments, be below the mean, probably from some inaccuracy in observation; yet, being compared with the former sets, they still serve to corroborate each other: for in these with the long tube, the vacuum seems to have been either completely maintained, or nearly so; and we accordingly find the maximum of expansion increased, and its rate rendered nearly uniform.

In the course of the preceding experiments, from accidents of various kinds, it was often necessary to reboil the quicksilver; and in that operation many tubes were broken. The frequent removal of the sock from the bottom of the vessel, in order to get others ground for it, became at last very troublesome; and made

* Mr. Cavendish, who assisted in the first part of the experiments with the open tube, informed me, that, in those made by his father Lord Charles, the difference between the expansion of quicksilver and glass, from 180° of heat, was .469. If to this we add Mr. Smeaton's dilatation of glass, the total expansion of 30 inches of quicksilver will be .544, which agrees with the experiments in the long tube, and gives a rate of only .003022 for each degree.—Orig.

more caution necessary, in boiling such as were ground, especially in frosty weather, which happened to be the case in the last days of March, 1775: it was therefore thought best in the interim to try, what might be the expansion of a column of quicksilver, carefully put into the tube, but not boiled therein? With this view, the standard barometer and apparatus were left out during the night of the 29th, that they might acquire the same temperature, which was found next morning to be $34^{\circ}\frac{1}{2}$; the unboiled quicksilver standing $\frac{1}{100}$ of an inch higher than that which had been boiled. The lamps being applied to the vessel, the lengthening of the unboiled column was perceived, on the whole, to be more irregular, and the progressive diminution quicker, than in former experiments; so as to give, for the maximum of expansion, only .443 for 180° .

On the morning of the 31st, the unboiled column, which on the preceding day had been the highest, was lower than the other by near $\frac{2}{100}$ of an inch, the temperature of both being $31^{\circ}\frac{1}{2}$. As the water acquired heat from the application of the lamps, the rate of expansion diminished; and at boiling was only .405 for 180° . The operation of the 30th seems to point out, in a manner sufficiently conclusive, that the air contained in the unboiled quicksilver, rendered its specific gravity less than that which had been boiled even a great while before; since it required a longer column of the first, to counterbalance the weight of the atmosphere. And though the vacua might possibly, at the beginning, have been equally complete in both; yet they could not continue long so: for the air escaping gradually from the unboiled quicksilver, its elasticity increasing with the heat, and uniting with the quicksilver vapour, must have resisted the dilatation of the column, and rendered it less than on former occasions: which actually appeared from experiment. This is farther confirmed by the observations of the subsequent day; for now the unboiled column was become the shortest, owing no doubt to more air having ascended, and rendered the vacuum still more incomplete. Thus, the causes of resistance increasing, the dilatation is lessened in a superior degree.

Having found, from the first 2 sets of this class, the rate of expansion of a column of quicksilver, in the tube of a barometer of the ordinary length, to be progressive and not arithmetical; and that its maximum, for the 180° comprehended between freezing and boiling, was less than had been supposed; it was thought proper to try, by means of artificial cold, whether the condensation, for the 32° below freezing, followed nearly the same law? For this purpose the tin vessel, containing the ground tube, was rammed quite full of pounded ice and salt, as well as the tin stand holding the iron cistern below. In this operation, 12 pounds of ice and 4 pounds of salt were employed, by which the mean temperature of the mixture was reduced to $+4^{\circ}$ of Fahrenheit. But before the eyes of the vessel could be sufficiently freed from the composition, so as to

permit the surface of the column to be distinctly seen and read off, it had risen to $+14^{\circ}$; the temperature of the air, and also of the standard barometer, being at the same moment $49\frac{1}{2}^{\circ}$. The observed condensation, arising from this difference of $35\frac{1}{4}^{\circ}$, was $\frac{1.0}{10.0}$ ths of an inch; or .1189 when reduced for the height of the barometer, which then stood at 30.296. Hence the condensation for 32° is .1072, or .00335 for each degree. In this day's experiment, when the temperature of the mixture had risen to 32° , that of the air and standard barometer was $52\frac{1}{4}^{\circ}$; whence the reduced difference, for the 20° between 32° and 52° was found to be .0664 answerable to former experiments.

The same experiment was repeated 2 days after, with great care, the vessel being filled no higher than the surface of the quicksilver. The mean temperature of the mixture was now $+4^{\circ}$, and that of the standard barometer $49\frac{1}{4}^{\circ}$. The observed condensation, arising from this difference of $45\frac{1}{4}^{\circ}$, was $\frac{1.6}{10.0}$; or .1594 when reduced for the height of the barometer, then standing at 30.416: hence the rate for 32° is .1127, or .003522 for each degree. When the temperature of the mixture had risen to 32° , that of the air was 51° : whence the augmented rate for the 20° , between 32° and 52° , was found to be .0662.

From the mean of these 2 experiments it appears, that the condensation of a column of 30 inches of quicksilver in the barometer, affected by the 32° of cold below freezing, is .1099: and that the expansion from 20° of heat, between 32° and 52° , is .0663, a number agreeing perfectly well with former results. If the condensation .1099, thus found, be added to the expansion .5117 arising from the 2d class of experiments, we shall have .6216 for the total difference of height of the columns of quicksilver in 2 barometers, sustaining the same pressure, but differing from each other in their temperatures 212° of Fahrenheit's thermometer.

The series of numbers expressed in the annexed table, agreeing in all essential respects with the expansions found by experiment, will therefore show that which corresponds to any intermediate temperature, for every 10° of the scale.

Rate of expansion of a column of quicksilver in the tube of a barometer.

	Temp.	Expans.	Differ.	2d Differ.
Expansion above 32° of Fahrenheit; equal to be subtracted from the height of the column of quicksilver of 30 inches.	212	.5117		
	202	.4888	.0229	.0007
	192	.4652	.0236	
	182	.4409	.0243	
	172	.4159	.0250	
	162	.3902	.0257	
	152	.3638	.0264	.0006
	142	.3367	.0271	
	132	.3090	.0277	
	122	.2807	.0283	
Condensation below 32° of Fahrenheit; equal to be added.	112	.2518	.0289	.0005
	102	.2223	.0295	
	92	.1922	.0301	
	82	.1615	.0307	
	72	.1302	.0313	
	62	.0984	.0318	.0005
	52	.0661	.0323	
	42	.0333	.0328	
	32	.0000	.0333	
	22	.0338	.0338	
	12	.0681	.0343	
	2	.1029	.0348	
	0	.1099	.0070	

Construction and application of the table of Equation, for the Expansion of the Quicksilver in the tubes of Barometers.—In the application of the barometer to the measurement of heights, various modes of calculation have been adopted. The easiest and best method seems however to be, by means of the tables of common logarithms, which were first thought of by Mr. Mariotte, and afterwards applied by Dr. Halley, Mr. Bouguer, Mr. de Luc, and others. They have all proceeded on the supposition, that air is a truly homogeneous and elastic fluid, whose condensations being proportionable to the weights with which it is loaded, its dilatations are in the inverse ratio of the weights; and in consequence of this law, that the heights of the atmosphere ascended, are in geometrical progression, while the corresponding successive descents of the quicksilver in the tube of the barometer, are in arithmetical progression.

Mr. de Luc makes use of an arithmetical or uniform equation for the heat of the quicksilver in his barometer, by which their relative heights are reduced to what they would have been in the fixed temperature of $54\frac{1}{4}^{\circ}$ of Fahrenheit. In the formulæ adapting his rule to English measures, (vol. 13, pp. 520, 530) it has been shown, that the easiest and simplest method is, to make the difference of temperature of the two barometers the argument for the equation; and that it is sufficient to reduce either column to what would have been its height in the temperature of the other. But whatever may heretofore have been the method of using the equation for the heat of the quicksilver, while it was considered as arithmetical; now that it has been shown, from the preceding experiments, to be progressive, there seems at least to be propriety in applying to each barometer the equation answering to its particular temperature. And though, for this purpose, any fixed temperature might have been assumed at pleasure, as that to which both barometers were to be reduced; yet, the freezing point being fundamental in all thermometers, and that being also the zero of the scale for the equation depending on the heat of the air, as will be shown hereafter, it has been preferred to any other.

From the experiments it appears, that a column of quicksilver of the temperature of 32° , sustained, by the weight of the atmosphere, to the height of 30 inches in the barometer, when gradually affected by different degrees of heat, suffers a progressive expansion; and that, having acquired the heat of boiling water, it is lengthened $\frac{5.117}{10000}$ parts of an inch: also, that the same column, suffering a condensation by 32° of cold, extending to the zero of Fahrenheit, is shortened $\frac{1.099}{10000}$ parts, the weight of the atmosphere remaining in both cases unaltered. But in the application of the barometer to the measurement of altitudes, since the pressure and length of the column change with every alteration of the vertical height, the equation, depending on the difference of temperature of the quicksilver, will necessarily augment or diminish by a proportionable part

of the whole. Thus, if the weight of the atmosphere should at any time be so great as to sustain 31 inches of quicksilver, the equation for difference of temperature will be just $\frac{1}{30}$ part more than that for 30 inches; at 25 inches it will be $\frac{5}{6}$; at 20 inches $\frac{2}{3}$; at 15 inches $\frac{1}{2}$; and at 10 inches only $\frac{1}{3}$ of that deduced from experiments; and so on for any other quantity.

SECT. 2. *Experiments on the Expansion of Air in the Manometer.*—The thermometer used in these experiments was above 4 feet long. Its scale extended from -4° to $+224^{\circ}$ of Fahrenheit, each degree being more than $\frac{2}{10}$ of an inch: when the barometer stood at 30 inches, its boiling point was fixed in the tin vessel formerly described. Mr. Ramsden's thermometers generally rise in the same vessel $213\frac{1}{2}^{\circ}$; and the long thermometer, being placed in the vessel he makes use of to fix his boiling points, rises only to 210° .

The manometers were of various lengths, from 4 to upwards of 8 feet: they consisted of straight tubes, whose bores were commonly from $\frac{1}{15}$ to $\frac{1}{25}$ of an inch in diameter. The capacity of the tube was carefully measured, by making a column of quicksilver, about 3 or 4 inches in length, move along it from one end to the other. These spaces were severally marked, with a fine edged file, on the tubes; and transferred from them to long slips of paste-board, for the subsequent construction of the scales respectively belonging to each. The bulb, attached to one end of the manometer at the glass-house, was of the form of a pear, whose point being occasionally opened, dry or moist air could be readily admitted, and the bulb sealed again, without any sensible alteration in its capacity.

The air was confined by means of a column of quicksilver, long or short, and with the bulb downwards or upwards, according to the nature of the proposed experiment. Here it must be observed that, from the adhesion of the quicksilver to the tube, the instrument will not act truly, except it be in a vertical position; and even then it is necessary to give it a small degree of motion, to bring the quicksilver into its true place; where it will remain in equilibrio, between the exterior pressure of the atmosphere on one side, and the interior elastic force of the confined air on the other. All the experiments were made when the barometer was at, or near, 30 inches. When the bulb was downwards, the height of the barometer at the time of observation augmented, and when upwards, diminished by the number of inches of quicksilver in the tube of the manometer, expressed the density of the confined air. Pounded ice and water were used to fix a freezing point on the tube; and by means of salt and ice, the air was further condensed, generally 4, and sometimes 5 or 6 degrees below zero. The thermometer and manometer were then placed in the tin vessel, among water which was brought into violent ebullition; where having remained a sufficient time, and motion being given to the manometer, a boiling point was marked on it. After this the fire was removed, and the gradual descents of the piece of

quicksilver, corresponding to every 20 degrees of change of temperature in the thermometer, were successively marked on a deal rod applied to the manometer. It is to be observed, that both instruments, while in the water, were in circumstances perfectly similar; that is, the ball and bulb were at the bottom of the vessel.

It is easy to conceive, in experiments of this very delicate nature, part of which, namely, those on air less dense than the atmosphere, were extremely difficult and even laborious, that mathematical exactness was not to be looked for; and that, notwithstanding every possible precaution was taken, irregularities would occur. These however were not so numerous as might have been expected, nor any way so great as to render the research fruitless: for a few of that kind being thrown out of the total number, the mean of the others, which were very consistent among themselves, served to prove beyond the possibility of doubt, that the expansions of common air did not keep pace with the dilatations of quicksilver. The manometrical space, answering to the 20° of the thermometer between 52° and 72°, was always found to be greater than any other 20° of the scale. Here it is to be understood, that I do not pretend to have ascertained the exact point

	Spaces of the quicksilver therm. Fahren- heit's scale.	Spaces of the ma- nometer, mea- sured in degrees of Fahrenheit.	Diff.
	212°	212°	
	192	194.4	17.6*
	172	176.2	18.2
	152	157.4	18.8
	132	138.0	19.4
	112	118.0	20.0
	92	97.2	20.8
	72	75.6	21.6
	52	53.0	22.6
	32	31.4	21.6
	12	11.4	20.0
	0	0	11.4

Experiments for determining the Actual Expansion of Common Air, in the Manometer, affected by the heat of 212°.—For this purpose it became necessary to ascertain, in every manometer, the exact proportion between the capacity of the tube and that of its bulb. This was done, by weighing the quicksilver that filled them respectively, in a balance that was sensible to a very small fraction of a grain. The contents of the bulb, and that part of the tube between it and

zero, expressed in grains, was called the air in experiment. The apparent expansion of that air was measured by the grains that filled the several sections of the tube between zero and the boiling point; the sum being the total expansion or increase of volume, from a heat of 212° . The apparent expansion, thus found, was again augmented for the dilatation of the tube, on the following principles.

In the first part of this paper I have shown, that solid glass rods dilate much less than barometer tubes. The mean between Mr. Smeaton's and my experiments, gives $\frac{1}{10000}$ of an inch for the longitudinal extension of every foot of these tubes, by 212° . From the rate of going of a clock, for near a year, whose pendulum rod is solid glass, its dilatation seems to be $\frac{1}{3}$ part of a steel rod, or $\frac{5}{10000}$ on a foot, by 212° . Now as the manometers resemble solid rods much more than they do barometer tubes, it is probable that their dilatation, even allowing for the greater extension of the bulb, would not exceed $\frac{6}{10000}$ of an inch on a foot, or $\frac{1}{1000}$ part on every 2 inches. In this ratio I have therefore augmented the apparent, to obtain the true, capacity of each manometer. The equation, amounting to about $\frac{1}{2120}$ part of the whole, being less than the common error of such complicated observations, might in fact have been entirely omitted, without producing any material alteration in the results.

Having in this manner computed the total increased volume of any number of equal parts of air, according to the capacity of the bulb and tube in grains, and very often likewise the partial expansions for intermediate temperatures, expressed by the contents of the corresponding sections of the tube, I then found the ratio answering to 1000 equal parts, which, being divided by the degrees of difference of temperature, gave the mean rate for the whole scale, or the particular rate for any intermediate section of it.

The experiments, considered in this way, are distributed into 4 classes, of which the results are comprehended in 4 tables. The first shows the expansion of air, whose density was much greater than that of the common atmosphere. The 2d, which was divided into 2 sets, contains those on air that sustained a pressure less than the atmosphere. In the 3d class, a very short column of quicksilver being employed to confine the air, its density differed little from that we commonly breathe in: this class was likewise subdivided into 2 sets. The 4th and last class of experiments were made on air of the common density, artificially moistened by the admission, sometimes of steam, and at others of water, into the bulb; it is accordingly distinguished into 2 sets.

From the experiments of the 1st class it appears, that 1000 equal parts of common air, loaded with $2\frac{1}{2}$ atmospheres, being affected with a heat of 212° , expands 434 of those parts; that is, in its dilated state, it occupies a space bearing, to that which it originally filled, the proportion of 1434 to 1000: hence the

mean rate of expansion of air of that extraordinary density is 2.04717 for each degree.

From the 1st set of the 2d class of experiments it appears, that 1000 equal parts of air, pressed only with $\frac{5}{8}$ of an atmosphere, and suffering a heat of 212° , expands nearly 484 of those parts, of which the mean rate for each degree is 2.28140. The maximum corresponds to that section of the scale between 52° and 72° ; and the rate for the extremes is less than the mean.

But in the 2d set of this class, when the confined air was rendered so extremely rare as to be pressed with only $\frac{1}{8}$ of an atmosphere, in which case there was a necessity for heating it red-hot before it was possible to make the quicksilver hang in any tube of a moderate length, the expansion of 1000 equal parts of air was, by the 7th and 8th experiments, diminished to about $\frac{2}{3}$ of the usual quantity; and by the 9th, it was considerably less, amounting only to 141.5 for the 180° comprehended between freezing and boiling, or 0.78613 for each degree. The maximum still corresponds to the space between 52° and 72° ; and the minimum is constantly at the boiling point.

From these last 3 experiments it would seem, that the particles of air may be so far removed from each other, by the diminution of pressure, as to lose a very great part of their elastic force; since, in the 19th experiment, the heat of boiling water applied for an hour together, could only make it occupy a space which, compared with what it filled at freezing, bears the proportion of 141.5 to 1000.

From the 3d class of experiments it appears, that common air, pressed with a single atmosphere, whether taken into the manometer in its natural state, or heated red-hot in it, has the same expansion with air of only $\frac{5}{8}$ of that density: for 1000 equal parts of this air expanded 484.21 from 212° of heat, of which the mean rate is 2.28401 for each degree. By comparing this result with that of the 1st class, and again with that deduced from the 2d set of the 2d class, it would seem that the elastic force of common air, is greater than when its density is considerably augmented or diminished by an addition to, or subtraction from, the weight with which it is loaded; for, in the first case, it bears the proportion of 484 to 434; and in the last it is, from the mean of 3 experiments, as 484 to 252, when pressed with only $\frac{1}{8}$ of an atmosphere.

As barometrical observations will probably never be made in a temperature higher than 92° in the shade, nor in one lower than 12° , if we subtract 26.038, the expansion answering to 12° , from 222.006, that which corresponds to 92° , we have 195.968 for the 80 intermediate degrees; or 2.45 for the mean rate on each. This equation, compared with Mr. de Luc's, bears the proportion of 245 to 210, which is a difference of $\frac{35}{1000}$ on every degree, or a 7th part of the whole: and though this rate will be found hereafter to exceed that deduced from the operations of the barometer in extreme temperatures; yet they agree

exceedingly well with each other for the mean heat of the air, when the barometer will come most frequently into use.

The experiments in the 4th class are all that now remain to be mentioned. The bare inspection of them show how greatly superior the elastic force of moist is to that of dry air. It is true indeed that 2 kinds of irregularities present themselves among the results: 1st, with regard to the total expansion for 212° : and 2dly, as to the greatest exertion of the elastic force, which sometimes seems to have taken place before the air has acquired the heat of boiling water. The 1st is easily accounted for: it must have arisen from different proportions of moisture being admitted into the same quantity of air, which there was no possibility of ascertaining, the bulbs and their apertures being of very different dimensions. With regard to the 2d irregularity, it may have proceeded from error of observation, it being difficult to determine the accurate temperature near boiling; especially when any part of the air rose above the top of the vessel, which was sometimes the case, notwithstanding its extraordinary height. Be that as it may, a very uniform increasing progression is perceived to take place, from the zero of Fahrenheit, as far as 152° or 172° ; and even to the boiling point, in those esteemed the best experiments. By adhering to the mean result it will appear that air, however moist, having that moisture condensed or separated from it by cold, its expansion differs not sensibly from that of dry air. Thus the rate for 32° below freezing 2.22799, is nearly the same as in dry air; but no sooner does the moisture begin to dissolve and mix with the air, by the addition of 20° of heat, than the difference is perceptible: for instead of 2.46675, the rate for 20° above 32° in dry air, we have 2.588 for that which is moist. In the next step of 20° , the rate for dry air is 2.5809; whereas that for moist is 2.97. In this manner the progression goes on continually increasing, so as to give 7.86854 for the mean rate on each degree of the 212° , which is near $3\frac{1}{2}$ times the expansion of dry air. And lastly, the rate for the 20° between 192° and 212° , is $2\frac{1}{2}$ the mean rate, and about 9 times that which corresponds to the zero of the scale: but if the comparison be drawn from the mean of the 5th, 6th, and 9th experiments, as being probably nearest the truth, the total expansion of moist, will be more than 4 times that of dry air; and the rate for the temperature at boiling, will be nearly 15 times that which corresponds to the zero of Fahrenheit.

SECT. 3. *An Account of the Barometrical Observations made in Britain.*—I am now to give an account of the principal barometrical observations that have been made in Britain, on heights determined geometrically with great care. These heights are classed in the following list in 6 sets, according to the districts of the country where they are situated, and nearly in the order of time in which the observations were made.

N ^o 1. Heights in and near London,	Height in feet.
St. Paul's Church-yard, north side, and iron gallery over the dome	281
Top of Paul's stairs, and the said gallery.....	324
Top of Scotland-yard wharf, and the dining-room of the Spaniard on Hampstead-heath	422
Great Pulteney-street, and the said dining-room	352
Pagoda in Kew gardens	416 $\frac{1}{2}$
Gun wharf in Woolwich Warren, and uppermost story of Shooter's-hill inn.....	444

N^o 2. Near Taybridge in Perthshire.

Station at the east gate of Castle Menzie's gardens near the village of Weem, and top of Weem Craig	700 $\frac{1}{2}$
The said station, and top of Bolfrack's Cairn	1076 $\frac{1}{2}$
The said station, and top of Dull Craig	1244 $\frac{1}{2}$
The said station, and top of Knock Farle.....	1364 $\frac{1}{2}$
The said station, and that at the rivulet of Glenmore, below the south observatory on Schihallien	1279 $\frac{1}{2}$
The said station, and south observatory.....	2098
The said station, and western summit of Schihallien	3281
Station at the rivulet of Glenmore, and the south observatory.....	818 $\frac{3}{4}$

N^o 3. Near Lanark.

Level of the Clyde at Lanark-bridge, and station in the garden at Lanark.....	362 $\frac{1}{2}$
Ditto level, and top of Stonebyre-hill	654
Robin Hood's well, before Carmichael-house, and top of Tinto, 4 feet below the summit of the Cairn	1642 $\frac{1}{2}$
Ditto well, and west end of Carmichael-hill	451 $\frac{1}{2}$

N^o 4. Near Edinburgh.

Leith pier-head, and top of the Calton-hill	344
Leith pier, and summit of Arthur's Seat	803
Leith pier, and Kirk-yetton Cairn, on the east end of the Pentland-hills	1544
Calton-hill, and ditto Cairn	1200
Level of Hawk-hill study, and top of Arthur's Seat	702 $\frac{1}{2}$
Hawk-hill observatory, and bottom of the little rock on Arthur's Seat, 7 $\frac{1}{4}$ feet below the summit	684
Hawk-hill garden door, and ditto little rock.....	730 $\frac{3}{4}$

N^o 5. Near Linhouse.

Linhouse, and East Cairn-hill, 5 feet below the summit	1176 $\frac{1}{2}$
Ditto, 18 feet below the top.....	1165 $\frac{1}{2}$
Linhouse, and West Cairn-hill, 11 feet below the top	1178 $\frac{1}{2}$
Ditto, and Corstown-hill, 4 feet below the top	386 $\frac{1}{2}$
Corstown-hill, and West Cairn-hill	792
Ditto, and East Cairn-hill	776 $\frac{1}{2}$

N^o 6. Near Carnarvon in North Wales.

Carnarvon Quay, and Snowdon Peak	3555
Ditto, and summit of Moel Eilio.....	2371

To enter into a minute detail of the geometrical operations, by which the whole of these vertical heights were determined, would be extremely tedious and uninteresting. That some idea may however be formed of the degree of accuracy with which they were ascertained, it will be sufficient to observe, that the requisite angles were taken with an astronomical quadrant of a foot radius, made by Sisson, and curiously adapted for the measurement of horizontal or base angles; which, as well as those of the vertical kind, might always be thus determined to within 10 seconds of the truth. The bases were measured with care; and, in order to ascertain the distances, the 3 angles of each triangle were, as often as possible, actually observed with the quadrant. That the variation of the

line of collimation of the instrument, which was found to alter in carrying, might occasion no error, one or more of the angles of elevation, at each station, were taken on the arc of excess, as well as on the quadrantal arc. In all cases, the usual* allowances were made for curvature and refraction: and for the correction of the last, sometimes the angles of depression as well as of elevation were taken. When time would permit, the geometrical operations were repeated at the first stations; or the angles of elevation were observed from some new point connected with the first, and whose relative height, with respect to the others, was known. Small altitudes were occasionally determined by levelling from one station to the other.

I now proceed to give some account of the barometrical observations. The heights in and near London being so very inconsiderable, it was easily foreseen, that nothing conclusive could be drawn from observations made on them alone. It was however natural enough to try, even on these, whether the rule we had been furnished with would answer? A small height of 41 feet 4 inches, which, without inconveniency, could be recurred to at all times of the day, and all seasons of the year, was the first that was made use of. St. Paul's, Hampstead, Kew pagoda, and Shooter's-hill, were the next. The mean results of many observations on the first 3, and of several on Shooter's-hill, were found to be defective. In general the coldest observations, made in the morning and evening, when the temperatures at the 2 stations differed least from each other, answered best. In the hottest part of the day, when that difference was the greatest, the results were most defective. Some months spent in Scotland in the summer of 1774, afforded opportunities of making barometrical observations on hills of various heights, from 3 or 400 to upwards of 3000 feet, as exhibited in the preceding list. That season was remarkably cold and wet; therefore, in these observations, the mean temperature of the air in the shade was commonly about 55° . The hottest never exceeded 63° in the plain; and the coldest, namely those on the highest mountains, were generally from 43° to 48° .

From the defect found in the results of these observations, which, with respect to temperature, correspond to the mean and hottest of those made at sunrise on Saleve, and without any exception whatever, I could easily discover, either that a much greater equation than what the rule directed, must be applied for each degree of heat above the zero of the scale; or, that the zero itself would fall considerably lower than $39^{\circ}74$, where Mr. de Luc's formula, adapted

* If the square of the distance be divided by the diameter of the earth, the quotient will give the curvature of the globe on that distance, or the excess of the apparent above the true level: and, by Mr. Maskelyne's rule, the square of the distance being divided by the diameter of the earth, augmented by $\frac{1}{4}$ part, we have the allowance for curvature and refraction; which last is supposed to raise the object, by an angle equal to that of a great circle subtended by $\frac{1}{16}$ part of the distance.—Orig.

to English measures, has fixed it. This first step towards a correction of the rule, naturally pointed out the 2d thing to be aimed at, namely, the obtaining of a sufficient number of cold observations, near the zero, and as far as possible below it, that the equation might disappear entirely, and even come to be applied with the contrary sign. Of this kind the winter seasons of 1774 and 1775 afforded a few on the small heights in and near the metropolis; but the best that have been furnished, are those which Dr. Lind, assisted by Mr. Hoy, made on Arthur's Seat near Edinburgh; and those which Capt. Calderwood made on the Cairn-hills, being a part of the Pentland range to the south-west of that city.

By comparing these sets of observations together, it appeared from all of them, that when the air was at or near the freezing temperature, the logarithmic differences gave the real height, in English fathoms and thousandth parts, without any equation; and when considerably below that point, the equation was to be subtracted, or applied with the sign $-$ instead of $+$. It was further perceived, that the same general conclusion might be drawn from the coldest, not only of the sun-rising, but even on the ordinary observations on Saleve, some reduction of the temperature being in certain cases made, on account of the exposure of the thermometer to the sun's rays: hence I was led to suppose, that the morning observations, instead of being made exceptions from the rule, were those which it might be presumed would form the best basis for deducing the equation depending on the heat of the air; because the mean temperature of the column was then found to differ least from that of its extremities; whereas in the hottest time of the day, that difference was generally the greatest. Having been enabled, by means of the cold observations, to form some judgment whereabouts the zero of the scale would fall, below which the equation was negative, and above it affirmative; it followed of course, that the next principal thing to be sought for, was the maximum of equation, or that corresponding to the highest temperatures the climate of our island would afford.

By comparing the tables of barometrical results, it will be found, that the observations for extreme temperatures belong to the Edinburgh class of observations (N^o 4) it being thought best in this case to omit the few hot ones obtained on the inconsiderable heights near London: the mean of the coldest, answering to the temperature of $21^{\circ}.75$, make the logarithmic excess $\frac{2.9}{1000}$; and the mean of the hottest, corresponding to the temperature of $69^{\circ}.6$, give a defect of $\frac{8.1}{1000}$. Now the sum of the two equations $\frac{1.1}{1000}$, being divided by the difference of temperature $47^{\circ}.85$, we have nearly 2.3 for the mean rate of the equation on each degree, which is less than that resulting from the operations of the manometer. Again, from the mean of the very best observations, as being made on the greatest heights, when the temperature of the air is 52° , it appears, that the defect is from $\frac{4.9}{1000}$ to $\frac{5.0}{1000}$, or 2.5 for each degree nearly, which agrees perfectly well

with the manometrical expansion. In this case, the ratio of the weight of quicksilver to air is as 11377 to 1; greater very considerably than 11232 to 1, assigned to them by Mr. De Luc, when the temperature is $69^{\circ}.32$, answering to the zero of his scale, without any allowance for the diminution of pressure on his columns, which should have rendered air still comparatively lighter. From the British observations, made on the most considerable heights, it appears, that when the temperature of the air is $28^{\circ}.2$, the ratio of its weight, with respect to that of quicksilver, is as 1 to 10552: hence the increase of the weight of air, on every degree of difference of temperature between $28^{\circ}.2$ and $52^{\circ}.5$, amounts to 34.4; and hence we have $52^{\circ}.5 - 4^{\circ}.2 = 48^{\circ}.3$ for the temperature of the air in Britain; when its weight would be $\frac{1}{11232}$ of that of quicksilver; and consequently agree with Mr. De Luc's, though the heat would differ from his 21° . It will no doubt be remarked, that the equation for the air, resulting from the operations of the barometer, falls short of that given by the manometer. Part of the difference may arise from the small number of barometrical observations obtained in extreme temperatures. I shall however adduce reasons hereafter for supposing that it really should diminish, because of the drier and less elastic state of the superior air, compared with that taken into the manometer at the earth's surface. In the mean time, since both instruments agree in the equation for 52° which is a heat that the barometer will very frequently be used in, it seems best to adhere to the mean manometrical result 2.45, in fixing the zero of the scale, which is obtained in the following manner.

Divide the excess or defect, expressed in 1000th parts of the logarithmic result, by 2.45, the mean expansion of air for each degree of the thermometer; the quotient will give the number of degrees, in the first case to be added to, and in the last subtracted from, the temperature of the air in the observation; the sum or difference answers to the zero of the scale, or that temperature when the logarithmic result gives the real height in English fathoms and 1000th parts.

According to this mode of computation, we have, from the aggregate of the several classes of British observations, the place of the zero as follows:

By the first class of observations in and near London	} $25^{\circ}.5$ and $71^{\circ}.2$ at $32^{\circ}.2$	
between the temperatures of		
2d, near Taybridge.	46.1	— 62.9 — 31.1
3d, near Lanark.	44.	— 62. — 32.8
4th, near Edinburgh	17.	— 70.7 — 31.3
5th, near Linhouse.	26.1	— 46.5 — 29.9
6th, near Carnarvon.	49.1	— 62.3 — 32.9
Mean place of the zero at		31.7

The number $31^{\circ}.7$ differing so very little from 32° , we may hereafter consider that remarkable point of Fahrenheit's thermometer, as the zero of the scale depending on the temperature of the air; and hence is deduced the 2d part of

the rule for measuring heights with the barometer. When the mean temperature of the column of air to be measured is at 32° of Fahrenheit, the difference of the common logarithms of the equated heights of quicksilver in the inferior and superior barometers, expressed in 1000th parts of an inch, gives the real height in fathoms and 1000th parts, the 3 figures towards the right hand being decimals, and the rest integers; which, being multiplied by 6, gives the result in feet.

Let us next consider, in a general way, how far this will correspond with Mr. De Luc's observations in extreme temperatures. It has already been remarked, that when the temperature of the air was at $69^{\circ}.32$, as indicated by thermometers exposed to the sun's rays, Mr. De Luc found that the differences of the common logarithms of the heights of the barometers at the two stations, gave the altitude between them, in French toises and 1000th parts: in which case the specific gravity of quicksilver to air was as 11232 to 1. When his formula is adapted to English measures, the zero of the scale necessarily descends to 39.74, where the English fathom bears the same proportion to the modulus of the common logarithms, as, in the former case, the French toise did to that modulus, the equation for the intermediate temperature being now applied with the contrary sign. As it has been shown, that the British observations differ in their circumstances from those on Saleve, and require a greater equation, it is unnecessary to enter into any minute comparison of the two sets: yet, that some idea may be formed of the cause, of part at least, of the difference that takes place between them, I have collected into one view, the computations of such as were made in extreme temperatures; namely, the coldest of those at sun-rising; the coldest and hottest of the ordinary observations; also those on the Dole, at Genoa, and at Turin, by which the heights of the lake of Geneva and of Turin, above the sea at Genoa, were obtained.

From the table it appears, that when the temperature of the air is at $29^{\circ}.5$, the logarithmic excess is $\frac{1.0.0.0}{1.0.0.0}$; and at $75^{\circ}.5$ reduced temperature, the defect is $\frac{1.0.6.0}{1.0.6.0}$. The sum of the two equations $\frac{1.0.5.0}{1.0.5.0}$ being divided by the difference of temperature 46° , we have, as in the British observations, nearly 2.3 for each degree, which is greater than that applied by Mr. De Luc's rule, in the proportion of 23 to 21. That too small an equation has been used in these hottest observations, supposing the original zero and temperature to remain, is sufficiently evident: for $\frac{1.0.6.0}{1.0.6.0}$ being divided by 42° the difference of temperature, we have, as before, 2.3 very nearly for the equation answering to each degree. Also, if we consider the ratio of the weight of quicksilver to air, actually resulting from the observations themselves, the same kind of error still exists. Now if from the aggregate of these observations, the same method be adopted, as was used in the British, for finding the zero of the scale, we shall have it as follows:

By Mr. De Luc's equation for the air and observed temperature.	By the manometrical equation and reduced temperature.
Coldest of the morning observations, from $25^{\circ}.2$ to $30^{\circ}.5$ at $33^{\circ}.7$	from $25^{\circ}.2$ to $30^{\circ}.5$ at $33^{\circ}.12$
Coldest of the ordinary observations. $27^{\circ}.1..41^{\circ}.9..38^{\circ}.7$ $26^{\circ}...35^{\circ}...32^{\circ}.97$
Hottest of the ordinary observations. $76^{\circ}...84^{\circ}.5..36^{\circ}.2$ $73^{\circ}.5..77^{\circ}...36^{\circ}.32$
On the Dole $59^{\circ}.2..71^{\circ}.5..27^{\circ}.6$ $58^{\circ}...70^{\circ}...32^{\circ}$
Light house of Genoa $75^{\circ}...79^{\circ}...26^{\circ}$ $75^{\circ}...79^{\circ}...33^{\circ}.40$
De La Caille's $58^{\circ}...30^{\circ}$ $58^{\circ}...33^{\circ}.35$
Zero at..... $32^{\circ}.03$	Zero at.... $33^{\circ}.52$

From the mean of these observations, though the results are irregular among themselves, it appears sufficiently evident, that if the morning observations on Saleve had been retained, instead of being made exceptions from the rule, the zero of the scale would have descended about 8° ; viz. from $69^{\circ}.3$ to $61^{\circ}.4$ of Fahrenheit, supposing always the equation 2.1 for each degree of temperature, and the French toise, as the standard measure, to have been adhered to; for the French toise bears to the English fathom, the proportion of 106575 to 100000; therefore $\frac{6.575}{100000} = \frac{6.1}{2.1} = 29^{\circ}.4 + 32^{\circ} = 69^{\circ}.4$, denotes the relative positions of the 2 zeros, the intermediate equation $\frac{6.1}{100000}$ being to be subtracted when the toise is used. But it has been shown, that the mean expansion of air is really greater, for such temperatures at least as the barometer can be applied in, than what Mr. De Luc supposed it, in the proportion of 245 to 210; hence it follows, that $\frac{6.1}{2.45} = 25^{\circ}.18 + 32^{\circ} = 57^{\circ}.18$. will denote the relative positions of the two zeros; which, instead of almost 30° , are only distant from each other a little more than 25° .

From what has been said it is easy to see, that in calculating heights according to Mr. De Luc's rule, when the temperature of the air is below his zero, which we may take at 40° , the English measure being used, the common error in the result will be equal to the sum of the two equations $2.1 + 2.45 = 4.55$ for each degree; which amounts to $\frac{3.6}{100000}$ parts for the 8° that the zero is too high. Above 40° , the former error $\frac{3.6}{100000}$ will be augmented by the difference of the equations for each degree that the temperature is above his zero, viz. $2.45 - 2.1 = \frac{0.35}{100000}$. It may be proper now to compare these observations, with others that have been made towards the pole and at the equator: from which it will appear probable, that the rule which answers in middle latitudes, will not in the frigid and torrid zones. In 1773, Captain Phipps, now Lord Mulgrave, commanding two of his Majesty's ships then sent on discoveries towards the North Pole, measured geometrically, with great care, the height of a mountain in Hakluyt's island near Spitzbergen, and found it to be 1503 feet above the level of the sea. On the morning of the 18th of August, the barometrical observations, at the sea-shore and top of the mountain, when reduced and calculated, gave 1577 for the same height, being $\frac{4.7}{100000}$ too much. Hence it appears that, instead of the usual

equation $\frac{2.4.5}{1000}$, to be added to the logarithmic result, to obtain the true height in Britain when the temperature is 42° , there is an access of $\frac{4.7}{1000}$: and, instead of the usual ratio of the weight of quicksilver to columns of air, of equal altitude and temperature in Britain, namely about 11200, we have that of 10224 to 1. Thus air at Spitzbergen seems to be specifically heavier than that affected with the same heat and pressure in the middle latitude: whence it follows that, instead of 32° which is found to be the zero of the scale about the middle of the temperate zone, we shall have $\frac{4.7}{2.4.5} = 19^{\circ}.2 + 42^{\circ} = 61^{\circ}.2$ for the zero at Spitzbergen, within 10° of the North Pole. By attending diligently to what Mr. Bouguer has told us of the steadiness of the barometer throughout the year in Peru; the uniformity of the mean temperature in every assigned station; and his mode of computing, by means of the tables of common logarithms, the altitudes of the Cordillero mountains above the valley that extends itself between them; it will be no difficult matter to discover, nearly at least, what sort of equation became necessary; and what were the relative weights of quicksilver and air of the mean temperature, not only in that high region of the atmosphere, but also at the level of the sea. Thus, by inspecting the table of computations, it will appear, that columns of air, whose bases were removed 6 or 8 thousand feet from the level of the sea, and whose heights equalled that distance, when the temperature was 55° of Fahrenheit, as determined from the mean between the coldest of the morning and hottest of the afternoon, the mean logarithmic defect was only $\frac{3.6.3}{1000}$: whereas, in measuring heights near the level of the sea, in middle latitudes, the common equation for that temperature is $\frac{5.7}{1000}$. The mean ratio of the weight of quicksilver to air, on these long columns comprehended respectively between Carabouro and Quitou, and the summits of Pichincha and Coraçon, is that of 16793 to 1. On the altitude of 1534 feet, intercepted between Carabouro and Quito, which short section of the column is about half-way between the level of the sea, and the summits of the Cordilleros, the mean temperature being 66° , the ratio is that of 15089 to 1: hence it seems probable, that quicksilver would have to the different sections of the general column of air, comprehended between the top of Coraçon, nearly the following ratios:

	Temp.	
At the level of the South Sea.....	$84\frac{1}{2}$	13100 to 1
Half-way from thence to Carabouro.....	$75\frac{1}{2}$	14100
At Carabouro.....	$66\frac{1}{2}$	15100
Half-way from thence to Coraçon.....	55.....	16100
At the summit of Coraçon.....	$43\frac{1}{2}$	17100
The mean of which is.....	65.....	15100

Mr. Bouguer tells us, that the barometer in the torrid zone varies not at the sea shore above $2\frac{1}{2}$ or at most 3 lines throughout the whole year. At Popayan,

its variation is only a line and a half; and at Quito a single line. Now let us suppose, that an altitude had been measured with the barometer at the level of the South Sea, where the descent of quicksilver at the upper station was exactly an inch in the mean heat of the day, answering to $84^{\circ}\frac{1}{2}$. On the former supposition of the weight of quicksilver to air, the height would be 13100 inches or 1091.7 feet.

$$\text{Hence } \left\{ \begin{array}{l} 29.930 \quad 84\frac{1}{2} - 169 = 29.761 \\ 28.930 \quad 84\frac{1}{2} - 169 = 28.761 \end{array} \right\} = 890.6 \text{ feet,}$$

the logarithmic result, which is defective 201.1, or nearly $\frac{201.1}{1000}$ parts. Now this equation being divided between 2.45 the mean expansion of air, we have nearly 92° for the difference between $84\frac{1}{2}$, the temperature of the observation, and the zero of the scale, which reduces it to $-7^{\circ}\frac{1}{2}$ of Fahrenheit.

Having now mentioned all the barometrical observations that have come to my knowledge, tending any way to throw light on this very intricate subject, it remains to sum up, from the whole, the general principles on which I have proceeded in constructing the table of equation for the heat of the air. It will be remembered, that I have more than once remarked, that in the British observations, when the temperature was 52° , the defect was $\frac{4.9 \text{ or } 5.0}{1000}$, the lowermost barometer standing at or near the level of the sea; but in the observations on Tinto, a considerable hill appertaining to the third class, whose base is elevated 700 feet above the level of the Clyde at Glasgow, when the temperature was 52° , I found the equation to be little more than $\frac{4.5}{1000}$. Again, these two facts being compared with the aggregate result of Mr. De Luc's observations, where the lowermost barometer stood about 1300 feet above the sea, the equation for the same temperature seemed not to exceed $\frac{4.2}{1000}$. Lastly, these circumstances being confronted with the results of Mr. Bouguer's observations, where the lowermost barometer stood from 6000 to 8000 feet above the sea, the mean equation for 55° was only $\frac{3.6 \cdot 3}{1000}$, which gives $\frac{3.4}{1000}$ for the heat of 52° . Now these Peruvian observations, which I believe to be exceedingly good, from the steadiness of the barometer in that part of the world, being substituted instead of those not yet obtained in our own quarter of the globe, there seemed to be a necessity for concluding, that the equation for middle latitudes, with any assigned temperature above or below the zero of the scale, diminished as the height of the place above the sea increased; which consequently implied, that the magnitude of the logarithmic terms increased faster than the dilatations of the air. But when the comparison was carried yet further, and the observations in Peru and at Spitzbergen were fairly brought into one view, there appeared to be sufficient grounds for suspecting, if not absolutely for concluding, that there could be no fixed zero for the scale depending on the temperature of the air; but that it would change with the density of the atmosphere appertaining to the latitudes,

climates, or zones of the earth, where the observations were made. On this supposition it was natural for the mind to form to itself some general hypothesis, which might serve to account for the appearances; and the first that presented itself was the following: that the atmosphere surrounding our globe might possibly be composed of particles, whose specific gravities were really different; that the lightest were placed at the equator; and that the density of the others gradually increased from thence towards the poles, where the heaviest of all had their position.

It is a well known and established fact, that in the middle latitudes, a north or north-east wind constantly raises the barometer, and generally higher as its continuance is longer. The contrary happens when a south or south-west wind blows; for it is commonly lowest when the duration and strength of the wind from that quarter have been the greatest. Thus the north-east wind, by blowing for any length of time, brings into the middle latitudes a mass of air heavier than that which naturally appertains to the region, and raises the barometer above its mean height. The continuance of a south-wester carries off the heavy air, deposits a much lighter body in its stead, and never fails to sink the barometer below its mean height: hence, in the middle parts of Europe, there is a difference of about $2\frac{1}{4}$ inches between the highest and lowest states of the barometer. But supposing it to be only 2 inches, the difference of pressure still amounts to $\frac{1}{15}$ part of the whole weight of the atmosphere. Now it is evident from the Peruvian observation, that the greatest fluctuation of the barometer, which is at the level of the sea, does not exceed 0.226 of an inch, or $\frac{1}{133}$ part of the whole pressure; and if the change should be no greater at the poles, which is not improbable, it follows, that the measurement of heights by means of the barometer, in middle latitudes, will be more precarious and uncertain than in the torrid and frigid zones.

Such in general were the first ideas which the comparison of the operations of the barometer with the effects of the north-east and south-west wind* on that

* I have been well informed, that in China the north-west wind raises the barometer most, and is highly electrical; it is at the same time the driest and the coldest; and at Canton, under the northern tropic, there is frequently ice. On the east coasts of North America the severity of the north-west wind is universally remarked; and there can scarcely be a doubt, that the inhabitants of California, and other parts on the west-side of that great continent, will, like those on the west of Europe, feel the strong effects of a north-east wind. The extraordinary dryness and density of the wind from the north pole, seems therefore to be occasioned by its passing over the continent of Europe and Asia on one side, and that of North America on the other. Those who live on the east and west coasts of South America, will find the driest and coldest winds come to them respectively from the south-west and south-east. As the winds seem to be colder, drier, and denser, in proportion to the extent of land they pass over from the poles towards the equator, so they appear to be more moist, warm, and light, in proportion to the extent of ocean they pass over from the equator towards the poles. Hence the humidity, warmth, and lightness, of the Atlantic wind to the inhabitants of Europe.

instrument suggested with regard to the different densities of the atmosphere in the different zones of the earth. But since the experiments on the expansion of moist air have shown its elasticity to be so much greater than that which is dry, the simple principle of heat and moisture may suffice to account for all the phenomena. Thus it is universally admitted, that there is a greater degree of humidity and heat in the air near the earth's surface, than there is in the higher regions of the atmosphere. The elasticity or expansion of the lowermost section of every column of air, whether long or short, will consequently be greater than the uppermost section of it; for the heat, by dissolving the moisture, produces a vapour lighter than air, which mixing with its particles, removes them farther from each other, increases the elasticity of the general mass, and diminishes its specific gravity comparatively more than it does that of the section immediately above it, where there is less heat and less moisture. Hence it is inferred, that the equation for the air, in any assigned vertical, will gradually diminish as the elevation of the place above the sea increases, and that it will vanish at the top of the atmosphere. This is in some respect confirmed by the experiments on the expansion of rare air; for from them it appears, when the particles are very far removed from each other, by a great diminution of pressure, as is undoubtedly the case in the higher regions of the atmosphere, they lose a great part of their elastic force. Thus the equation, answering to any particular temperature, above or below the zero of the scale, at different heights above the surface, will be expressed by the ordinates to a curve of the hyperbolic order, whose curvature may be supposed to change fast near the surface of the earth, and differ insensibly from a straight line at great heights above it.

With regard to the latitudinal equation, the same principle of heat and moisture seems to make it probable, that such will become necessary in operating with the barometer; for it is well known, that there is a great degree of humidity in the air between the tropics; and, on the contrary, that the polar atmospheres are very dry. The heat and moisture being greatest at the equator, there the elasticity or equation will also be the greatest at the level of the sea; and the zero of the scale will necessarily descend to a lower point of the thermometer, than that to which it corresponds in middle latitudes. As the elasticity of the air at the level of the sea, or equal heights above it, with the same degree of heat, will always be proportionable to the quantity of moisture dissolved in it, therefore it will gradually diminish from the equator towards the poles, that is to say, the zero of the scale will ascend in the thermometer, coincide with the 32^d degree in the middle latitudes, and in its motion upwards will give the equation to be applied with the contrary sign in high latitudes. Hence it is inferred that every latitude, climate, or zone, will not only have its

particular zero, but also its particular curve, whose ordinates will always measure the equations applicable in the respective situations. The equatorial curve will probably change the fastest, and the others become gradually flatter; as they approach towards the poles, where the greater, but more uniform density of the atmosphere may occasion it to differ but little from a straight line. I apprehend however that even at the pole some small diminution might be found to take place in the equation, were it possible, in that region, to prove it by experiments at a sufficient height above the level of the sea.

XXXV. Of a New Micrometer and Megameter. By the Abbé Boscovich, &c.
p. 789.

Having heard that the Abbé Rochon had exhibited a kind of micrometer, which, by means of a prism of rock crystal whose angles could be varied, gave 2 images of the same object, and changed their distances by the circular motion of one of the 2 parts which composed it; I told some of my friends, and among others the Abbe Fontana, that I saw how the thing was done; but that an improvement would be made in it if the distance from the prism to the focus of the eye-glass were made variable. I added that the same effect might be produced without the double refraction of the rock crystal, with a prism made of common glass, only smaller than the aperture of the object-glass. The rays which pass through the prism would then form an image which would be seen out of its natural place; and those which pass without would give another image, in the same place it would have appeared in, if the prism had not been there.

A few days after, the Abbé Fontana was told, that the Abbé Rochon had thought of another micrometer with a prism of rock crystal, which, approaching more or less to the focus of the object-glass, had the advantage of producing a very great effect, and might be acromatic; that having accordingly had his instrument executed, and made several observations with it, he had prepared a paper on the subject, to be read at the next meeting of the academy. This the Abbé Fontana immediately told me, and I repeated what I had said to him the first time on the effect of this micrometer; adding at the same time the precise measure of the scale for the measure of the same effect, and the facility of obtaining the same thing without the rock crystal; mentioning likewise other advantages which might be derived from the common glass prism not covering the whole aperture of the object-glass; and, among others, that of being able to measure much greater angles by this means than by the double refraction of the rock crystal.

The Abbé Rochon accordingly read his paper to the Academy, and mention has been made of it in the public prints: he has therefore the merit of having thought of the same thing, at the same time with, or perhaps before me,

without any knowledge whatever of my ideas on the subject; he has been the first who announced it to the world, who had it executed, and who made use of it: I have therefore no pretensions whatever on that head; he has the merit of a great discovery, and astronomy has the sole obligation of it to him. But the Abbé Rochon has only made use of the double refraction of the rock crystal for his micrometer, and I am assured he has said, that his prism could give him no more than 6 degrees. Now it is well known, that pieces of rock crystal, large enough and pure enough for these purposes, are extremely rare; besides, the difficulty of working them is great, that substance being harder than glass, and requiring the utmost attention in cutting, in order to obtain the difference desired between the 2 refractions. I think therefore, that it will be doing an essential service, to propose another micrometer of common glass, to explain the theory of it, and to extend it to much larger angles, which may render it applicable to the optical instruments made use of in the navy, in taking geographical latitudes and longitudes.

I had already made a prism of this sort, and showed the Abbé Fontana its effect for the double image of the sun on his excellent little achromatic glass: the 2 images were procured by applying this prism to the object-glass with the hand, in such a manner that it covered only $\frac{1}{2}$ of it: pushing it more or less forward, occasioned a change in the brightness of the light of the 2 images, and showed that they might be made equally clear. By changing the inclination of this piece, the distance between the 2 images was varied, which did not alter when its distance from the object-glass was varied without the glass. This piece was a common prism, which gave a refraction a little greater than the apparent diameter of the sun: I added another to it afterwards, of the same kind and equal, both of them having circular bases. Turning one of the 2 parts on its axis, will vary the angle from 0, to double each in particular, which occasions the 2 images to approach to, or recede from, each other. A much slower variation is obtained by the greater or less distance of the prism from the object-glass; but there is a particular reason for which one cannot give it too large a one, the contraction of the pencil of rays belonging to each point of the object, not allowing that distance to be very great, for fear of weakening too much the direct image towards the middle of the field, by the interception of too great a part of the same pencil, which in the end would occasion its being altogether lost.

There is now making for me a rude machine, in which one of the pieces may be turned by the hand on its axis, to make the distance between the 2 images somewhat larger than that which is intended to be measured, as for instance the diameter of the sun; and by the help of a moveable screw, one may carry the prism, thus composed, to a distance from the object-glass, by a motion similar to that of the small mirror of the telescope. I have had it adapted to an ordi-

nary glass of about 4 feet, where its effect, for the sun's diameter, must be much greater than an inch of motion in a minute; for the other planets one may have 10 or 15 lines in a second, or even more. Generally the scale is the whole length of the glass for the total refraction of the prism, which is also the case in the Abbé Rochon's prism, for the difference of the 2 refractions. But one may vary the angle by applying the prism without the glass near the object-glass, and turning one of the parts on its axis. In that case, the scale of the excess of the sum of the refractions of the 2 parts of the prism above the difference, will be in length no more than half the circumference of a circle, though the circle may be made as large as one pleases; but the difference of the distance of the images, will not be proportional to the difference of the arcs run through by the index. In order to determine the relation which the motion of the index bears to the variation of the distance between the 2 images, one must have the solution of a geometrical problem, which is easily gained by spherical trigonometry; but it will always be better to determine this relation by an actual terrestrial observation of a divided ruler, observed at a given distance.

When the angle happens to be a large one, the colours would naturally be such as would greatly deform one of the 2 images of the object, namely, that given by the rays that pass through the prism; but this is easily remedied, at least in a great measure, by making each prism of 2 pieces, one of common and the other of flint-glass. We may multiply the composed achromatic prisms with variable angles, by making the one give degrees from 5 to 5, or from 2 to 2, and the other the minutes. We may put 2 on the outside near the object-glass, which will change the distance of the images by the circular motion, and give the angle required a little larger than the real one; and another within, which will exactly give the seconds. I have already thought of the instruments requisite for obtaining all these objects with accuracy, as well as for the application of a variable prism to the common sailor's octant, and have by me the solution of the necessary problems: this will be the object of a work I am preparing. In the mean time I publish this, to give others an opportunity of hitting on something better concerning the mechanical construction of these instruments.

XXXVI. Of a New Instrument for Measuring Small Angles, called the Prismatic Micrometer. By the Rev. Nevil Maskelyne, D.D., F.R.S., &c.
p. 799.

Practical astronomy was much benefited by the invention of the wire micrometer, for measuring differences of right ascension and declination: and it received at least equal advantage from Mr. Savery's ingenious invention of the divided object-glass micrometer, which has been rendered more commodious by

the late Mr. John Dollond's application of it to the object-end of a reflecting telescope, or the present Mr. Peter Dollond's application of it to the object-end of an achromatic refracting one. But, valuable as the object-glass micrometer undoubtedly is, some difficulties have been found in the use of it, owing to the alterations in the focus of the eye, which are apt to cause it to give different measures of the same angle at different times. For instance, in measuring the sun's diameter, the axes of the pencils of rays, which come through the 2 segments of the object-glass from contrary limbs of the sun, crossing each other at the focus of the telescope under an angle equal to that of the sun's diameter, the union of the limbs of the 2 images of the sun cannot appear perfect, unless the eye be disposed to see objects distinctly which are placed at the point of intersection. But if the eye be disposed to see objects distinctly, which are placed nearer the object-glass than the intersection is, the 2 limbs will appear separated by the interval of the axes of the pencils in that place; and if the eye be disposed to see objects distinctly, which are placed farther from the object-glass than the intersection is, the 2 limbs will appear to encroach on each other by the distance of the axes of the pencils, after their crossing, taken at that place.

To explain this, let o , v , plate 2, fig. 7, represent the centres of the two semi-circular glasses of the object-glass micrometer, separated to the distance ov from each other, subtending the angle oav , equal to the sun's diameter, at the point a , which is the common focus of the 2 pencils of rays having oa and va for their axes, namely, those proceeding from contrary sides of the sun, and passing through the contrary semi-circles; and let d be the eye-glass. It is evident, that if d be properly placed to give distinct vision of objects placed at the point a , the rays oa , va , as well as all the other rays belonging to those pencils, will be collected into one point on the retina of the eye; and consequently, the 2 opposite limbs of the 2 images of the sun will seem to coincide, and the 2 images of the sun to touch one another externally. But if the state of the eye should alter, the place of the eye-glass remaining the same, the eye will no longer be disposed to see the image formed at the point a distinctly, but to see an object placed at ef , nearer to or farther from the object-glass distinctly; and therefore an image will be formed on the retina exactly similar to the somewhat confused image formed by the rays on a plane perpendicular to their course at ef . Consequently, as the 2 cones of solar rays, boa , cva , formed by the 2 semi-circles, are separated or encroach on each other, at this point of the axis, by the distance ef , the 2 images of the sun will not seem to touch each other externally, but to separate or to encroach on each other by the interval ef . The error thus introduced into the measure of the sun's diameter, will be the angle erf , subtended by ef at r , the middle point between o and v , which is to eaf or oav ,

the sun's apparent diameter, as ae to er , or even to ar , on account of the smallness of ae with respect to ar .

These considerations concerning the cause of a principal error that has been found in the object-glass micrometer led me to inquire, whether some method might not be found of producing 2 distinct representations of the sun, or any other object, which should have the axes of the pencils of rays, by which they are formed, diverging from one and the same point, or nearly so: and it occurred to me, that this might be done by the refraction of a prism placed to receive part of the rays proceeding from the object, either before or after their refraction through the object-glass of a telescope. If the prism be placed without the object-glass, the rays that are refracted through it will make an angle with the rays that pass beside it, equal to the refraction of the prism; and this angle will not afterwards be altered by the refraction of the object-glass. Consequently 2 images of an object will be represented, and the prism so applied will enable us to measure the apparent diameter of any object, or any other angular distance which is equal to the refraction of the prism. But if the prism be placed within the object-glass, that is, between the object-glass and eye-glass, the angle measured by the instrument will vary according to the distance of the prism from the focus of the object-glass, bearing the same ratio to the refraction of the prism, as the distance of the prism from the focus bears to the focal length of the object-glass.

Let ACB , fig. 8, represent the object-glass, and d the eye-glass of a telescope, and PR a prism placed to intercept part of the rays coming from an object, suppose the sun, before they fall on the object-glass. The rays EE proceeding from the eastern limb of the sun, and refracted through the object-glass ACB without passing through the prism, will form the corresponding point of the sun's image at e ; and the rays ww , proceeding in like manner from the western limb of the sun, will be refracted to form the correspondent point of the sun's image at w . But the rays $2E$, $2E$, $2w$, $2w$, proceeding in like manner from the eastern and western limbs of the sun, and falling on the prism PR , and thence refracted to the object-glass ACB , will, after refraction through it, form the correspondent points of the sun's image at $2e$, $2w$. Let the refraction of the prism be equal to the sun's apparent diameter: in this case, at whatever distance the prism be placed beyond the object-glass, the 2 images of the sun we , $2w2e$, will touch each other externally at the point $e2w$; for the rays $2w$, $2w$, proceeding from the western limb of the sun, being inclined to the rays EE , proceeding from the eastern limb, in the angle of the sun's apparent diameter, will, after suffering a refraction in passing through the prism equal to the sun's apparent diameter, emerge from the prism, and fall on the object-glass parallel to the rays EE , and

consequently will have their focus $2w$ coincident with the focus e of the rays EE , and therefore the 2 images of the sun we , $2w2e$, will touch each other externally at the point $e2w$, and the instrument will measure the angle $EC2w$, and that only.

But if the prism be placed within the telescope, the angle measured by the instrument, will be to the refraction of the prism, as the distance of the prism from the focus of the object-glass, is to the focal distance of the object-glass: or if 2 prisms be used to form the 2 images, with their refracting angles placed contrary ways, as represented in fig. 9 and 10, the angle measured, will be to the sum of the refractions of the prisms, as the distance of the prisms from the focus of the object-glass, is to the focal distance of the object-glass. For let ACB (fig. 9) represent the object-glass, and d the eye-glass of a telescope, and PR , RS , 2 prisms interposed between them, with their refracting angles turned contrary ways, and the common sections of their refracting planes touching each other at R . The rays proceeding from an object, suppose the sun, will be disposed, by the refraction of the object-glass, to form an image of the sun at the focus; but part of them falling on one prism, and part on the other, will be thus refracted contrary ways, so as to form 2 equal images we , $2w2e$, which, if the refractions of the prisms be of proper quantities, will touch each other externally at the point $e2w$. Let ECN be the axis of the pencil of rays EE proceeding from the sun's eastern limb; and wco the axis of the pencil of rays ww proceeding from the sun's western limb; and the point N the place where the image of the sun's eastern limb would be formed, and the point o where that of the western limb would be formed, were not the rays diverted from their course by the refractions of the prisms. But by this means part of the rays EE , which were proceeding to N , falling on the prism PR , will be refracted to form an image of the sun's eastern limb at e , while other rays EE , which fall on the prism RS , will be refracted to form an image of the sun's eastern limb at $2e$. In like manner, part of the rays ww , which were proceeding to form an image of the sun's western limb at o , falling on the prism RS , will be refracted to form an image of the sun's western limb at $2w$ coincident with e , the point of the image correspondent to the sun's eastern limb; while other rays ww , which fall on the prism PR , will be refracted to form the image of the sun's western limb at w . The two images we , $2w2e$, are supposed to touch each other externally at the point $e2w$. The ray EER , which belongs to the axis ECN , and is refracted by the prism PR to e , undergoes the refraction NRe , which (because small angles are proportional to their sines, and the sine of NRe is equal to the sine of its supplement NRC) is to NCR , as NC or ce is to NR or Re . In like manner the ray wGR , which belongs to the axis wco , and is refracted by the prism RS to $2w$ or e , undergoes the refraction ore , which is to oce , as oc or ce is to Ro or Re ; there

fore, by composition, ORN , the sum of the refractions ORE , NRE , is to OCN the sum of the angles OCE , NCE , or the sun's apparent diameter, as CE to RE ; that is, as the focal distance of the object-glass to the distance of the prisms from the focus of the object-glass.

Or let the prisms PR , RS , be placed with their refracting angles P , S , turned from each other, as in fig. 10: the refraction of the prism PR will transfer the image of the sun from ON to we , and the refraction of the prism RS will transfer the image ON to $2w2e$, the 2 images $2w2e$, we , touching each other externally at the point $2ew$. Let ECN , wco , be the axes of the pencils of rays proceeding from the 2 extreme limbs of the sun, and N , o , the points where the images of the sun's eastern and western limbs would be formed by the object-glass, were it not for the refraction of the prisms; the ray EFR , which belongs to the axis ECN , and is refracted by the prism RS to $2e$, undergoes the refraction $NR2e$; and the ray wGR , which belongs to the axis wco , and is refracted by the prism PR to w , undergoes the refraction ORw . Now $NC2e$, part of the angle measured, is to $NR2e$, the refraction of the prism RS , as Rw to cw ; and ocw , the other part of the angle measured, is to ORw , the refraction of the prism PR , in the same ratio of Rw to cw : therefore OCN , the whole angle measured, is to ORN , the sum of the refractions of the 2 prisms, as Rw to cw ; that is, as the distance of the prisms from the focus of the object-glass to the focal distance of the object-glass.

When the prisms are placed in the manner represented in fig. 9, the point e , of the image we , is illuminated only by the rays which fall on the object-glass between A and F , and the point $2w$ only by the rays which fall on the object-glass between B and G . Now the angles CRF , CRG , equal to the refractions of the prisms, being constant, the spaces FC , CG , will increase in proportion as the distances RF , RG , increase, and the spaces AF , GB , diminish as much; and therefore the images at the point of mutual contact $e2w$, will be each illuminated by half the rays which fall on the object-glass when the prisms are placed close to the object-glass; but will be enlightened less and less the nearer the prisms are brought to the focus of the object-glass.

But when the prisms are placed in the manner shown in fig. 10, the images at the point of contact, as the prisms are removed from the object-glass towards the eye-glass, will be enlightened with more than half the rays that fall on the object-glass, and will be most enlightened when the prisms are brought to the focus itself; for the point $2e$, of the image $2w2e$, will be enlightened by all the rays EE that fall on the object-glass between B and F , and the point w , of the image we , will be enlightened by all the rays ww which fall on the object-glass between A and G . But the difference of the illuminations is not very considerable in achromatic telescopes, on account of the great aperture of the object-

glass; as the greatest space FG is to the focal distance of the object-glass, as the sum of the sines of the refractions of the prism is to the radius.

There is a 3d way, and perhaps the best, of placing the prisms, so as to touch each other along their sides which are at right angles to the common sections of their refracting planes. In this disposition of the prisms, the images will be equally enlightened, namely, each with half the rays which fall on the object-glass, wherever the prisms be placed between the object-glass and eye-glass.

From what has been shown it appears, that this instrument, which may be properly called the prismatic micrometer, will measure any angle that does not exceed the sum of the refractions of the prisms, excepting only very small angles, which cannot be taken with it, on account of the vanishing of the pencils of rays at the juncture of the 2 prisms near the focus of the object-glass; that it will afford a very large scale, namely, the whole focal length of the object-glass for the greatest angle measured by it; and that it will never be out of adjustment; as the point of the scale where the measurement begins (or the point of o) answers to the focus of the object-glass, which is a fixed point for celestial objects, and a point very easily found for terrestrial objects. All that will be necessary to be done, in order to find the value of the scale of this micrometer, will be to measure accurately the distance of the prisms from the focus, when the instrument is set to measure the apparent diameter of any object subtending a known angle at the centre of the object-glass, which may be easily found by experiment, as by measuring a base and the diameter of the object observed placed at the end of it, in the manner practised with other micrometers; for the angle subtended by this object, will be to the angle subtended by a celestial object, or very remote land object, when the distance of the prisms from the principal focus is the same as it was found from the actual focus in the terrestrial experiment, as the principal focal distance of the object-glass is to the actual focal distance in the said experiment.

It will, I apprehend, be the best way in practice, instead of 1 prism, to use 2 prisms, refracting contrary ways, and so divide the refraction between them, as represented in fig. 9 and 10. Achromatic prisms, each composed of 2 prisms of flint and crown glass, placed with their refracting angles contrary ways, will undoubtedly be necessary for measuring angles with great precision by this instrument: and I can add with pleasure, that I find by experiment made with this instrument, as it was executed by Mr. Dollond with achromatic prisms, ground with great care for this trial above a year ago, that the images after refraction through the prisms appear very distinct; and that observations of the apparent diameters of objects may be taken in the manner here proposed with ease and precision.

Two or more sets of prisms may be adapted to the same telescope, to be used

each in their turn, for the more commodious measurement of different angles. Thus it may be very convenient to use one set of prisms for measuring angles not exceeding $36'$, and consequently fit for measuring the diameters of the sun and moon, and the lucid parts and distances of the cusps in their eclipses; and another set of prisms to measure angles not much exceeding 1 minute, and consequently fit for measuring the diameters of all the other planets. This latter set of prisms will be the more convenient for measuring small angles, on account of a small imperfection attending the use of this micrometer, as before mentioned; namely, that angles cannot be measured with it when the prisms approach very near the focus of the object-glass, the pencils of rays being there lost at the point where the prisms touch each other.

On the principles that have been here explained, a prism placed within the telescope of an astronomical instrument, adjusted by a plumb-line or level, to receive all the rays that pass through the object-glass, may conveniently serve the purpose of a micrometer, and supersede the use both of the vernier scale and the external micrometer; and the instrument may then be always set to some even division before the observation. Thus the use of a telescopic level may be extended to measure, with great accuracy, the horizontal refractions, the depression of the horizon of the sea, and small altitudes and depressions of land objects. Time and experience will doubtless suggest many other useful applications of this instrument.

A paper from the learned Abbé Boscovich was read before this Society the 9th of last June, describing a similar contrivance as an invention of the Abbé Rochon, in which the Abbé Boscovich himself also claims some share; I therefore desire to acquaint this Society, that I communicated this invention to Mr. Dollond, and had it executed by him; and also showed the instrument itself, so executed, to my esteemed friend Alexander Aubert, Esq., fellow of this Society, a gentleman very well qualified to judge of things of this nature, above a year before the communication of the Abbé Boscovich's paper, as will appear from their written attestations, drawn up at my desire, describing the particulars of the communication of this invention which I made to them so long ago. May I be permitted to remark, that this instrument having been executed by my directions, in several forms, by Mr. Dollond, between the months of March and August, 1776, and set up and tried at his house in the presence of several of his workmen, could not be considered as an absolute secret concealed from the public. However, I doubt not that the following attestations of Mr. Aubert and Mr. Dollond, will sufficiently prove my title to this invention of the prismatic micrometer; and I take this opportunity of exhibiting to the Society the instrument itself, mentioned in Mr. Dollond's letter as executed by himself according to my directions, and sent to the Royal Observatory in the month of August 1776.

Greenwich, Dec. 11, 1777.

To the Rev. Dr. Maskelyne.

REV. SIR,

St. Paul's Church-yard, Nov. 22, 1777.

According to your desire I send the following particulars of the experiments which were made by your directions, for completing a new kind of micrometer for measuring small angles. About the beginning of April 1776, I received your first directions respecting this matter, which were to make 2 prismatic glasses or wedges, of such angles that rays of light, which passed through them, should be refracted about $18'$ of a degree: these were to be placed between the object-glass and eye-glass of an achromatic telescope about 30 inches long. The angular edges of the 2 prismatic glasses were to be placed in contact with each other; they were to be moved in a parallel position from the object-glass to the focus of the eye-glass, and to be of such a size as to cover the aperture of the object-glass when brought close to it. By the refraction of these wedges 2 images were formed in the telescope, which were at the greatest distance, about 36, when the wedges were close to the object-glass, and approached as they were moved towards its focus, where they united; so that the whole focal distance of the object-glass was to be the length of the scale for measuring the angular distance of the 2 images formed in the telescope. When these wedges were applied, as above described, the 2 images were found to be coloured to a great degree, occasioned by the refraction of the wedges. This defect you directed me to remove, by making the prismatic glasses or wedges achromatic, on the same principles as the achromatic object-glasses; and, after some difficulties, this was effected; the 2 images formed in the telescope appeared free from colours and distinct. The above experiments were made in a rough wooden tube, with an inconvenient method of moving the wedges by hand; in this state it was when shown to Alexander Aubert, Esq., F.R.S., towards the end of May, 1776; after which you desired to have it done in a more complete manner, in a brass tube, with a means of turning the tube round to take angles in different directions, and a method of moving the wedges with a screw. This was completed about the middle of August in the same year, and then sent to the Royal Observatory. I have the honour to be, &c.

PETER DOLLOND.

I hereby certify, that in the month of May, 1776, the Rev. Mr. Maskelyne, Astronomer-royal, produced to me, at Mr. Dollond's house in St. Paul's Church-yard, and in his presence, as a new invention of his own, an instrument for measuring small angles, consisting of 2 achromatic prisms or wedges applied between the object-glass and eye-glass of an achromatic telescope about 30 inches long, by moving of which wedges nearer to, or further from, the object-glass, the two images of an object produced by them appeared to approach to, or recede from, each other, so that the focal length of the object-glass became a scale for measuring the angular distance of the two images.

London, Nov. 27, 1777.

ALEX. AUBERT.

XXXVII. The Report of the Committee appointed by the Royal Society to consider of the Best Method of Adjusting the Fixed Points of Thermometers; and of the Precautions necessary to be used in making Experiments with those Instruments. p. 816.

It is universally agreed, by all those who make and use Fahrenheit's thermometers, that the freezing point, or that point which the thermometer stands at when surrounded by ice or snow beginning to melt, is to be called 32° ; and that the heat of boiling water is to be called 212° : but for want of further regulations concerning the manner in which this last point is to be adjusted, it is placed not less than 2 or 3 degrees higher on some thermometers, even of those made by our best artists, than on others. The 2 principal causes of this difference are, first, that it has never been settled at what height of the barometer this point is to be adjusted;* and 2dly, that so much of the quicksilver in the thermometer as is contained in the tube, is more heated in the method used by some persons, than in that used by others. To show that this last circumstance ought by no means to be disregarded, suppose that the ball of a thermometer be dipped into boiling water as far as to the freezing point, and consequently that the length of the column of quicksilver in that part of the tube which is not immersed in the water be 180° ; and suppose that the heat of that part of the column of quicksilver be no more than 112° . If the thermometer be now entirely immersed in the water, the heat of this column will be increased 100° ; and consequently its length will be increased by $\frac{100}{11500}$ parts of the whole, as quicksilver expands $\frac{1}{11500}$ part of its bulk by each degree of heat; and consequently the thermometer will stand $\frac{180 \times 100}{11500}$ or rather more than $1^{\circ}\frac{1}{2}$ higher than it did before. Another thing to be considered in adjusting the boiling point is, that if the ball be immersed deep in the water, it will be surrounded by water which will be compressed by more than the weight of the atmosphere, and on that account will be rather hotter than it ought to be.

We are of opinion, that the quicksilver in the tube ought, if possible, to be kept of the same heat as that in the ball, and that the ball ought not to be immersed deep in the water. These 2 requisites may be obtained by using a vessel covered so as to allow no more passage than what is sufficient for carrying off the steam; for then, if the thermometer be inclosed in this vessel in such manner, that the boiling point shall rise but a little way above the cover, almost

* Fahrenheit found that the heat of boiling water differed according to the height of the barometer; but supposed the difference to be much greater than it really is. Mr. De Luc has since, by a great number of experiments made at very different heights above the level of the sea, found a rule by which the difference in the boiling point, answering to different heights of the barometer, is determined with great exactness. According to this rule the alteration of the boiling point, by the variation of the barometer from $29\frac{1}{2}$ to $30\frac{1}{2}$ inches, is $1^{\circ}.59$ of Fahrenheit.—Orig.

all the quicksilver in the tube will be surrounded by the steam of the boiling water, and consequently will be nearly of the same heat as the water itself: we therefore made some experiments to determine how regular the boiling point would be when tried in such vessels, both when the ball was immersed in the water, and when it was exposed only to the steam as recommended by Mr. Cavendish, in these abridgments, vol. 14, p. 51.

The vessel used in these experiments is represented in fig. 11, pl. 2. ABba is the pot containing the boiling water; dd is the cover; E is a chimney for carrying off the steam; mm is the thermometer fastened to a brass frame; this thermometer is passed through a hole ff in the cover, and rests on it by a circular brass plate gg fastened to its frame, a piece of woollen cloth being placed between gg and the cover, the better to prevent the escape of the vapours. Two pots of this kind were used by us; one 5 inches in diameter and 9 deep; the other, $4\frac{1}{4}$ in diameter and 23 deep. Two of the thermometers principally used were short ones, the brass plate (gg) being placed only $3\frac{3}{4}$ inches above the top of the ball, and the boiling point rising not much above that plate: the 3d thermometer was much longer, the plate (gg) being 17 inches above the ball. They were all 3 quick; the 1st containing only $2\frac{1}{2}$ degrees to an inch; the 2d 5° ; and the 3d 10° . The 1st had a cylinder instead of a ball, $1\frac{1}{4}$ inch long and $\frac{4}{10}$ in diameter;* the two others had spherical balls, about $\frac{3}{4}$ of an inch in diameter.

On trying these thermometers in the abovementioned vessels, with the water rising 2 or 3 inches above the top of the ball, we found some variations in the height, according to the different manner of making the experiment, but not very considerable; for the most part there was very little difference whether the water boiled fast or very gently; and what difference there was, was not always the same way, as the thermometer sometimes stood higher when the water boiled fast, and sometimes lower. The difference however seldom amounted to more than $\frac{1}{10}$ of a degree, unless a considerable part of the sides of the pot were exposed to the fire; but in some trials which we made with the short thermometers in the short pot, with near 4 inches of the side of the vessel exposed to the fire,† they constantly stood lower when the water boiled fast than when slow, and the height was in general greater than when only the bottom of the

* In the two short thermometers the quicksilver would have descended into the ball when cold, had not the tube been swelled a little, close to the ball, in order to prevent it.—Orig.

† In all our experiments, the water was boiled over a portable black-lead furnace, covered with an iron plate, which had a hole cut in it just large enough to receive the bottom of the pot; so that, by passing the bottom through this hole to a greater or less depth, we could expose more or less of the sides to the fire. In the other experiments, not more than one inch of the sides was ever exposed to the fire.—Orig.

pot was exposed to the fire. This difference however was not perceived in the trials of the long thermometer in the deep pot, as there seemed very little difference in the height whether the water boiled fast or slow, or whether more or less of the side of the pot was exposed to the fire. The greatest difference observed in the same thermometer, on the same day and in the same water, according to the different manner of trying the experiment, was half a degree.

We made some trials with the long thermometer in the deep pot, to determine how much the height of the boiling point was affected by a greater or less depth of water above the ball. By a mean of the experiments it stood .66 of a degree higher when the water rose 14 inches above the ball, than when it was only 3 inches above the ball; so that increasing the depth of water above the ball by 11 inches, raised the thermometer .66 of a degree, that is .06 for each inch. We would by no means infer however from hence, that it is a constant rule, that the height of the boiling point is increased .06 of a degree by the addition of each inch in the depth of the water above the ball; as perhaps the proportion would be found very different in greater depths of water or in wider vessels.

If this rule be constant, it would show that, when the pressure on that part of the water which surrounds the ball is increased, by increasing the depth of water above the ball, the height of the boiling point is not altered by it more than one half as much as by an equal increase of pressure produced by an alteration in the weight of the atmosphere: for the pressure on that part of the water which surrounds the ball, is as much increased by an alteration of 11 inches in the depth of the water above the ball, as by an increase of $\frac{11}{13\frac{1}{2}}$ of an inch in the height of the barometer; and such an alteration in the height of the barometer is sufficient to raise the boiling point $1^{\circ}.3$.

It seems as if the height of the boiling point was in some measure increased by having a great depth of water below the ball, as in general the short thermometers stood higher when tried in the deep pot than in the short one; this effect however did not always take place. In the former of these cases, the depth of water below the ball was about 18 inches, in the other only 4; but the depth of water above the ball was the same in both cases. It must be observed, that when there was a great depth of water in the vessel, either above or below the ball, the experiments were much more irregular, and the quicksilver in the tube remained much less steady than when it was small. When the depth of water in the vessel is great, it is apt to boil in gusts, which seems to be the cause of this irregularity; though we could not perceive any regular connection between these gusts and the rising of the thermometer.

In the experiments made with the water not rising so high as the ball, so that the thermometer was exposed only to the steam, we very seldom found any

sensible difference whether the water boiled fast or slow: but whenever there was any, the greater height was when the water boiled fast: the difference however never amounted to more than $\frac{1}{20}$ of a degree. There was scarcely ever any sensible difference whether the short thermometers were tried in the short pot or the deep one, though in the former case the ball was raised very little above the surface of the water, and in the latter not less than 14 inches: neither did we find any sensible difference in trying them in the tall pot, whether there was a greater or less depth of water in the vessel.

As it was however suspected, that the heat of the steam might possibly be less near the top of the pot than lower down (for in these experiments the ball of the thermometer was always at the same depth below the cover, though its height above the surface of the water was very different) we made 2 holes in the side of a pot 4 inches deeper than the deepest of the foregoing, one near the top of the pot, and the other not far from the bottom, and passed the ball of the thermometer through one or the other of these holes, taking care to stop up both holes very carefully, so that no air could enter into the pot by them: no sensible difference could be perceived in the height, whether the thermometer was placed in the upper or lower hole, though in one case the ball was only 3 inches, and in the other 21 inches, below the cover. The heat of the steam therefore appears to be not sensibly different in different parts of the same pot; neither does there appear to be any sensible difference in its heat, whether the water boil fast or slow; whether there be a greater or less depth of water in the pot; or whether there be a greater or less distance between the surface of the water and the top of the pot; so that the height of a thermometer tried in steam, in vessels properly closed, seems to be scarce sensibly affected by the different manner of trying the experiment.

Though there was scarcely any difference in the height of the quicksilver, whether the water boiled fast or slow, yet, when the water boiled slow, the thermometer was a great while before it rose to its proper height; and when it boiled very slow, it seemed doubtful whether it would have ever risen to it, especially if the ball was raised a great way above the surface of the water: but when, by making the water boil briskly, the thermometer had once risen to its proper height, the water might then be suffered to boil very gently, even for a great length of time, without the thermometer sinking sensibly lower.*

All 3 thermometers were found to stand, in general, from 30 to 65 hundredths

* The reason of this seems to be, that, while any air is left in the pot, the steam cannot acquire its full degree of heat; and that when the water boils very gently, the air is not easily quite expelled from the pot. That the steam will not acquire its full degree of heat while any air is left in the pot, will appear from the next paragraph but one.

of a degree higher when the ball was immersed a little way in the water (neglecting those observations in which much of the sides of the pot were exposed to the fire) than when it was tried in steam: at a medium they stood $\frac{4.3}{100}$ higher, which is equal to the difference produced by a variation of $\frac{3}{10}$ of an inch in the barometer; so that the boiling point, adjusted at a given height of the barometer, with the ball immersed a little way in the water, will in general agree with that adjusted in steam, when the barometer is $\frac{3}{10}$ of an inch higher.

It must be observed, that in all these experiments a piece of flat tin plate was laid loosely on the mouth of the chimney E, so as to leave no more passage for the steam than what was sufficient to prevent the tin plate from being lifted up. In trying the thermometers in steam, this is by no means unnecessary; for, if the cover of the pot does not fit pretty close, the thermometers will immediately sink several degrees on removing the tin plate; but when their balls are immersed in the water, the removal of the tin plate has no sensible effect.

If this cover to the chimney had been heavy, the included steam might have been so much compressed by it, that the water and steam might have acquired a considerably greater heat than they ought to have done; but as this plate lay loose on the chimney, and as its weight was not greater than that of a column of quicksilver, whose base is equal to that of the mouth of the chimney, and whose altitude is $\frac{1}{50}$ of an inch, the excess of the compression of the included steam, above that which it would suffer in an open vessel, could not be greater than that which would be caused by an increase of $\frac{1}{50}$ of an inch in the height of the barometer, which is too small to be worth taking notice of; for if the excess of compression was greater than that, the tin plate must necessarily be lifted up so much as to afford a sufficient passage for the steam to escape fast enough, though urged by no greater force than that.

Though in the different trials of the same thermometer in steam, on the same day, and with the same water, so little difference was observed, according to the different manner of trying the experiment; yet there was a very sensible difference between the trials made on different days, even when reduced to the same height of the barometer, though the observations were always made either with rain or distilled water. The difference however never amounted to more than a quarter of a degree, except in one thermometer, in which there were 3 observations out of 18 which differed more than that; one of them differed so much as 0.65° from some of the rest. In the observations made with the ball immersed a little way in the water, there was a greater difference between the observations of different days, even neglecting those in which much of the sides of the pot were exposed to the fire. In 2 of the thermometers the different observations differed about $\frac{3.5}{100}$ of a degree from each other; but in the other thermometer they varied $\frac{8}{100}$.

We do not at all know what this difference could be owing to, especially in the observations in steam. It could not proceed entirely from some unknown difference in the water; for if it did, the difference between the different thermometers should have been always the same, which was not the case, though in general, on those days in which one thermometer stood high, the others did also, especially in the trials in steam. Also, as far as can be perceived from our experiments, there seems to be very little difference between different waters, with respect to the heat which they acquire in boiling. We could not be sure that there was any difference between rain or distilled water and pump-water, provided the latter had boiled long: neither did any difference seem to arise from the water containing such substances as are disposed to part readily with their phlogiston; for, on trying the thermometers in the steam of distilled water, their height was not sensibly altered by pouring in a small quantity of a solution of liver of sulphur, or of iron filings imperfectly rusted. The thermometer however seemed to stand sensibly lower in pump water beginning to boil, than in the same water long boiled, but the difference scarcely exceeded $\frac{1}{10}$ or $\frac{1}{5}$ of a degree.

We made some experiments to determine the heat of water boiling in open vessels. In general, when the vessel was almost full, and the water boiled fast, and the ball of the thermometer was held from $\frac{3}{4}$ to 2 or 3 inches under water, and also in that part of the vessel where the current of water ascended upwards, that is, in the hottest part of the water, its heat was not much different from that of the steam of water boiling in closed vessels, varying only from a quarter of a degree more than that, to as much less; but if the water boiled gently, its heat would frequently be half or three-quarters of a degree cooler than the steam. If the experiment was tried in the deep pot with such a quantity of water in it that the surface was at least 14 or 15 inches below the top of the pot, so that though the vessel was open, yet the water was not much exposed to the air, its heat then seemed scarcely less than when boiled in closed vessels.

In making these experiments, we chiefly made use of the 2 short thermometers, in which, as the quantity of quicksilver contained in the tube was small, the error arising from that part of the quicksilver being not heated equally with that in the ball, could be but small: for example, in the 2d of the short thermometers, the number of degrees contained in that part of the tube between the circular plate gg and the ball was 18° . In the experiments in steam, this part of the tube was heated to the same degree as the ball. Suppose now, that in open vessels it was heated only to 122° , or was 90° cooler than the ball, it is plain that the thermometer would stand at only $\frac{18 \times 90}{11500}$, or $\frac{1}{7}$ of a degree lower than it did in steam, provided the heat of the quicksilver in the ball was the

same in both cases. In the other short thermometer, as there were only half as many degrees to an inch, the error was only half as great.

In several of the experiments however we made use of the long thermometer; but then it was necessary to make an allowance on account of the quicksilver in the tube being not heated equally with that in the ball. The better to enable us to do this, we made use of a thermometer tube, filled with quicksilver in the same manner as a thermometer, only without any ball to it, or a thermometer without a ball, as we may call it. A small brass plate was fixed to the tube near the top of the column of quicksilver, to show the heat, as in a common thermometer. In all our experiments with the long thermometer in open vessels, this tube, without a ball, was placed by its side; whence, as the quicksilver in the tube of the long thermometer could hardly fail of being nearly of the same heat as that in the tube without a ball, we knew pretty nearly the heat of the quicksilver in the tube of the former, and consequently how much higher it would have stood if the quicksilver in its tube had been of the same heat as that in the ball. For example, on October 19, the long thermometer tried in an open vessel, the water boiling fast, stood $1^{\circ}.65$ lower than it did when tried in steam the same day, the quicksilver in the tube without a ball standing at the same time at 109° : we may therefore conclude, that the heat of the quicksilver, in that part of the tube of the long thermometer which was not immersed in the water, was also 109° ; and consequently, as that part of the tube contained about 170° , the thermometer stood $\frac{170 \times 103}{11500}$, or $1^{\circ}.52$ lower than it would have done if the quicksilver in the tube had been of the same heat as that in the ball; and consequently the quicksilver in the ball of the thermometer was in reality $.07$ cooler than when tried in steam.

We examined the boiling points of several thermometers, made by different artists, by trying them in steam when the barometer was at 30.1, and finding what division on the scale the quicksilver stood at. The difference of the extremes was $3^{\circ}\frac{1}{4}$; but, by a mean of all, it was found to stand at $213^{\circ}.1$, and consequently would have stood at 212° , if the barometer had been at 29.4; so that if the boiling point was to be adjusted, either in steam, when the barometer is at 29.4, or with the ball immersed 2 or 3 inches in water, when the barometer is at 29.1, it would agree best with the mean of the abovementioned thermometers. But as it seems to be of no great signification to make the boiling point agree very nearly with the mean of the thermometers made at present, when the extremes differ so widely; and as we apprehend that it will be more convenient to the makers that some height should be chosen which differs less from the mean, as thus they will more frequently have an opportunity of adjusting the boiling point without the trouble and danger of mistakes which attend the

making a correction, we recommend, that the boiling point should be adjusted when the barometer is at 29.8, if the person chuses to do it in steam; or when the barometer is at $29\frac{1}{2}$, if he chuses to do it in close vessels, with the ball immersed to a small depth under the water. Our reason for pitching on this precise height is, that thus the boiling point will differ from Mr. De Luc's boiling point, by a simple fraction of the degrees of his common scale, namely $\frac{3}{4}$ of a degree higher.

We are informed by Mr. De Luc, that the method he used in adjusting the boiling point, though he forgot to mention it in the *Récherches sur les Modifications de l'Atmosphere*, was to wrap rags round the tube of the thermometer, and to try it with the ball immersed in water in an open vessel, of the form described in the abovementioned book, while boiling water was poured at different times on the rags, in order that the quicksilver in the tube might be heated, if possible, to the same degree as that in the ball. As well as we can judge from the abovementioned experiments in open vessels, and from the few trials we have made of this method, we are inclined to think, that the boiling point adjusted this way will in general differ but little from that adjusted in steam at the same height of the barometer, especially if the thermometer be not very long, and do not extend a great way below the freezing point;* consequently, as Mr. De Luc's boiling point was adjusted when the barometer was at 27 Paris or 28.75 English inches, it will stand lower than that adjusted in the manner recommended by us, by $\frac{3}{4}$ of a degree of his scale; or $80^{\circ}\frac{3}{4}$ on De Luc's thermometer, will answer to 212° on Fahrenheit's adjusted in the manner proposed.

Though the boiling point be placed so much higher on some of the thermometers now made than on others, yet we would not have the reader think that this can make any considerable error in the observations of the weather, at least in this climate; for an error of $1^{\circ}\frac{1}{2}$ in the position of the boiling point will make an error of only half a degree in the position of 92° , and of not more than $\frac{1}{4}$ of a degree in the point of 62° . It is only in nice experiments, or in trying the heat of hot liquors, that this error in the boiling point can be of much signification.

There is another circumstance that we have not yet taken notice of, which, in strictness, causes some error in thermometers, namely, the difference of ex-

* In order to see how much the quicksilver in the tube of the thermometer would be heated in this method of adjusting the boiling point, we took the abovementioned tube without a ball, wrapped it round with rags, and poured boiling water on it as above described: the heat of the quicksilver therein was found to be about 21° less than that of boiling water; and therefore the boiling point of a thermometer, adjusted in this manner, supposing the thermometer to be dipped into the water as far as to the point of 32° , should stand about $\frac{1}{3}$ of a degree lower than it would do if the quicksilver in the tube was heated equally with that in the ball.—Orig.

pansion of the glass tube and the scale. But this error is in almost all cases so small as to be not worth regarding; we have however, in the note below, given a rule for computing the value of it.*

In making experiments with thermometers, it evidently is equally necessary that the quicksilver in the tube should be of the same heat as that in the ball, as it is in adjusting the boiling point: for this reason, in trying the heat of liquors much hotter or colder than the air, the thermometer ought, if possible, to be immersed as far as to the top of the column of quicksilver in the tube. As this, however would often be very difficult to execute, the observer will frequently be obliged to content himself with immersing it to a much less depth. But then as the quicksilver, in a great part of the tube, will be of a different heat from that in the ball, it will be necessary, where any degree of accuracy is required, to make a correction, on that account, to the heat shown by the thermometer. If the heat of the quicksilver in the tube be known, the correction may readily be made by help of the annexed table; the only difficulty lies in estimating what that heat may be. In all probability the heat of the quicksilver in the tube will not be very different from that of the air which surrounds it;† but as that air

* The usual way of adjusting thermometers is, to mark the boiling and freezing points on the glass tube, and not to set off those points on the scale till some time after, when the tube and scale may both be supposed to be nearly of the temper of the air in the room; consequently, when the thermometer is exposed to a greater heat than that, the scale, if of brass, will expand more than the glass tube, and the divisions on it will be longer than they ought to be; but, if the scale be of wood, it will expand less than the glass tube, and the divisions will be too short. Let now the heat of the air, when the divisions were set off on the scale, be called A ; let the degree of heat which the thermometer stands at in the experiment be called D ; and let the degree answering to that point of the scale in which the thermometer is fastened to the scale be called F . Then, if all parts of the thermometer and scale are heated equally, and the scale is of brass, the thermometer will appear to stand lower than it ought to do by the $\frac{D - F \times D - A}{165000}$ part of a degree, observing, that if $D - F \times D - A$ is negative, it will stand higher than it ought to do; but if the scale is of wood, it will stand higher than it ought to do by the $\frac{D - F \times D - A}{216000}$ part of a degree. If the thermometer be fastened to the scale by the ball, or any part of the tube lower than the observed heat, the error will be the same, whether that part of the tube and scale, which is above the observed degree, be of the same heat as the ball or not: but if the thermometer is fastened to the scale by the top of the tube, as is frequently done, then the error will vanish whenever that part of the tube and scale, which is above the observed degree, is not much heated. This rule is founded on Mr. Smeaton's experiments, who found, that white glass expands $\frac{1}{1000}$ of an inch in a foot by 180° of heat; that brass wire expands $\frac{2.32}{100000}$; and that wood expands scarcely sensibly.—Orig.

† This must evidently be the case, unless the quicksilver in the tube is considerably heated by its contact with that in the ball. To see whether this was the case, some sand was heated in a small copper dish, over a lamp, to the heat of about 212° , and the abovementioned tube, without a ball, laid horizontal with the end extending about half an inch over the sand; but to prevent its being heated by it, a piece of wood, about a quarter of an inch thick, was laid between the sand and it. After it had remained a sufficient time in this situation, the division which the quicksilver stood at was

will be affected by the steam of the liquor, and the fire by which it is heated, it will commonly be of a very different heat from the rest of the air of the room in which the experiment is made; but as no great nicety is required in estimating the heat of the quicksilver in the tube, insomuch that a mistake of 25° in it will cause an error of only $\frac{1}{2}$ a degree in the correction, when the number of degrees in that part of the tube which is not immersed in the liquor is not more than 220° , it will commonly be not difficult to guess at the heat of the quicksilver in the tube as near as is required.* But if the observer is desirous of more accuracy, he may find the heat of the surrounding air by holding the ball of a small thermometer near the tube of the thermometer with which he tries the heat of the liquor; or, what will be much better, he may have a tube without a ball, such as is above described, fastened to the frame of the thermometer, on one side of the tube; or if he has 2 such tubes, of different lengths, it will be still more accurate.

observed. The piece of wood was then removed, and the end of the tube laid in the sand, which was heaped over it so that about half an inch of the column of quicksilver was entirely surrounded by the hot sand, and must therefore be heated to nearly the same degree as it. The quicksilver in the tube rose very little higher than before, and seemingly not more than might be owing to the expansion of the half inch of quicksilver which was surrounded by the sand; so that it should seem, that heating one end of the column of quicksilver does not communicate much heat to the rest of the column; and consequently, that when the ball of a thermometer is immersed in hot liquor, the quicksilver in the tube will not be much hotter than the surrounding air.—Orig.

* The better to enable the reader to guess at the heat of the quicksilver in the tube, in cases of this kind, we tried how much the quicksilver in the abovementioned tube, without a ball, would be heated when held over a vessel of boiling water. It is true, that these experiments cannot be of any great service towards this purpose, as the tubes will be very differently heated, according to the degree of heat of the fluid, and the quantity of steam which it furnishes, and according to the nature of the fire by which it is heated; yet as the experiments may perhaps serve in some measure to rectify our ideas on this head, we will give the result. When the abovementioned tube without a ball, the length of the column of quicksilver in which was 15 inches, was held perpendicularly over the vessel of boiling water, with its bottom even with the surface of the water, the heat of the quicksilver was in all the trials we made from 68 to 28° hotter than the air of the room. If the tube was held inclined to the horizon, in an angle of about 30° , with the bottom of the column of quicksilver reaching not more than three-quarters of an inch within the circumference of the pot, so that the column of quicksilver was as little heated by the steam as could easily be done, it was from 30 to 7° hotter than the air. When a shorter tube of the same kind, in which the column of quicksilver was 7 inches, was used, the quicksilver was from 62 to 44° hotter than the air, when held perpendicularly, and from 49 to 36° hotter when held inclined. The water in these trials frequently boiled pretty fast, but never very violently. It was in general heated over a portable black lead furnace placed in the middle of the room; but it was once heated over an ordinary chafing-dish, when the quicksilver in the long tube, held perpendicularly, was found to be 64° hotter than the air. When the experiments were tried without doors, the heat of the quicksilver in the tube would vary very much, according as the wind blew the steam and hot air from or towards the tube, but it sometimes rose as high as it did within doors.

The most convenient method we know of making these tubes without a ball, is, to fill a thermo-

To avoid the inconvenience of this correction, perhaps it may be thought that, both in adjusting the boiling point and in trying the heat of liquors, it would be better that not much more than the ball of the thermometer should be immersed, and that the tube should be held inclined in such manner as to be heated as little as possible; as it may be said, that by this means you will find the heat of liquors pretty nearly, without the trouble of making any correction; and that, though in strictness a correction would be required in observing the heat of the air with such thermometers, yet the heat of the atmosphere never differs so much from the mean heat, as to make that correction of much consequence.* But, on the other hand, this method of making and using thermometers is much less exact than the former, and therefore is unfit for nice experiments; and besides, a correction would be as necessary with this kind of thermometer in trying the heat of air, artificially heated, or in finding the heat of large quantities

meter in the usual manner, and heat the ball till there is a proper quantity of quicksilver in the tube, and then to make the column of quicksilver separate at the neck of the ball, and run to the extremity of the tube, so as to leave a vacuum between the ball and the column of quicksilver, as is expressed in fig. 12, where the shaded part *AD* represents the column of quicksilver, and *BA* that part in which there is a vacuum. The tube must then be sealed somewhere between *B* and *A*, as at *E*, and cut off there; after which it must be held with the end *D* upwards, so as to make the column of quicksilver run to the extremity *E*: by this method of filling, it is plain that no sensible quantity of air can be left between *E* and the column of quicksilver; but yet the quicksilver will be apt not to run sufficiently close to the extremity *E*, as the weight of the column will be scarcely sufficient to force it into the narrow space which will commonly be left in sealing the tube, especially when held nearly horizontal: for this reason it will be proper to open the tube at *D*, so as to let in the air, and then seal it again. It must be observed, that the space left between *D* and the column of quicksilver, ought not to be less than the 10th part of the length of the column of quicksilver; as otherwise the included air might be too much compressed by the expansion of the quicksilver when much heated.—Orig.

* The degrees on all thermometers are intended to answer to equal portions of the solid contents of the tube; and consequently, if the quicksilver in the tube is kept constantly of the same heat as that in the ball, the degrees will answer to equal increments of bulk of the whole quantity of quicksilver in the thermometer, that is of a given weight of quicksilver. But if only the quicksilver in the ball is heated, and that in the tube is kept always of the same heat, the degrees will answer to equal increments of a given bulk of quicksilver; so that the scale of the thermometers will be really different in these two methods of proceeding, and in high degrees the difference will be very considerable: for example, let two thermometers be made, and in the first of them let care be taken, both in adjusting the fixed points and in trying the heat of liquors, that the quicksilver in the tube shall be of the same heat as that in the ball; and in adjusting the fixed points of the 2d, and in trying the heat of liquors with it, let care be taken that the quicksilver in the tube shall remain always of the same invariable heat, and let the freezing and boiling points be marked 32 and 212 on both of them: then will the degree of 620 on the 1st answer to that of 600 on the 2d; that of 406 to 400; that of 302 to 300; and that of 119.7 to 120; that is, a liquor which appears to be of 620° of heat by the 1st, will appear to be of 600° by the 2d, &c. It hence appears, that it would be improper to employ the latter method of adjusting and using thermometers for ordinary purposes, and the former for nice experiments.—Orig.

of hot liquors, in which it would be difficult to prevent the quicksilver in the tube from being heated by the steam, as it is in finding the heat of liquors with the other thermometer, whenever the ball is not immersed to a sufficient depth; so that, on the whole, the former method of making and using thermometers seems much the best.

A much better way, of avoiding the trouble of making a correction, would be to have 2 sets of divisions made to such thermometers as are intended for trying the heat of liquors; one of which should be used when the tube is immersed almost to the top of the column of quicksilver; and the other, when not much more than the ball is immersed, in which last case the observer should be careful that the tube should be as little heated by the steam of the liquor as conveniently can be. It is difficult to give rules for constructing this 2d set of divisions, as the heat of the quicksilver in the tube will be very different according to the temper of the air in the room, the quantity and nature of the fluid whose heat is to be tried, the manner in which it is heated, and the other circumstances of the experiment; but, on the whole, we think that which is given in the following table would be as proper as any.

Degree answering to that point of the tube which is 2 inches above the ball.										
	+75	+50	+25	0	-50	-100	-200	-300	-400	-500
-500										-500
-400									-400	-396.5
-300								-300	-297.4	-294.8
-250								-248.9	-246.7	-244.6
-200							-200	-198.3	-196.5	-194.8
-150							-149.3	-148.0	-146.7	-145.4
-100						-100	-98.9	-97.7	-96.6	-95.5
-50					-50	-49.7	-49.0	-48.3	-47.6	-46.9
0				0	+0.2	+0.3	+0.6	+0.9	+1.2	+1.5
+150	149.5	149.4	149.0	148.7	148.4	147.3				
+200	198.8	198.5	198.3	198.0	197.5	197.0				
+250	247.5	247.1	246.8	246.4	245.7					
+300	295.8	295.3	294.9	294.4	293.4					
+350	343.7	343.1	342.5	342.0						
+400	391.1	390.4	389.7	389.1						
+450	438.1	437.3	436.5	435.7						
+500	484.7	483.8	482.9	482.0						
+600	576.5	575.4	574.3	573.2						

To make use of this table, seek in the uppermost horizontal line the degree of the thermometer answering to that point of the tube which is 2 inches above the ball; and in the left-hand column seek the degrees of the 2d set of divisions; the corresponding numbers in the table are the corresponding degrees of the 1st set, or the degrees which they must be set opposite to. The right-hand perpendicular column shows the heat which the quicksilver in the tube was supposed to be of in forming this table. Though this 2d set of divisions be far from accurate,

yet it is at least as much so as a thermometer adjusted in the latter method can be; so that this double set of divisions possesses all the advantages which can be expected from that method of adjusting thermometers, without the inconveniences.

A Table for correcting the observed Height of a Thermometer, whenever the Quicksilver in the Tube is not of the same Heat as that in the Ball.

Diff. of heat.	Degrees not immersed in the liquors.														
	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750
50	0.2	0.4	0.7	0.9	1.1	1.3	1.5	1.7	2.0	2.2	2.4	2.6	2.8	3.1	3.3
100	0.4	0.9	1.3	1.8	2.2	2.6	3.0	3.5	3.9	4.4	4.8	5.2	5.7	6.1	6.6
150	0.7	1.3	2.0	2.6	3.3	3.8	4.6	5.2	5.9	6.5	7.2	7.9	8.4	9.2	9.8
200	0.9	1.8	2.6	3.5	4.4	5.1	6.1	7.0	7.8	8.7	9.6	10.0	11.0	12.0	13.0
250	1.1	2.2	3.3	4.4	5.5	6.4	7.6	8.7	9.8	11.0	12.0	13.0	14.0	15.0	16.0
300	1.3	2.6	3.8	5.1	6.4	7.7	9.1	10.0	12.0	13.0	14.0	16.0	17.0	18.0	20.0
350	1.5	3.0	4.6	6.1	7.6	9.1	11.0	12.0	14.0	15.0	17.0	18.0	20.0	21.0	23.0
400	1.7	3.5	5.2	7.0	8.7	10.0	12.0	14.0	16.0	17.0	19.0	21.0	23.0	24.0	26.0
450	2.0	3.9	5.9	7.8	9.8	12.0	14.0	16.0	18.0	20.0	22.0	24.0	25.0	27.0	29.0
500	2.2	4.4	6.5	8.7	11.0	13.0	15.0	17.0	20.0	22.0	24.0	26.0	28.0	31.0	33.0
550	2.4	4.8	7.2	9.6	12.0	14.0	17.0	19.0	22.0	24.0	26.0	29.0	31.0	34.0	36.0

To make use of this table, in the left-hand perpendicular column look for the number of degrees contained in that part of the tube which is not immersed in the fluid whose heat is to be tried, and in the upper horizontal line seek the supposed difference of heat of the quicksilver in that part of the tube from that in the ball; the corresponding number in the table is the correction, which must be added to the observed heat when the quicksilver in the tube is cooler than that in the ball, and subtracted when it is warmer: for example, let the observed heat of the fluid be 475° , let the thermometer be immersed in the fluid as far as to the degree of 25° , or to that part of the tube which should be marked 25° if the divisions were continued long enough; then is the number of degrees in that part of the tube which is not immersed in the fluid 450; and let the heat of the quicksilver in that part of the tube be supposed 100° ; and consequently the difference of heat of the quicksilver in that part of the tube from that in the ball 375; then in the left-hand perpendicular column seek the number 450, and in the upper horizontal line the number 375; the corresponding number in the table, or the correction, is 15° , and therefore the true heat of the fluid is 490° .

This correction may be had very easily without the help of the table, only by multiplying the number of degrees not immersed in the fluid by the supposed difference of heat, dividing the product by 10000, and diminishing the quotient by $\frac{1}{8}$ part of the whole.

In the following pages we have thrown together the practical rules, which we would recommend to be observed in adjusting the fixed points of thermometers.

Rules to be observed in adjusting the boiling point.—The most accurate way of adjusting the boiling point is, not to dip the thermometer into the water, but to expose it only to the steam, in a vessel closed up in the manner represented in fig. 14, where ABba is the vessel containing the boiling water, dd the cover, E a chimney made in the cover intended to carry off the steam, and mm the thermometer passed through a hole in the cover. Those who would make use of this method must take care to attend to the following particulars.

1st. The boiling point must be adjusted when the barometer is at 29.8 inches; unless the operator is willing to correct the observed point in the manner directed below. 2dly. The ball of the thermometer must be placed at such a depth within the pot, that the boiling point shall rise very little above the cover, for otherwise part of the quicksilver in the tube will not be heated, and therefore the thermometer will not rise to its proper height. The surface of the water in the pot also should be at least an inch or two below the bottom of the ball; as otherwise the water, when boiling fast, might be apt to touch the ball: but it does not signify how much lower than that the surface of the water may be. 3dly. Care must be taken to stop up the hole in the cover through which the tube is inserted, and to make the cover fit pretty close, so that no air shall enter into the pot that way, and that not much steam may escape. A piece of thin flat tin plate must also be laid on the mouth of the chimney, so as to leave no more passage than what is sufficient to carry off the steam. The size of this plate should be not much more than sufficient to cover the chimney, that its weight may not be too great; and the mouth of the chimney should be made flat, that the plate may cover it more completely. It must be observed, that when the tin plate is laid on the mouth of the chimney, it will commonly be lifted up by the force of the steam, and will rattle till it has slipped aside sufficiently to let the steam escape without lifting it up. In this case it is not necessary to put the plate back again, unless by accident it has slipped aside more than usual. If the artist pleases, he may tie each corner of this plate by a string to prongs fixed to the chimney, and standing on a level with the plate, as thus it will necessarily be kept always in its place;* but we would by no means recommend having it made with a hinge, as that might be apt to make it stick, in which case the included vapour might be so much compressed as to cause an error. We would also by no means advise lining the tin plate with leather, or any other soft substance, for the sake of making it shut closer, as that also might be apt to make it stick. The chimney also ought not to be made less than half a square inch in area; for though a

* Fig. 13 is a perspective view of the chimney and tin plate; ABCD is the plate; E the chimney; ff, gg, mm, and nn, the prongs fastened to the chimney, to which the 4 corners of the plate are to be tied by the strings AF, BG, CM, and DN; the ends F, G, M, and N, of the prongs must be on a level with the plate, and the strings should not be stretched tight.—Orig.

smaller chimney would be sufficient to carry off the steam, unless the vessel is much larger than what we used; yet the adhesion which is apt to take place between it and the tin plate when wet, might perhaps bear too great a proportion to the power which the included steam has to lift it off, if it was made much less. It is convenient that the chimney be not less than 2 or 3 inches long, as thus the observer will be less incommoded by the steam; but it would be improper to make it much longer, for the longer the chimney is, the greater disposition has the air to enter into the pot between it and the cover.

It is most convenient not to make the cover fit on tight, but to take on and off easily; and to wrap some spun cotton round that part of the cover which enters into the pot, in order to make it shut closer; or, what seems to answer rather better, a ring of woollen cloth may be placed under the cover, so as to lie between the top of the pot and it. These methods of making the cover shut close can be used more conveniently when the cover is made to enter within the pot, as in the figure, than when it goes on on the outside.

There are various easy ways by which the hole in the cover, through which the tube of the thermometer is passed, may be stopped up, and by which the thermometer may be suspended at the proper height. The hole in the cover may be stopped up by a cork, which must first have a hole bored through it, large enough to receive the tube, and be then cut into two, parallel to the length of the hole. Another method, more convenient in use, but not so easily made, is represented in fig. 16, which exhibits a perspective view of the apparatus; *aa* is the cover; *h* the hole through which the thermometer is passed; *bb* a flat piece of brass fixed on the cover; and *dd^{ee}* a sliding piece of brass, made so as either to cover the hole *h*, or to leave it uncovered as in the figure, and to be tightened in either position by the screw *s* sliding in the slit *mm*; a semi-circular notch being made in the edge *bb*, and also in the edge *dd*, to inclose the tube of the thermometer: pieces of woollen cloth should also be fastened to the edges *bb* and *dd*, and also to the bottom of the sliding piece *dd^{ee}*, unless that piece and the cover are made sufficiently flat, to prevent the escape of the steam. In order to keep the thermometer suspended at the proper height, a clip may be used like that represented in fig. 17, which by the screw *s* must be made to embrace the tube tightly, and may rest on the cover. That part of the clip which is intended to bear against the tube, had best be lined with woollen cloth, which will make it stick tighter to the tube, and with less danger of breaking it. Another method, which is rather more convenient, when the top of the tube of the thermometer is bent into a right angle, in the manner frequently practised at present for the sake of more conveniently fixing it to the scale, is represented in the same figure 16; *gg^{ff}* is a plate of brass, standing perpendicularly on the cover, and *llⁿⁿ* a piece of brass, bent at bottom into the form of a loop, with a

notch in it, so as to receive the tube of the thermometer, and to suffer the bent part to rest on the bottom of the loop; this piece must slide in a slit *kk*, cut in the plate *llnn*, and be tightened at any height by the screw *r*.

4thly. It is best making the water boil pretty briskly, as otherwise the thermometer is apt to be a great while before it acquires its full heat, especially if the vessel is very deep. The observer too should wait at least a minute or 2 after the thermometer appears to be stationary, before he concludes that it has acquired its full height.

5thly. Though this appears to be the most accurate way of adjusting the boiling point; yet, if the operator was to suffer the air to have any access to the inside of the vessel, he would be liable to a very great error: for this reason we strongly recommend it to all those who use this method, not to deviate at all from the rules laid down without assuring themselves, by repeated trials with a pretty sensible thermometer, that such alteration may be used with safety. But the covering the chimney with the tin plate ought by no means to be omitted; for though, if the cover of the pot fits close, it seldom signifies whether the plate is laid on or not, yet, if by accident the cover was not to fit close, the omitting the tin plate would make a very great error. Making the chimney very narrow would not answer the end properly; for if it was made so small as to make the vessel sufficiently close when the water boiled gently, it would not leave sufficient passage for the escape of the steam when it boiled fast.

Another way of adjusting the boiling point is, to try it in a vessel of the same kind as the former, only with the water rising a little way, namely from 1 to 3 or 4 inches above the ball, taking care that the boiling point shall rise very little above the cover, as in the former method. In this way there is no need to cover the chimney with the tin plate; and there is less need to make the cover fit close, only it must be observed, that the closer the cover fits, the less the operator will be incommoded by the steam. The height of the barometer at which the boiling point should be adjusted, when this method is used, is $29\frac{1}{4}$ inches, or $\frac{3}{10}$ of an inch less than when the former method is used.

It will be convenient to have 2 or 3 pots of different depths; for if a short thermometer is to be adjusted in the same pot which is used for a long one, it will require a great depth of water, which, besides taking up more time before it boils, makes the observation rather less accurate, as the heat seems to be less regular when the depth of water in the pot is very great, than when it is less. Perhaps some persons, for the sake of heating the water more expeditiously, may be inclined to use an apparatus of such kind, that the fire shall be applied to a considerable part of the sides of the pot, as well as to the bottom; we would however caution them against any thing of that kind, as the observations are considerably less regular than when little more than the bottom of the pot is

heated. If the pot is heated over a chafing-dish or common fire, we apprehend that there can seldom be any danger of too much of the sides being heated; but if the operator should be apprehensive that there is, it is easily prevented by fastening an iron ring an inch or 2 broad round the pot near the bottom. This precaution is equally necessary when the thermometer is adjusted in steam, especially when there is not much water in the pot. The greatest inconvenience of this method of adjusting the boiling point, is the trouble of keeping a proper depth of water in the pot; as to do this, it is necessary first to find the height of the boiling point coarsely by trying it in an open vessel, and then to put such a quantity of water into the pot, that it shall rise from 1 to 3 or 4 inches above the ball, when the thermometer is placed at such a depth within the pot, that the boiling point shall rise very little above the cover. The operator must be very careful that the quantity of water in the pot be not so small as not entirely to cover the ball.

A 3d way of adjusting the boiling point, is, to wrap several folds of linen rags or flannel round the tube of the thermometer, and to try it in an open vessel, taking care to pour boiling water on the rags, in order to keep the quick-silver in the tube as nearly of the heat of boiling water as possible. The best way is to pour boiling water on the rags 3 or 4 times, waiting a few seconds between each time, and to wait some seconds after the last time of pouring on water before the boiling point is marked, in order that the water may recover its full strength of boiling, which is in good measure checked by pouring on the boiling water. In this method

the boiling point should be adjusted when the barometer is at 29.8 inches, that is, the same as when the 1st method is used; the water should boil fast, and the thermometer should be held upright, with its ball 2 or 3 inches under water, and in that part of the vessel where the current of water ascends.*

Whichever of these methods of adjusting the boiling point is used, it is not necessary to wait till the barometer is at the proper height, provided the operator will take care to correct the observed height according to the above table.

Height of the barometer when the boiling point is adjusted according to,			Correction in 1000ths of the interval between 32° and 212°.		
1st or 3d method.	2d method.		1st or 3d method.	2d method.	
	30.64	10	29.69	29.39	1
	53	9	58	28	2
30.71	41	8	47	17	3
59	29	7	36	06	4
48	18	6	25	28.95	5
37	07	5	14	84	6
25	29.95	4	03	73	7
14	84	3	28.92	62	8
03	73	2	81	51	9
29.91	61	1	70	—	10
80	50	0	59	—	11

lower.

higher.

* In a vessel of boiling water one may almost always perceive the current of water to ascend on one side of the vessel, and to descend on the other.—Orig.

To make use of this table, seek the height which the barometer is found to stand at in the left-hand column, if the boiling point is adjusted either in the 1st or 3d method, and in the 2d column if it is adjusted in the 2d method; the corresponding number in the 3d column shows how much the point of 212° must be placed above or below the observed point, expressed in thousandth parts of the interval between the boiling and freezing point: for example, suppose the boiling point is adjusted in steam when the barometer is at 29 inches, and that the interval between the boiling and freezing points is 11 inches; the nearest number to 29 in the left-hand column is 29.03, and the corresponding number in the table is 7 higher, and therefore the mark of 212° must be placed higher than the observed point by $\frac{7}{1000}$ of the interval between boiling and freezing, that is, by $\frac{11 \times 7}{1000}$, or .077 of an inch. This method of correcting the boiling point is not strictly just, unless the tube is of an equal bore in all its parts: but the tube is very seldom so much unequal as to cause any sensible error, where the whole correction is so small. The trouble of making the correction will be abridged by making a diagonal scale such as is represented in fig. 15. It is not very material what kind of water is used for adjusting the boiling point, so that it is not at all salt; only, if any kind of hard water is used, it is better that it should be kept boiling for at least 10 minutes before it is used. But we would advise all those desirous of adjusting thermometers in the most accurate manner for nice experiments, to employ rain or distilled water, and to perform the operation in the first mentioned manner, that is, in steam.

On the freezing point.—In adjusting the freezing, as well as the boiling point, the quicksilver in the tube ought to be kept of the same heat as that in the ball. In the generality of thermometers indeed, the distance of the freezing point from the ball is so small, that the greatest error which can arise from neglecting this precaution, is not very considerable, unless the weather is warmer than usual; but as the freezing point is frequently placed at a considerable distance from the ball, the operator should always be careful either to pile the pounded ice to such a height above the ball, that the error, which can arise from the quicksilver in the remaining part of the tube not being heated equally with that in the ball, shall be very small; or he must correct the observed point, on that account, according to the annexed table:

Heat of the air.	Correction.
42°	.00087
52	.00174
62	.00261
72	.00348
82	.00435

The 1st column of this table is the heat of the air, and the 2d is the correction expressed in 1000th parts of the distance between the freezing point and the surface of the ice: for example, if the freezing point stands 7 inches above the

surface of the ice, and the heat of the room is 62, the point of 32° should be placed $7 \times .00261$, or .018 of an inch lower than the observed point. This correction also would be made more easy by the help of a diagonal scale, similar to that proposed for the boiling point.

On the precautions necessary to be observed in making observations with thermometers.—In trying the heat of liquors, care should be taken that the quick-silver in the tube of the thermometer be heated to the same degree as that in the ball; or, if this cannot be done conveniently, the observed heat should be corrected on that account: but for this we refer to the former part.

H. Cavendish; W. Heberden; Alex. Aubert; J. A. De Luc; N. Maskelyne; S. Horsley; J. Planta.

END OF THE SIXTY-SEVENTH VOLUME OF THE ORIGINAL.

I. On certain Traces of Volcanos on the Banks of the Rhine. By Sir Wm. Hamilton, K. B., F. R. S. Vol. LXVIII. Anno 1778. p. 1.

The first certain token of volcanos having existed in this country was evident to me in the court of the palace of the elector palatine at Dusseldorff, which is at this moment new paving with a lava exactly like that of Etna and Vesuvius, which comes from a quarry belonging to the same elector at Unkel, between Bonn and Coblenz. At Cologne, I was struck with the sight of numberless basaltic columns inserted in the walls of the town; and columns of the same sort are universally used as posts in the streets, and at every door; they are chiefly pentagonal, but some are hexagonal, and a few have only 4 sides; they are very like the basaltes of the Giants Causeway, but without their regular articulations. These come likewise from the Unkel quarry. The walls of most of the ancient buildings in the town of Cologne are of a tuffa exactly resembling that of Naples and its environs. This species of stone abounds on the banks of the Rhine, between Bonn and Coblenz: these circumstances made me keep a sharp look out, and, on my approach to Bonn, I was struck with the volcanic forms of the Sevenbergen, or Seven Mountains, about 2 leagues from the town, on the other side of the Rhine. In the walls and streets of Bonn are many of the abovementioned columns of basaltes, and the pavement of the town is of lava. The stone in general use for building here, is a very compact one, a hard volcanic tuffa like that of Pianura near Naples, and of the sort called Piperno in Italy; it is something like free-stone, but, on near inspection, is mixed with fragments of lava and other volcanic substances.

I visited Wolckenberg, Tackenfelts, and Stromberg, 3 of the Sevenbergen, and found the first 2 entirely composed of tuffa, and the last of tuffa and lava:

and, by the shape and appearance of the rest of these mountains, doubtless they are all equally composed of the same volcanic substances. The craters on the mountains are discernible, though much altered, and filled up by time and the rubbish thrown from the quarries that are constantly worked on their tops. On each side of the Rhine, most of the way from Bonn to Coblenz, particularly between Prohl and Andernach, are high rocks of lava or tuffa. Where the volcanos had not operated, the mountains and rocks are of slate. At Erpel, in a mountain close to the river, and opposite the convent situated on an island about 3 leagues from Bonn, there are some traces of basaltic columns, the quarry seeming to have been nearly exhausted. I have often thought (and this exhausted quarry brings it to my mind again) that the reason why there are scarcely any remains of lavas that have taken the columnal form on Vesuvius and the volcanos near Naples is, that they have been carried off for the use of paving the great Roman roads. The Appian way is mostly composed of lava of a pentagonal and hexagonal form, and seems evidently made of pieces of such basaltic columns. These lavas being ready cut by nature, would naturally be carried off first, as the cutting of solid rocks of lava for such purposes is attended with very great expence.

At Unkel, above a league farther on towards Coblenz, just opposite the town on the other side of the Rhine, is the great quarry belonging to the elector-palatine, which affords a most pleasing and uncommon sight: it is entirely composed of the most regular detached basaltic columns, and though millions of these columns have been extracted, as the towns of Cologne and Bonn testify, yet the quarry is very rich. They lie mostly in an horizontal direction, but some are perpendicular, and others inclining towards the Rhine, which, being very low, shows many of them in the bed of the river itself; they rise from thence into the mountain, where is the present quarry, above 100 feet. They are chiefly pentagonal; the smallest are in general the most distinct and regular, about 6 inches diameter; the largest of the columns were about 3 feet long, and about $1\frac{1}{2}$ foot in diameter. The other lavas in this neighbourhood are of the same substance, and some incline to the same forms, but none so regular. I have not the least doubt but that all basaltes, wherever they exist, have originated subterraneous fire, and are true lavas.

At Andernach, between Bonn and Coblenz, I saw vast heaps of tuffa ready cut, lying on the banks of the Rhine, and some Dutch vessels loading it; on inquiry I found, that a considerable trade of this material is carried on between this town and Holland, where they grind down this sort of stone by wind-mills into a powder, which they use as a puzzolane for all their buildings under water. This also corresponds with an idea mentioned in one of my former letters to the R. S., that the tuffas of Naples were composed of a puzzolane, prepared by vol-

nic fire deep in the bowels of the earth, and, mixing with water at the time of its explosion, formed a sort of natural mortar or cement. The Dutch reduce it again to its pristine state of puzzolane.

II. Of the Heat, &c. of Animals and Vegetables. By Mr. J. Hunter, F.R.S. p. 7.*

As I had formerly, (says Mr. H.) in making experiments on animals, relative to heat and cold, made similar ones on vegetables, and had generally found a great similarity between them in these respects, I was led to pursue the subject on the same plan; but I was still further induced to continue my experiments on vegetables, as there seemed to be a material difference between them in their power of supporting cold. From observations and the foregoing experiments it plainly appears, that the living principle will not allow the heat of such animals to sink much lower than the freezing point, though the surrounding atmosphere be much colder, and that in such a state they cannot support life long; but it may be observed, that most vegetables of every country can sustain the cold of their climate. In very cold regions, as in the more northern parts of America, where the thermometer is often 50° below 0, where people's feet are known to freeze and their noses to drop off if great care be not taken, yet the spruce-fir, birch, juniper, &c. are not affected.

Yet that vegetables can be affected by cold, daily experience evinces; for the vegetables of every country are affected if the season be more than ordinarily cold for that country, and some more than others; for in the cold climates abovementioned, the life of the vegetable is often obliged to give way to the cold of the country: a tree shall die by the cold, then freeze and split into a great number of pieces, and in so doing produce considerable noise, giving loud cracks which are often heard at a great distance. In this country the same thing sometimes happens to exotics from warmer climates: a remarkable instance of this kind happened this winter in his Majesty's garden at Kew. The *Erica arbores* or tree-heath, a native of Spain and Portugal, which had kept its health extremely well against a garden wall for 4 or 5 years, though covered with a mat, was killed by the cold, and then being frozen split into innumerable pieces.† But the question is, is every tree dead that is frozen? I can only say, that in all the experiments I ever made on trees and shrubs, whether in the growing or

* Of this ingenious paper the latter part only, which relates to the heat of vegetables, is retained in these Abridgments; the former part, which contains the experiments on animals, being reprinted in Mr. H.'s *Observations on the Animal Œconomy*.

† This must be owing to the sap in the tree freezing, and occupying a larger space when frozen than in a fluid state, similar to water; and that there is a sufficient quantity of sap in a tree newly killed is proved by the vast quantity which flows out upon wounding a tree. But what appeared most remarkable was, that in a walnut-tree, on which I made many of my experiments, I observed that more sap issued out in the winter than in the summer. In the summer, a hole being bored, scarcely any came out; but in the winter it flowed out abundantly.—Orig.

active state, or in the passive, that the whole or part which was frozen, was dead when thawed.

The winter 1775-6 afforded a very favourable opportunity for making experiments relative to cold, which I carefully availed myself of: though, previous to that winter, I had made many experiments on vegetables respecting their temperature comparatively with that of the atmosphere, and when they were in their different states of activity. I therefore examined them in different seasons, with a view to see what power vegetables have. I shall relate these experiments in the order in which they were made. They were begun in the spring, the actions of life on which growth depends being then on the increase; and they were continued till those actions were on the decline, and also when all actions were at an end, but while the passive powers of life were still retained.

The first were made on a walnut-tree, 9 feet high in the stem, and 7 feet in circumference in the middle. A hole was bored into it on the north side, 5 feet above the surface of the ground, 11 inches deep towards the centre of the tree, but obliquely upwards, to allow any sap, which might ooze through the wounded surface, to run out. I then fitted to this part a box about 8 inches wide and 5 deep, and fastened it to the tree: the bottom of the box opened like a door with a hinge. I stuffed the box with wool, excepting the middle, opposite to the hole in the tree: for this part I had a plug of wool to stuff in, which, when the door was shut, inclosed the whole. The intention of this was to keep off as much as possible all immediate external influence either of heat or cold.

The same thermometer with which I made my former experiments, $7\frac{1}{2}$ inches long, was sunk into a long feather of a peacock's tail, with a slit on one side to show the degrees; by this means the ball of the thermometer could be introduced into the bottom of the hole.

Exp. 1. March 29th, I began my experiments at 6 in the morning, the atmosphere at $57^{\circ}\frac{1}{2}$, the thermometer in the tree at 55° ; when it was withdrawn the quicksilver sunk to 53° , but soon rose to $57^{\circ}\frac{1}{2}$.* This experiment was repeated 13 times with the same success. Here the tree was cooler than the atmosphere; when one should rather have expected to have found it warmer, since it could not be supposed to have as yet lost its former day's heat.—*Exp. 2.* April 4th, half past 5 in the evening, the tree at 56° , the atmosphere at 62° ; the tree therefore still cooler than the atmosphere.—*Exp. 3.* April 5th, wind in the north, a coldish day, 6 o'clock in the evening, the thermometer in the tree was at 55° , the atmosphere at 47° ; the tree warmer than the atmosphere.—*Exp. 4.* April 7th, a cold day, wind in the north, cloudy, at 3 o'clock in the afternoon, the thermometer in the tree was at 42° , the atmosphere at 42° also.—*Exp. 5.*

* The sinking of the quicksilver upon being withdrawn I imputed to the evaporating of the moisture of the fluid on the ball.—Orig.

April 9th, a cold day, with snow, hail, and wind, in the north-east; at 6 in the evening the thermometer in the tree at 45° , the atmosphere at 39° . Here the tree was warmer than the atmosphere, just as might have been expected. If these experiments prove any thing, it is that there is no standard; and probably these variations arose from some circumstance which had no immediate connection with the internal powers of the tree; but it may also be supposed to have arisen from a power in the tree to produce or diminish heat, as some of them were in opposition to the atmosphere.

After having endeavoured to find out the comparative heat between vegetables and the atmosphere, when the vegetables were in action; I next made my experiments on them when they were in the passive life. As the difference was very little when in their most active state, I could expect but very little when the powers of the plant were at rest. From experiment on the more imperfect classes of animals it plainly appears, that though they do not resist the effects of extreme cold till they are brought to the freezing point, they then appear to have the power of resisting it, and of not allowing their cold to be brought much lower. To see how far vegetables are similar to those animals in this respect, I made several experiments: I however suspected them not to be similar, because such animals will die in a cold in which vegetables live; I therefore supposed that there is some other principle. I did not confine these experiments to the walnut-tree, but made similar ones on several trees of different kinds, as pines, yews, poplars, &c. to see what was the difference in different kinds of trees. The difference proved not to be great, not above a degree or 2: however, this difference, though small, shows a principle in life, all other things being equal; for as the same experiments were made on a dead tree, which stood with its roots in the ground, similar to the living ones, they became more conclusive.

In October I began the experiments upon the walnut-tree, when its powers of action were on the decline, and when it was going into its passive life.

Exp. 6. October 18th, at half past 6 in the morning, the atmosphere at $51^{\circ}\frac{1}{2}$, the thermometer in the tree was at $55^{\circ}\frac{1}{2}$; but, on withdrawing and exposing it for a few minutes in the common atmosphere, it fell to $50^{\circ}\frac{1}{2}$.—*Exp.* 7. October 21st, 7 in the morning, the atmosphere at 41° , the tree at 47° .—*Exp.* 8. October 21st, in the evening at 5 o'clock, the atmosphere at $51^{\circ}\frac{1}{2}$, the tree at 57° .—*Exp.* 9. October 22d, at 7 in the morning, the atmosphere at 42° , the tree at 48° .—*Exp.* 10. October 22d, 1 o'clock afternoon, the atmosphere at 51° , the tree at 53° .—*Exp.* 11. October 23d, in the evening of a wet day, the atmosphere at 46° , the tree at 48° .—*Exp.* 12. October 28, a dry day, the atmosphere at 45° , the tree at 46° .—*Exp.* 13. October 29th, a fine day, the atmosphere at 45° , the tree at 49° .—*Exp.* 14. November 2d, wind east, the atmosphere at 43° , the tree at 43° .—*Exp.* 15. November 5th, wet day, the at-

mosphere at 43° , the tree at 45° .—*Exp.* 16. Nov. 10th, atmosphere at 49° , the tree at 55° .—*Exp.* 17. Nov. 18th, atmosphere at 42° , the tree at 44° .—*Exp.* 18. Nov. 20th, fine day, the atmosphere at 40° , the tree at 42° .—*Exp.* 19. Dec. 2d, the atmosphere at 54° , the tree at 54° .

In all these experiments, which were made at very different times in the day, viz. in the morning, at noon, and in the evening, the tree was in some degree warmer than the atmosphere, excepting in one, when their temperatures were equal. For the sake of brevity I have drawn up my other experiments, which were made on different trees, into 4 tables, as they were made at 4 different degrees of heat of the atmosphere, including those made in the time of the very hard frost in the winter of 1775-6. They were as follows :

1st. The Atmosphere at 29° .

Names.	Height. Ft. In.	Diam. In.	Heat.	Names.	Height. Ft. In.	Diam. Ft. In.	Heat.
Carol. poplar	2	2	$29\frac{1}{2}^{\circ}$	Cedar libanon	2 2	0 $4\frac{1}{2}$	$28\frac{1}{2}$
Engl. poplar	4	$2\frac{1}{4}$	$29\frac{1}{2}$	Arbutus	2 6	0 $3\frac{1}{2}$	30
Orien. plane	3	$1\frac{1}{4}$	30	Arbor vitæ	2 8	0 $3\frac{1}{2}$	29
Occid. plane	3 6	2	30	Dissid. cyprus.	3	0 $2\frac{1}{2}$	30
Carol. plane	1	$1\frac{3}{4}$	30	Lacker varnish	3 6	0 2	30
Birch	3 6	$2\frac{1}{2}$	$29\frac{1}{2}$	Walnut tree	5	2 4	31
Scotch fir	3 6	4	$28\frac{1}{2}$				

The old hole in the walnut-tree being full of sap was frozen up, but a fresh one was made.

2d. The Atmosphere at 27° .

Names.	Height. Ft. In.	Diam. In.	Heat.
Spruce fir	4	$2\frac{1}{2}$	32°
Scotch fir	1 $5\frac{1}{2}$	$1\frac{1}{2}$	28
Silver fir	3 11	$2\frac{1}{2}$	30
Weymouth fir	4 6	$2\frac{1}{2}$	30
Yew	3 7	3	30
Holly	2 6	2	30
Plumb-tree	4 6	3	$31\frac{1}{2}$
Dead cedar	3 11	3	29
Ground under snow	3 deep—		$3\frac{1}{4}$

3d. The atmosphere at 24° .

Names.	Heat.
Spruce fir	23°
Scotch fir	23
Silver fir	23
Weymouth fir	23
Yew	22
Holly	23
Dead cedar	24

The same trees we mentioned when the thermometer was at 29° , in new holes made at the same height, and left some time pegged up till the heat produced by the gimlet was gone off; but in which, as they were moist from the sap, the heat could be very little, especially as the gimlet was not in the least heated by the operation.

4th: Atmosphere 16° .

Car. poplar	17°	Carol. plane	17°
Eng. poplar	17	Birch	17
Ori. plane	17	Scotch fir	$16\frac{1}{2}$
Occ. plane	17		

The sap of the walnut-tree, which flowed out in great quantity, froze at 32° .

I did not try to freeze the sap of the others. Now, since the sap of a tree, when taken out, freezes at 32° ; also, since the sap of a tree, when taken out of its proper canals, freezes when the heat of the tree is at 31° ; and since the heat of the tree can be so low as 17° without freezing; by what power are the juices of the tree, when in their proper canals, kept fluid in such a cold? Is it the principle of vegetation? Or is the sap inclosed in such a way as that the process of freezing cannot take place, which we find to be the case when water is confined in globular vessels? If so, its confinement must be very different from the confinement of the moisture in dead vegetables; but the circumstance of vegetables dying with the cold, and then freezing, appears to answer the last question. These, however, are questions which at present I shall not endeavour to solve. I have made several experiments on the seeds of vegetables similar to those on the eggs of animals; but, as inserting them would draw out this paper to too great a length, I will reserve them for another.

III. **The Force of Fired Gunpowder, and the Initial Velocities of Cannon Balls, determined by Experiments; from which is also deduced the Relation of the Initial Velocity to the Weight of the Shot and the Quantity of Powder.* By Mr. Charles Hutton, F. R. S., of the Military Academy at Woolwich. p. 50.

These experiments were made at Woolwich in the summer of the year 1775, in conjunction with several able officers of the royal artillery at that place, and other ingenious gentlemen. The object of them was the determination of the actual velocities with which balls are impelled from given pieces of cannon, when fired with given charges of powder. These experiments were made according to the method invented by Mr. Robins, and described in his treatise, entitled, *New Principles of Gunnery*, of which an account was printed in the *Philos. Trans.* for the year 1743. Before the discoveries of that gentleman, very little progress had been made in the true theory of military projectiles. His book, however, contained such important discoveries, that it was soon translated into several of the languages on the continent, and the celebrated Euler honoured it with a very extensive commentary, in his translation of it into the German language. That part of it has always been particularly admired which relates to the experimental method of ascertaining the actual velocities of shot, and in imitation of which were made the experiments related in this paper. Experiments in the

* This paper was honoured with the Royal Society's prize medal: the public delivery of which to the author, and the pronouncing of an excellent appropriate oration on that occasion, on the 30th of Nov., 1778, by Sir John Pringle, was the last act of this gentleman in his capacity of president of the R. S.—He was immediately succeeded in that office by Sir Joseph Banks.

The paper itself contains the first part of a series of artillery experiments, then projected. A further extension of them, as conducted in several years, was given in Dr. Hutton's *Tracts*, printed in 1786; and a far more important part still remains unpublished in that author's possession.

manner of Mr. Robins were generally repeated by his commentators and others, with universal satisfaction, the method being so just in theory, so simple in practice, and altogether so ingenious, that it immediately gave the fullest conviction of its excellence, and of the abilities of its author. The use which that gentleman made of this invention was, to obtain the actual velocities of bullets experimentally, in order to compare them with those which he computed a priori from his new theory, and thus to verify the principles on which it is founded. The success was fully answerable to his expectations, and left no doubt of the truth of his theory, when applied to such pieces and bullets as he had used. But these were very small, being only musket balls of about 1 ounce weight; for, on account of the great size of the machinery necessary for such experiments, Mr. Robins and other ingenious gentlemen had not ventured to extend their practice beyond bullets of that kind, but satisfied themselves with earnestly wishing for experiments made in a similar manner with balls of a larger sort. By the experiments in this paper I have endeavoured, in some degree, to supply this defect, having made them with small cannon balls of above 20 times the size, or from 1 lb. to near 3 lb. weight. These are the only experiments that I know of which have been made with cannon balls for this purpose, though the conclusions to be deduced from such are of the greatest importance to those parts of natural philosophy which are dependent on the effects of fired gunpowder; nor do I know of any other practical method of ascertaining the initial velocities of military projectiles within any tolerable degree of the truth. The knowledge of this velocity is of the utmost consequence in gunnery: by means of it, together with the law of the resistance of the medium, every thing is determinable relative to that business; for, besides its being an excellent method of trying the strength of different sorts of powder, it gives us the law relative to the different quantities of powder, to the different weights of shot, and to the different lengths and sizes of guns. Besides these, there does not seem to be any thing wanting to determine every inquiry that can be made concerning the flight and ranges of shot, except the effects arising from the resistance of the medium.

Of the Nature of the Experiment, and of the Machinery used in it.—The intention of the experiment is to discover the actual velocity with which a ball issues from a piece, in the usual practice of artillery. This velocity is very great; from 1000 to 2000 feet in a second of time. For conveniently estimating so great a velocity, the first thing necessary is to reduce it, in some known proportion, to a small one. This we may conceive to be effected thus: suppose the ball, with a great velocity, to strike some very heavy body, as a large block of wood, from which it will not rebound, so that they may proceed forward together after the stroke. By this means it is obvious, that the original velo-

city of the ball may be reduced in any proportion, or to any slow velocity, which may conveniently be measured, by making the body struck to be sufficiently large; for it is well known, that the common velocity, with which the ball and block of blood would move forward after the stroke, bears to the original velocity of the ball only, the same ratio which the weight of the ball has to that of the ball and block together. Thus then velocities of 1000 feet in a second are easily reduced to those of 2 or 3 feet only; which small velocity being measured by any convenient means, then the number denoting it being increased in the proportion of the weight of the ball to the weight of the ball and block together, the original velocity of the ball itself will thus be obtained. In these experiments, this reduced velocity is rendered easy to be measured by a very simple and curious contrivance, which is this: the block of wood, which is struck by the ball, is not left at liberty to move straight forward in the direction of the motion of the ball, but it is suspended, as the weight or ball of a pendulum, by a strong iron stem, having a horizontal axis at top, on the ends of which it vibrates freely when struck by the ball. The consequence of this simple contrivance is evident: this large ballistic pendulum, after being struck by the ball, will be penetrated by it to a small depth, and it will then swing round its axis describing an arch, which will be greater or less according to the force of the blow struck; and from the size of the arch described by the vibrating pendulum, the velocity of any point of the pendulum itself can be easily computed; for a body acquires the same velocity by falling from the same height, whether it descends perpendicularly down, or otherwise; therefore, the length of the arch described, and of its radius, being given, its versed sine becomes known, which is the height perpendicularly descended by the corresponding point of the pendulum. The height descended being thus known, the velocity acquired in falling through that height becomes known, from the common rules for the descent of bodies by the force of gravity; and this is the velocity of that point of the pendulum: this velocity of any known point whatever is then to be reduced to the velocity at the centre of oscillation, by the proportion of their radii or distances from the axis of motion; and the velocity of this centre, thus obtained, is to be esteemed the velocity of the whole pendulum itself; which being now given, that of the ball before the stroke becomes known from the given weights of the ball and pendulum. Thus then the mensuration of the very great velocity of the ball is reduced to the observation of the magnitude of the arch described by the pendulum, in consequence of the blow struck. This arch may be measured after various ways: in the following experiments it was ascertained by measuring the length of its chord by means of a piece of tape or small ribband, one end of which was fastened to the bottom of the pendulum, and the rest of it made to slide through a small machine contrived for the purpose hereafter described; for

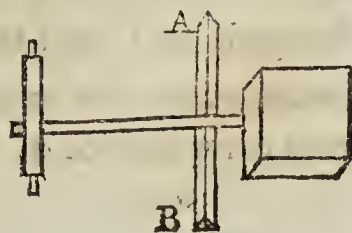
thus the length of the tape drawn out, was equal to the length of the chord of the arch described by the bottom of the pendulum.

This description may convey a general idea of the nature and principle of the experiment; but besides the centre of oscillation, and the weights of the ball and pendulum, the effect of the blow depends also on the place of the centre of gravity and the point of impact: it will therefore be now necessary to give a more particular description of the machine, and of the methods of finding the above-mentioned requisites, and then investigate our general rule for determining the velocity of the ball, in all cases, from them and the chord of the arch of vibration.

Of the Particular Description of the Machine, and of the Determination of the Centres of Gravity and Oscillation.—Plate 4, fig. 1, is a representation of the machine used in these experiments; the first pendulum consisted of a block of sound and dry elm, being nearly a cube of 20 inches long, which was fastened to a strong iron stem on the back part by screw-bolts, having a thick iron axis at the top, the ends being turned truly cylindrical, to roll pretty freely in sockets made to receive them; the whole being supported by a four-legged stand of very strong timber, firmly fixed in the ground. A is the face of the cube into which the balls were fired; by means of the blow it is made to swing round the axis BC, and the chord of the arch described is measured by the tape DEF fastened to the bottom of the wood at D, and sliding with some slight friction through a small machine, fixed at E for that purpose, the tape being marked with inches and tenths, for more easily measuring the chord or part of it drawn through by the pendulum. The whole length of this pendulum, from the middle of the axis to the ribband at D, was $102\frac{1}{2}$ inches. The weight and the other dimensions were taken each day when the experiments were made, and then registered; and the manner of discovering the places of the centres of gravity and oscillation was as follows.

To find the centre of oscillation, the pendulum was hung up, and made to vibrate in small arcs, and the time of making 2 or 300 vibrations was observed by a half-second pendulum. Having thus obtained the time answering to a certain number of vibrations, the finding of the centre of oscillation is easy: for if v denote the number of vibrations made in s seconds, then it is well known that $vv : ss :: 39.2 : \frac{39.2ss}{vv} =$ the distance in inches from the axis of motion to the centre of oscillation; and by this rule the place of that centre was found for each day.

The centre of gravity was ascertained by one or both of the 2 following methods. First, a triangular prism of iron AB, being placed on the ground with an edge upwards, the pendulum was laid across it, and moved forward or backward on the stem or block as the case



required, till the 2 ends exactly balanced each other; then, as it lay, the distance was measured from the middle of the axis to the part which rested on the edge of the prism, or the centre of gravity of the pendulum. The other method was as in the latter of the 2 annexed figures, where the ends of the axis being supported on fixed uprights, and a chord fastened to the lower end of the pendulum, was passed over a pulley at P, different weights w were fastened to the other end of it, till the pendulum was brought to a horizontal position. Then, taking also the whole weight of the pendulum, and its length from the axis to the bottom where the chord was fixed, the place of the centre of gravity is found by this proportion, as p the weight of the pendulum: w the appended weight :: d the whole length from the axis to the bottom: $\frac{dw}{p}$ = the distance from the axis to the centre of gravity. Either of these 2 methods gave the place of the centre of gravity sufficiently exact; but the coincidence of the results of both of them was still more satisfactory.



Of the Rule for Computing the Velocity of the Ball.—Having described the methods of obtaining the necessary dimensions, we proceed now to the investigation of the theorem by which the velocity of the ball is to be computed. The several weights and measures being found, let then b denote the weight of the ball, p the whole weight of the pendulum, g the distance of the centre of gravity below the axis, h the distance of the centre of oscillation, k the distance to the point struck by the ball, z the velocity of this point struck after the blow, v the original velocity of the ball, c the chord of the arch measured by the tape, and r its radius, or the distance from the axis to the bottom of the pendulum. Then the effect of the blow struck by the ball is as $\frac{gh}{kk}$; or $hh : gh :: p : \frac{ghp}{kk}$ = the weight of a body, which, being placed at the point struck, would acquire the same velocity from the blow, as the pendulum does at the same point. Here then are 2 bodies, b and $\frac{ghp}{kk}$, the former of which, with the velocity v , strikes the latter at rest, so that after the blow they both proceed uniformly forward together with the velocity z ; in which case it is well known that $b : b + \frac{ghp}{kk} :: z : v$; and therefore the velocity z is $= \frac{bkkv}{bkk + ghp}$. But because of the accession of the ball to the pendulum, the place of the centre of oscillation will be changed; and from the known property of that point we find $\frac{bkk + ghp}{bk + gp}$ = to its distance from the axis. Call this distance of the centre of oscillation, of the mass compounded of the ball and pendulum, h . Then, since z is the velocity

of the point whose distance is k , we have this proportion, as $k : H :: z :$
 $\frac{zH}{k} = \frac{bkv}{bk + gp} =$ the velocity of this compound centre of oscillation.

Again, since $\frac{cc}{2r}$ is the versed sine of the described arc c , its radius being r ; therefore $r : H :: \frac{cc}{2r} : \frac{cc}{2rr} \times \frac{bkk + ghp}{bk + gp} =$ the versed sine of the radius H , or the versed sine of the arc described by the centre of oscillation, which call v ; then is v the perpendicular height descended by this centre, and the velocity it acquires by the descent through this space is thus easily found, viz. as $\sqrt{16\frac{1}{2}} : \sqrt{v} :: 32\frac{1}{2} : \frac{8.02c}{r\sqrt{2}} \times \sqrt{\frac{bkk + ghp}{bk + gp}} =$ the velocity of the centre of oscillation deduced from the chord of the arc which is actually described.

Having thus obtained 2 different expressions for the velocity of this centre, independent of each other, let an equation be made of them, and it will express the relation of the several quantities in the question; thus then we have $\frac{bkv}{bk + gp} = \frac{8.02c}{r\sqrt{2}} \sqrt{\frac{bkk + ghp}{bk + gp}}$, from which we obtain $v = \frac{8.02c}{bkr\sqrt{2}} \sqrt{[(bk + gp) \times (bkk + ghp)]}$ the true expression for the original velocity of the ball the moment before it struck the pendulum.

COROL. But this theorem may be reduced to a form much more simple and fit for use, and yet be sufficiently near the truth. Thus, let the root of the compound factor $\sqrt{[(bk + gp) \times (bkk + ghp)]}$ be extracted, and it will be equal to $\sqrt{h \times (pg + bk \times \frac{h+k}{2h})}$ within the 100000th part of the truth in such cases as generally happen. But since $bk \times \frac{h+k}{2h}$ is usually but about the 300th or 400th part of pg , and that bk differs from $bk \times \frac{h+k}{2h}$ but by about the 80th or 100th part of itself, therefore $pg + bk$ is within about the 20000th or 30000th part of $pg + bk \times \frac{h+k}{2h}$. Consequently v is $= 8.02c \sqrt{\frac{1}{2}h} \times \frac{pg + bk}{bkr}$ very nearly. Or, further, if g be written for h in the last term bk , then finally v is $= 8.02cg \sqrt{\frac{1}{2}h} \times \frac{p+b}{bkr}$, or $v = 5.672cg \sqrt{h} \times \frac{p+b}{bkr}$; which is an easy theorem to be used on all occasions; and being within about the 3000th part of the truth, it is sufficiently exact for all practical purposes whatever. Where it must be observed, that c, g, h, r , may be taken in any measures, either feet or inches, &c. provided they be but all of the same kind; but h must be in feet, because the theorem is adapted to feet.

SCHOLIUM. As the balls remain in the pendulum during the time of making one whole set of experiments, by the addition of their weight to it, both its weight and the centres of gravity and oscillation will be changed by the addition of each ball which is lodged in the wood, and therefore p, g , and h , must be corrected after every shot in the theorem for determining the velocity v . Now the succeeding value of p is always $p + b$; or p must be corrected by the continual

addition of b : and g is corrected by taking always $g + \frac{k-g}{p+b} b$, or $g + \frac{k-g}{p} b$ nearly for each successive value of g ; or g is corrected by adding always $\frac{k-g}{p} b$ to the next preceding value of g : and lastly, h is corrected by taking for its new values successively $\frac{pgh + bkk}{pg + bk}$, or by adding always $\frac{k-h}{pg + bk} bh$, or $\frac{k-h}{p} b$ nearly, to the preceding value of h ; so that the 3 corrections are made by adding always,

$$b \text{ to the value of } p,$$

$$\frac{k-g}{p} \times b \text{ to the value of } g,$$

$$\frac{k-h}{p} \times b \text{ to the value of } h.$$

Before proceeding to the experiments, it may not be improper to take notice of 3 seeming causes of error, which have not been brought to account in our theorem for determining the velocity of the shot: and to examine here whether their effects can sensibly affect the conclusion. These are the penetration of the ball into the wood of the pendulum, the resistance of the air to the back of it, and the friction on the axis: by each of these 3 causes the motion of the pendulum seems to be retarded. The principle on which our rule is founded, supposes the momentum of the ball to be communicated to the pendulum in an instant; but this is not accurately the case, because this force is communicated during the small time in which the ball makes the penetration; but as this is generally effected before the pendulum has moved $\frac{1}{10}$ of an inch out of its vertical position, and usually amounts to scarcely more than the 200th part of a second, its effect will be quite imperceptible, and therefore it may safely be neglected in these experiments. As to the 2d retarding force, or the resistance of the air to the back of the pendulum, it is manifest that it will be quite insensible, when it is considered that its velocity is not more than 3 feet in a second, that its surface is but about 20 inches square, and that its weight is 4 or 500 pounds. Neither can the effect of the last cause, or the friction on the axis, ever amount to a quantity considerable enough to be brought into account in these experiments: for, besides that care was taken to render this friction as little as possible, the effect of the small part which does remain, is nearly balanced by the effect it has on the distance of the centre of oscillation; for as this centre was determined from the actual vibrations of the pendulum, the friction on the axis would a little retard its motion, and cause its vibrations to be slower, and the consequent distance of this centre to be greater; so that the other parts of our theorem being multiplied by \sqrt{h} , or the root of this distance, which is as the time of a vibration, it is evident that the friction in the one case operates against that in the other; and that the difference of the two is the real efficacious cause of resistance, and which therefore is either equal to nothing, or very nearly so. These general causes of error in the principles of the experiments are therefore

safely omitted in the theorem: and our only care must be to guard against accidental errors in the actual execution of the business.

Of the Experiments.—The gun, with which the experiments were made, was of brass; the diameter of the bore or cylinder at the muzzle was $2\frac{4}{5}$, or 2.16 inches; but its diameter next the breech was a small matter less, being there only $2\frac{2}{5}$, or 2.08 inches; so that the greatest cast-iron ball it would admit was just $19\frac{1}{2}$ ounces avoirdupois, or $1\frac{1}{4}$ pound wanting half an ounce; but sometimes leaden balls were used, which weighed above $1\frac{3}{4}$ pound; and sometimes long or cylindrical shot, which weighed near 3 lb. The length of the bore was $42\frac{3}{5}$, or 42.6 inches, so that it was nearly $20\frac{1}{2}$ calibers long.

The powder used was of the sort commonly made for government; the quantity was 2, 4, or 8 ounces, to a charge, which was always put into a light flannel bag, and rammed more or less, as expressed in each day's experiments, but without using any wad before it. The distance of the gun from the pendulum was 29 or 30 feet; which distance was found to be proper by firing the piece, with 8 oz. of powder without a ball, at different distances, till the force of the elastic fluid was found not to move the pendulum. The penetrations of the balls into the wood were attempted to be taken, but were soon neglected on account of their uncertainty, because of so many balls striking in or near the same part of the wood. The depth of the penetration seemed to be near about 3 inches in solid wood when 2 oz. of powder were used.

The first course of experiments was on the 13th of May, 1775, being a clear, dry day. The weights and measures then taken were thus, viz.

$p = 328$ pounds, the weight of the pendulum.

$g = 72$ inches, the distance of the centre of gravity.

$h = 88$ inches = $7\frac{1}{3}$ feet, the distance of the centre of oscillation.

$r = 102\frac{1}{2}$ inches, the distance to the bottom or tape.


The value of $h = 88$ was determined from the number of 40 vibrations being made in a minute; for $40^2 : 60^2 :: 4 : 9 :: 39.2 : 88$. The number of rounds or shot was 8, and the circumstances and results as exhibited in the following table:


Number.	Weight of Powder.	Diam. of the ball.	Height of the Charge.	Struck below the axis, k .	Weight of the ball.	Weight of the ball, b .	Values of p .	Values of g .	Chord of the arc, c .	Veloc. per second.
	Oz.	In.	Inches.	Inches.	Oz.	Pounds	Pounds	Inches.	Inches.	Feet.
1	2	1.98		92.5	$17\frac{1}{2}$	1.094	328.0	72.0	13.0	458
2	2	1.98		92.5	$17\frac{1}{2}$	1.094	329.1	72.1	17.8	631
3	2	1.98	3.15	91.6	$17\frac{1}{2}$	1.094	330.2	72.2	18.1	650
4	2	1.97	3.15	91.	$17\frac{1}{2}$	1.078	331.3	72.3	17.6	646
5	2	1.97	3.15	90.5	$17\frac{1}{2}$	1.078	332.3	72.3	16.3	604
6	2	1.96	3.15	92.4	17	1.063	333.4	72.4	16.2	598
7	4	1.97	4.5	92.	$17\frac{1}{4}$	1.078	334.4	72.5	24.0	881
8	4	1.96	4.5	90.5	17	1.063	335.5	72.5	25.0	950

By computing the velocities from our theorem investigated in the corollary, they come out as they are here registered in the last column of the table, and they are all pretty regular, excepting the first, which is about $\frac{1}{4}$ part less than the rest, with the same weight of powder, and which irregularity must have been caused by some unperceived accident. The values of p and g were each corrected by their respective theorems; but the value of h was kept the same ($7\frac{1}{3}$ feet) throughout, because its correction was so small as not to make a difference of above a foot or two at most in the velocity: and for the same reason this correction is neglected, as quite unnecessary, in the rest of the experiments of the other days following.

The mean velocity of the 2d, 3d, 4th, 5th, and 6th numbers is 626, and of the 7th and 8th it is 915; that is, the velocity with 2 oz. of powder was 626 feet per second, and that with 4 oz. was 915 feet; and these 2 velocities are in the ratio of 1 to 1.46. But the mean weight of the balls in the former case was $17\frac{1}{3}$ oz., and in the latter it was $17\frac{1}{8}$ oz.; and the ratio of the quantities of powder was that of 1 to 2. But the direct subduplicate ratio of the powder, compounded with the inverse subduplicate ratio of the weights of the shot, forms the ratio of 1 to 1.42, which is nearly equal to the ratio (1 to 1.46) of the velocities; that is, in this instance the velocities are very nearly as the square roots of the quantities of powder directly, and the square roots of the weights of the balls inversely. The powder was forced up with only one stroke of the rammer.

The 2d course was performed on the 3d of June, 1775, which was a clear, dry day, but windy. Some of the experiments of this day are doubtful, as indeed is evident from their irregularity, on account of the wind blowing the tape, which was not very properly secured by the little brazen machine through which it was made to slide. The powder was taken from the bottom of a barrel, and the charges rammed a little closer than those of the former day; and so tight did the shots fit towards the breech, that many strokes of the rammer were necessary to drive them home.

The 4th and 5th shots were of a long form, which may be called spherico-cylindrical, as they were cylinders terminated by hemispherical ends, so that their section through the axis was of this form , and the length of the axis was nearly double the diameter of the shot.

The 4th shot or 1st of the long sort, struck sideways, making a hole of the shape of the above section, only its length or axis was not horizontal but vertical, thus . The last shot lay obliquely in the wood; it appeared to have struck with its end foremost, or nearly so, as the oblique position in which it lay seemed to be caused by its striking against a former shot lodged in the wood, with the hance of its end, so as to flatten it in that part. Of the pendulum,

the weight, length, and centres of gravity and of oscillation were the same as when taken the former day before the experiments were made; the former balls having been extracted, and the holes filled up with wood.

Number.	Weight of powder.	Diameter of the ball.	Height of the charge.	Struck below the axis, <i>k</i> .	Weight of the ball.	Weight of the ball, <i>b</i> .	Values of <i>p</i> .	Values of <i>g</i> .	Chord of the arc, <i>c</i> .	Veloc. per second.
	Oz.	In.	Inches.	Inches.	Oz.	Pounds	Pounds	Inches.	Inches.	Feet.
1	2	2.08	2.85	88½	19½	1.219	328.0	72.0	24.3	800
2	2	2.08	2.85	89	19½	1.219	329.2	72.1	30.5	1003
3	2	2.08	2.85	93½	19½	1.219	330.4	72.1	30.0	943
4	2	2.08	3.35	92½	46½	2.906	331.6	72.2	57.0	767
5	2	2.08	3.35	93	46½	2.906	334.5	72.4	54.0	731

Here the 1st shot is again so much smaller than the 2 following, that some irregularity must have attended it, on which account no use can be made of it. The mean between the 2d and 3d is 973; and between the 4th and 5th the mean is 749; that is, the velocity of the 19½ ounce ball is 973, and that of the 46½ ounce shot 749 feet per second, which 2 numbers are in the ratio of 1.3 to 1. But the reciprocal subduplicate ratio of the weights (19½ and 46½) is the ratio of 1.54 to 1: therefore, in this instance, the velocity of the heavier shot is a little less than would arise from the inverse ratio of the square roots of the weights of the shot. But the accurate ratio cannot certainly be drawn from these numbers, on account of the doubtfulness of some of them, as before observed.

It is very remarkable, that in the experiments of this day, the mean velocity with 2 oz. of powder, is 973, whereas it was no more than 626 in the former day with the same quantity of powder, though the balls were heavier with the greater velocity in the ratio of 19 to 17 nearly. This remarkable difference must be chiefly owing to the windage in the first course: and hence we may perceive the great advantage to be gained by the use of balls approaching in proportion nearer to the diameter of the bore of the gun than what is prescribed in the present establishment. Possibly however some part of this difference might be owing to some small inequality in the powder, as that which was used this day was taken from the bottom of a barrel. Perhaps also some part of the effect may be owing to the greater degree of ramming which the powder had in this course.

The 3d course was made on the 12th of June, 1775, being a clear, dry, and calm day. The powder in the experiments of this day was rammed in the same degree as in the last. It was also nearly the same in the succeeding days, as may be perceived by inspecting the 4th column of each course, which, denoting

the height of the charge, shows the degree of compactness with which the powder was lodged in the piece. The dimensions, as taken this day, were thus:

$p = 324$ pounds, the weight of this pendulum.

$g = 71.4$ inches, the distance of the centre of gravity.

$h = 88$ inches $= 7\frac{1}{3}$ feet, the distance of the centre of oscillation.

$r = 102\frac{1}{2}$ inches, the whole length to the tape.


Number.	Weight of powder.	Diameter of the ball.	Height of the Charge.	Struck below the axis, k .	Weight of the ball.	Weight of the ball, b .	Values of p .	Values of g .	Chord of the arc, c .	Vec. per second.
	Oz.	Inches.	Inches.	Inches.	Oz.	Pounds	Pounds	Inches.	Inches.	Feet.
1	2	2.080	2.85	94	$19\frac{1}{2}$	1.219	324.0	71.4	23.0	700
2	2	2.036	2.85	94	$18\frac{1}{4}$	1.141	325.2	71.5	24.5	799
3	2	2.045	2.85	$93\frac{1}{2}$	$18\frac{1}{2}$	1.156	326.4	71.6	22.0	715
4	4	2.062	4.	$92\frac{1}{4}$	19	1.188	327.5	71.7	27.3	880
5	4	2.036	4.	$93\frac{1}{2}$	$18\frac{1}{4}$	1.141	328.7	71.7	35.0	1163
6	4	2.045	4.	$93\frac{1}{2}$	$18\frac{1}{2}$	1.156	329.9	71.8	33.0	1087

Here the common mean weight of the ball is $18\frac{2}{3}$ ounces, the mean velocity with 2 oz. of powder is 738, and that with 4 oz. of powder is 1043 feet per second. The ratio of these 2 velocities, is that of 1 to 1.414: that is, accurately the ratio of the square roots of the quantities of powder.

Of the Experiments made with the 2d Pendulum.—The 1st pendulum was gradually more and more rent and shattered by the firing of so many balls into it, till at the end of the last course of experiments it had become quite useless. Another was then fitted up, and with it were performed the 2 following courses. This 2d pendulum consisted of a cubical block of sound elm, of nearly 2 feet long, fixed to the iron stem, but not exactly in the manner of the former; for in this the stem was placed vertically over the centre of the top end, to which point it continued whole, but there divided in 2, each passing to the right and left over the top, down the sides, and returning along the bottom, and being at proper intervals fastened to the wood with iron pins. A thick sheet of lead was fastened over each of the 2 upright faces into which the shot were to be fired, both to guard them from splintering very much, and to add to the weight of the pendulum. The whole was then firmly secured by 2 very thick iron bands or hoops, passed horizontally quite around the wood, and firmly fixed to it, the one next the upper end, and the other near the lower, so as strongly to resist the endeavours of the shot to split the wood.

The whole weight of the pendulum, thus fitted up, was 552 pounds; its whole length, from the middle of the axis to the tape at the bottom, was 101 inches; the distance to the centre of gravity was 78 inches; and the distance to the centre of oscillation was 88 inches, equal to $7\frac{1}{3}$ feet, which was exactly the

same as that of the former pendulum, their numbers of vibrations being alike in the same time. Instead of suspending this pendulum, after the manner of the former, by the ends of its axis in grooves turned to fit them, they were only placed on flat, level pieces of wood, on which this pendulum vibrated much freer than the other did; but a small nail was driven into the supporting wood, just behind each end of the axis, to prevent the stroke of the shot from throwing it off the stand.

To this pendulum was adapted a better machine for the tape to slide through than the former, the inconvenience of which had often been experienced by its catching and entangling the tape, so as to interrupt its free motion, and once indeed to break it. This new one, however, was at once very simple, and perfectly free from every inconvenience, giving just the necessary degree of friction to the tape, without ever stopping its motion; so that, of the real quantity drawn out by the vibration of the pendulum, there could not possibly be the least doubt. This simple contrivance consisted barely of about 6 or 8 inches of the list of woollen cloth fastened on the arch of a small piece of wood, which was shaped into the form of the segment of a circle thus , the tape being made to pass through between the curved side and the list, which was moderately stretched and fastened by its 2 ends to those of the little arch.

On the whole, the machinery was all so perfect, and every circumstance attending the experiments of the 2 ensuing days so carefully observed, that we may with great safety rely on the conclusions resulting from them. And as those of the one day were made with leaden balls, and those of the other with iron ones, which differ greatly in weight, every other circumstance being the same, they afford very good means for discovering the law of the different weights of shot, while the variations in the powder from 2 to 4 and 8 ounces, furnish us with the rule for the different quantities of it.

The 4th course was on the 20th of July, a fine clear day. The powder was a mixture of several of the sorts made for government, and the balls were of lead. The quantities of powder were 2, 4, and 8 ounces alternately; and the dimensions at first were thus:

$p = 552$ pounds, the whole weight of the pendulum.

$r = 101$ inches, its whole length.

$g = 78$ inches, the distance of the centre of gravity.

$h = 88$ inches $= 7\frac{1}{3}$ feet, that of the centre of oscillation.

Number.	Weight of powder.	Diam of the ball.	Height of the charge.	Struck below the axis, <i>k</i> .	Weight of the ball.	Weight of the ball, <i>b</i> .	Values of <i>p</i> .	Values of <i>g</i> .	Chord of the arc, <i>c</i> .	Veloc. per second.
	Oz.	In.	Inches.	Inches.	Oz.	Pounds	Pounds	Inches.	Inches.	Feet.
1	2	2.021	2.85	90.	28 $\frac{1}{4}$	1.766	552.0	78.0	14.8	612
2	4	2.021	4.4	87.	28 $\frac{1}{4}$	1.766	553.8	78.0	20.5	879
3	8	2.032	7.1	87.	28 $\frac{3}{4}$	1.797	555.5	78.1	27.5	1164
4	2	2.026	2.85	90.	28 $\frac{1}{2}$	1.781	557.3	78.1	15.0	622
5	4	2.026	4.4	88.	28 $\frac{1}{2}$	1.781	559.1	78.1	20.5	871
6	8	2.032	7.1	92.	28 $\frac{3}{4}$	1.797	560.9	78.2	28.5	1154
7	2	2.021	2.85	89.8	28 $\frac{1}{4}$	1.766	562.7	78.2	14.3	605
8	4	2.026	4.4	91.3	28 $\frac{1}{2}$	1.781	564.5	78.2	21.0	870
9	8	2.026	7.1	87.	28 $\frac{3}{4}$	1.781	566.2	78.3	26.8	1169

Let us now collect together the several velocities belonging to the same quantity of powder, in order to take their means, thus :

Veloc. with 2 ounces.	Veloc. with 4 ounces.	Veloc. with 8 ounces.
612	879	1164
622	871	1154
605	870	1169
<hr/>	<hr/>	<hr/>
3) 1839	3) 2620	3) 3487
<hr/>	<hr/>	<hr/>
The means 613	873	1162

The uniformity of these velocities is very striking, and the means with 2, 4, and 8 ounces of powder are 613, 873, and 1162, which are in the ratio of 1, 1.424, and 1.9; which numbers are nearly in the ratio of the square roots of the quantities (2, 4, and 8) of powder, the numbers in this latter ratio being 1, 1.414, and 2, where the small difference lies chiefly in the last number. A small part of this defect in the greatest velocity is to be attributed to the mean weight of the balls used with it being greater than in the others; for the mean weight of the balls used with 8 oz. of powder, is 28 $\frac{3}{4}$ ounces, while that with the 2 and 4 ounces is only 28 $\frac{1}{4}$; the reciprocal sub-duplicate ratio of these is that of 1 to 1.006, in which proportion, increasing 1.9 the number for the greater velocity, it becomes 1.91, which still falls short of 2 by .09, which is about the $\frac{1}{22}$ part too small for the sub-duplicate ratio of the powder. This defect of a 22d part is owing to 3 evident causes, viz. 1. The less length of cylinder through which the ball was impelled; for by inspecting the 4th column, denoting the height of the charge, it appears, that the balls lay 3 or 4 inches nearer the muzzle of the piece with the 8 ounce charge than with the others. 2. The greater quantity of elastic fluid which escaped in this case than in the others by the windage; this happens from its moving with a greater velocity, in consequence of which a greater quantity escapes by the vent and windage than with the smaller velocities. 3. The 3d cause is the greater quantity of powder blown out unfired in this case than in that of the less velocities; for the ball, being impelled with the greater velocity, would be sooner out of the piece

than the others, and the more so as it had a less length of the bore to move through; and if powder fire in time, which cannot be denied, though indeed that time is manifestly very short, a greater quantity of it must remain unfired when the ball with the greater velocity issues from the piece, than when that which has the less velocity goes out, and still the more so as the bulk of powder which was at first to be inflamed in the one case so much exceeded that in the others. The effect however will arise chiefly from the first and last of these 3 causes, as that of the 2d will amount to very little; because the effect arising from the greater velocity with which the fluid escapes at the vent and windage, is partly balanced by the shorter time in which it acts.

From the above reflections we may also perceive, how small the quantity of powder is which is blown out unfired in any of these cases, and the amazing quickness with which it fires in all cases: for though the time in which the ball passed through the barrel, when impelled by the 8 oz. of powder, was not greatly different from the half only of the time in which it was impelled by the 2 oz. it is evident that in half the time there was nearly 4 times the quantity of powder fired.

The 5th or last course was on the 21st of September, 1775; fine clear weather, but a little windy. The machinery and the balls were of iron, but the powder the same as in the last course, and the dimensions as follow:

$p = 553$ pounds, the weight of the pendulum.

$r = 101$ inches, its length.

$g = 78\frac{1}{8}$ inches, the distance of the centre of gravity.

$h = 84.775$ inches = 7.065 feet, that of the centre of oscillation, the pendulum making 68 vibrations in 100 seconds.

Number.	Weight of powder.	Diam. of the ball.	Height of the charge.	Struck below the axis, k .	Weight of the ball.	Weight of the ball, b .	Values of p .	Values of g .	Chord of the arc, c .	Veloc. per second.
	Oz.	In.	In.	Inches.	Oz. Dr.	Pounds	Pounds	Inches.	Inch	Feet.
1	2	2.062	3.	88.3	19 0	1.188	553.0	78.1	11.4	702
2	4	2.062	4.3	88.3	19 0	1.188	554.2	78.1	17.8	1008
3	8	2.062	6.7	91.0	19 0	1.188	555.5	78.2	23.6	1419
4	2	2.070	3.	90.7	19 $3\frac{1}{2}$	1.201	556.8	78.2	11.4	682
5	4	2.080	4.3	90.7	19 $8\frac{1}{2}$	1.221	558.1	78.2	17.3	1020
6	8	2.064	6.7	90.7	19 $0\frac{3}{4}$	1.190	559.4	78.2	22.3	1352
7	2	2.060	3.	91.	18 15	1.184	560.6	78.3	11.4	695
8	4	2.058	4.3	90.	18 14	1.180	561.9	78.3	15.3	948
9	8	2.049	6.7	90.	18 $9\frac{3}{4}$	1.163	563.1	78.3	22.9	1443
10	2	2.047	3	88.3	18 9	1.160	564.3	78.3	10.9	703
11	4	2.037	4.3	88.3	18 $4\frac{1}{2}$	1.142	565.5	78.4	14.8	973
12	8	2.036	6.7	88.3	18 $3\frac{3}{4}$	1.140	566.6	78.4	20.6	1360
13	2	2.034	3.	92.	18 3	1.137	567.8	78.4	11.4	725
14	4	2.034	4.3	92.	18 3	1.137	569.0	78.4	15.0	957
15	8	2.031	6.7	94.3	18 $1\frac{1}{2}$	1.131	570.1	78.5	22.5	1412

Let us now take the means among those of the same quantity of powder, thus:

Veloc. with 2 ounces.	Veloc. with 4 ounces.	Veloc. with 8 ounces.
702	1068	1419
682	1020	1352
695	948	1443
703	973	1360
725	957	1412
<hr/> 5) 3507	<hr/> 5) 4966	<hr/> 5) 6986
<hr/> The means, 701	<hr/> 993	<hr/> 1397

And these mean velocities, with 2, 4, and 8 ounces of powder, are as the numbers 1, 1.416, and 1.993; but the sub-duplicate ratio of the weights (2, 4, and 8) of powder, gives the numbers 1, 1.414, and 2, to which the former are sufficiently near. It is obvious however, that the greatest difference lies in the last number, which answers to the greatest velocity, and which is again in defect. It will be still a little more in defect if we make the allowance for the weights of the balls; for the mean weight of the balls with the 2 and 4 ounces is $18\frac{3}{4}$ ounces, but of the 8 ounces it is $18\frac{3}{5}$; diminishing therefore the number 1.993 in the reciprocal sub-duplicate ratio of $18\frac{3}{5}$ to $18\frac{3}{4}$, it becomes 1.985, which falls short of the number 2 by .015 or the 133d part of itself; which defect is to be attributed to the same causes as it was in the last course of experiments before explained.

Let us now compare the corresponding velocities in this course and the last.

In this course they are 701, 993, 1397;

In the last they were 613, 873, 1162.

Now the ratio of the first 2 numbers, or the velocities with 2 oz. of powder, is that of 1 to 1.1436; the ratio of the next 2, is that of 1 to 1.1375; and the ratio of the last is that of 1 to 1.2022. But the mean weight of the shot was, for 2 and 4 ounces of powder, $28\frac{1}{2}$ ounces in the last course, and $18\frac{3}{4}$ ounces in this; and for 8 ounces of powder, it was $28\frac{2}{3}$ in the last, and $18\frac{3}{5}$ in this: taking now the reciprocal sub-duplicate ratios of these weights of shot, we obtain the ratio of 1 to 1.224 for that of the balls which were fired with 2 oz. and 4 oz. of powder, and the ratio of 1 to 1.241 for the balls which were fired with 8 ounces. But the real ratios above found are not greatly different from these. And the variation of the actual velocities from this law of the weights of shot, incline the same way in this course, as they appeared to do in the 2d course of these experiments.

We may now collect into one view the principal inferences that have resulted from these experiments.

1. And first, it is made evident by them, that powder fires almost instantaneously.

neously, seeing that almost the whole of the charge fires, though the time be much diminished. 2. The velocities communicated to balls or shot of the same weight, with different quantities of powder, are nearly in the sub-duplicate ratio of those quantities. A very small variation, in defect, taking place when the quantities of powder become great. 3. And when shot of different weights are fired with the same quantity of powder, the velocities communicated to them are nearly in the reciprocal sub-duplicate ratio of their weights. 4. So that, universally, shot which are of different weights, and impelled by the firing of different quantities of powder, acquire velocities which are directly as the square roots of the quantities of powder, and inversely as the square roots of the weights of the shot, nearly. 5. It would therefore be a great improvement in artillery, to make use of shot of a long form, or of heavier matter; for thus the momentum of a shot, when fired with the same weight of powder, would be increased in the ratio of the square root of the weight of the shot. 6. It would also be an improvement to diminish the windage; for by so doing, one-third or more of the quantity of powder might be saved. 7. When the improvements mentioned in the last 2 articles are considered as both taking place, it is evident that about half the quantity of powder might be saved, which is a very considerable object. But important as this saving may be, it seems to be still exceeded by that of the article of the guns; for thus a small gun may be made to have the effect and execution of a gun of 2 or 3 times its size in the present mode, by discharging a shot of 2 or 3 times the weight of its natural ball or round shot. And thus a small ship might discharge shot as heavy as those of the greatest now made use of.

Finally, as the above experiments exhibit the regulations with regard to the weights of powder and balls, when fired from the same piece of ordnance, &c.; so by making similar experiments with a gun, varied in its length, by cutting off from it a certain part before each course of experiments, the effects and general rules for the different lengths of guns may be certainly determined by them. In short, the principles on which these experiments were made, are so fruitful in consequences, that, in conjunction with the effects resulting from the resistance of the medium, they seem to be sufficient for answering all the inquiries of the speculative philosopher, as well as those of the practical artillerist.

*IV. A New Case in Squinting. By Erasmus Darwin, M.D., F.R.S. p. 86.
Dated Lichfield, March 1777.*

The following case in squinting, as a similar one has not been recorded or explained by others, may perhaps merit attention from its novelty.

In 1771, Dr. D. was desired to see a child of the Rev. Dr. Sandford, in Shropshire, to determine if any method could be devised to cure him of squint-

ing. The child was then about 5 years old, and exceedingly tractable and sensible, which enabled Dr. D. to make the following observations with great accuracy and frequent repetition.

1. He viewed every object which was presented to him with only one eye at a time.
2. If the object was presented on his right side, he viewed it with his left eye; and if it was presented on his left side, he viewed it with his right eye.
3. He turned the pupil of that eye, which was on the same side with the object, in such a direction, that the image of the object might fall on that part of the bottom of the eye where the optic nerve enters it.
4. When an object was held directly before him, he turned his head a little to one side, and observed it with only one eye, viz. with that most distant from the object, turning away the other in the manner above described; and when he became tired with observing it with that eye, he turned his head the contrary way, and observed it with the other eye alone, with equal facility; but never turned the axes of both eyes on it at the same time.
5. He saw letters, which were written on bits of paper, so as to name them with equal ease, and at equal distances, with one eye as with the other.
6. There was no perceptible difference in the diameters of the irises, nor in the contractibility of them, after having covered his eyes from the light.

These observations were carefully made by writing single letters on shreds of paper, and laying wagers with the child that he could not read them when they were presented at certain distances and directions.

From these circumstances it appeared, that there was no defect in either eye, which is the common cause of squinting, so well observed by M. Buffon and Dr. Reid; and hence, that the disease was simply a depraved habit of moving his eyes, and might probably be occasioned by the form of a cap or head-dress, which might have been too prominent on the sides of his face, like bluffs used on coach-horses; and might thence, in early infancy, have made it more convenient for the child to view objects placed obliquely with the opposite eye, till by habit the muscoli adductores were become stronger, and more ready for motion than their antagonists.

A paper gnomon was made, and fixed to a cap; and when this artificial nose was placed over his real nose, so as to project an inch between his eyes, the child, rather than turn his head so far to look at oblique objects, immediately began to view them with that eye which was next to them. But the death of Dr. Sandford, which happened soon after, occasioned the removal of his family; and the grief and cares of Mrs. Sandford prevented this, and the other methods proposed, from being put in execution.

In Feb. 1777, Dr. D. had again an opportunity of seeing master Sandford, and observed all the circumstances of his mode of vision to be exactly as they were 6 years before, except that they seemed established by longer habit; so

that Dr. D. could not by any means induce him to bend the axes of both his eyes on the same object not even for a moment. A gnomon of thin brass was made to stand over his nose, with a half circle of the same metal to go round his temples; these were covered with black silk, and by means of a buckle behind his head, and a cross piece over the crown of his head, this gnomon was managed so as to be worn without any inconvenience, and projected before his nose about $2\frac{1}{2}$ inches. By the use of this gnomon he soon found it less inconvenient to view all oblique objects with the eye next to them, instead of the eye opposite to them. After this habit was weakened by a week's use of the gnomon, 2 bits of wood, about the size of a goose-quill, were blackened, all except $\frac{1}{4}$ of an inch at their summits; these were frequently presented for him to look at, one being held on one side the extremity of his black gnomon, and the other on the other side of it. As he viewed these they were gradually brought forwards beyond the gnomon, and then one was concealed behind the other: by these means, in another week, he could bend both his eyes on the same object for half a minute together. By the practice of this exercise before a glass, almost every hour in the day, he became in another week able to read for a minute together with his eyes both directed on the same objects; and Dr. D. had no doubt, if he had patience enough to persevere in these efforts, but he would in the course of some months overcome this unsightly habit.

Dr. D. concludes the account of this case by adding, that all the other squinting people he had had occasion to attend to, had one eye much less perfect than the other, according to the observations of Mr. Buffon and Dr. Reid. These patients, where the diseased eye is not too bad, are certainly curable by covering the best eye many hours in a day; as, by a more frequent use of the weak eye, it not only acquires a habit of turning to the objects which the patient wishes to see, but gains at the same time a more distinct vision; and the better eye at the same time seems to lose somewhat in both these respects, which also facilitates the cure.

This evinces the absurdity of the practice of prohibiting those who have weak eyes from using them; since the eye, as well as every other part of the body, acquires strength from that degree of exercise which is not accompanied with pain or fatigue; and he was induced to believe, that the most general cause of squinting in children originates from the custom of covering the weak eye, which has been diseased by any accidental cause, before the habit of observing objects with both eyes was perfectly established.

The facility with which master Sandford received the images of oblique objects on the insensible part of the retina of one eye, while he viewed them with the other, induced Dr. D. to observe the size of this insensible spot, and to endeavour to ascertain the cause of it. There was formerly a dispute among philoso-

phers, whether the choroid coat of the eye or the retina was the immediate organ of vision, which had lately been revived in some measure in Dr. Priestley's valuable history of Light and Colours; and it was then thought by one party in this dispute, that the defect of the choroid coat, where the optic nerve enters the eye, was the cause of this want of vision in that part. But the following observation shows beyond a doubt the fallacy of this supposition: the diameter of the optic nerve, at its entrance into the eye, is about $\frac{1}{6}$ of an inch, and the perforation of the choroid coat, through which it passes, must of necessity be of the same diameter: now the dark spot, which is seen in objects opposed to the centre of the optic nerve, if it was occasioned by the deficiency of the choroid coat, should, at 9 inches distance from the eye, be 54 times the diameter of this aperture, or 9 inches in diameter; whereas Dr. D. found by experiment, that a paper of 1 inch in diameter could not be totally concealed at 9 inches distance from his eye: and M. Le Cat by accurate observations found, that the insensible part of his eye was but between the 30th and 40th part of an inch in diameter. This experiment is so easily made, that it can be attended with no fallacy; and at the same time that it shows that the insensible spot, where the optic nerve enters the eye, is not owing to the deficiency of the choroid coat, entirely subverts the opinion of the choroid coat being the organ of vision; for vision exists where the choroid coat is not.

Nor is the insensibility of the centre of the optic nerve owing to the ingress of the arteries along with it into the eye; for a large branch of this artery runs along the bottom of the eye, where vision is most distinct, and because all this artery is covered with the expanse of the retina on the external side of it. Mr. Savage made an experiment for another purpose, which however shows, that the optic artery, where it is branched under or through the retina, does not much disturb the power of vision. It is this: if you look on a white wall on a luminous day, with the sun shining on the wall only by its reflected light, you will discern the parts of the wall become darker and lighter at every pulsation of the optic artery. This darker and lighter appearance is like net-work, and not uniform like the wall itself; but the whole, though rather darker while the diastole of the artery compresses the retina, is yet distinctly visible.

The following circumstance seems to give rise to the insensibility of the central part of the optic nerve at its ingress into the eye, which he had observed in several calves' eyes. The point of a pair of scissars was introduced behind the ciliary circle, and the whole of the cornea, aqueous humour, iris, and crystalline, being removed, the retina was beautifully seen through the vitreous humour somewhat magnified. On exposing this to the sun-shine, and inspecting it with nicety, a white filament, about the 10th of an inch in length, arising from the centre of the optic nerve, was seen ascending straight upwards into

the vitreous humour, like a thin white worm. The use of this may be to supply the vitreous humour or crystalline with nourishment, whether it be a nerve or an empty blood-vessel; but this is certain, that its rising so high above the surface of the retina must render it incapable of vision: whence there is just reason to conclude, that this conformation must be the true cause of the insensibility of this part of the eye.

Dr. D. does not affirm, that the human eye, either during infancy or in our riper years, is similar in conformation to that of a calf, nor have we (he remarks) sufficient opportunities to observe them; but he suspects this vessel may, after the growth of the animal, be totally obliterated; and that, in some few instances, the optic nerve may even in this part become sensible to light. One instance he was certain he had seen, as it was in a man capable of the most patient and accurate observation, who on numberless repeated trials, at different times, in his presence, could never lose sight of the smallest object with either of his eyes.

Supplement to the Case in Squinting.—It afterwards occurred to Dr. D. that the unusual mode of squinting described in the above paper, must have arisen from some original difference in the sensibility of some parts of the eye, which might have rendered it more easy for master Sandford, when a child, to observe objects with one eye only, and that with the eye most distant from objects presented obliquely to him.

Two circular papers, each of 4 inches diameter, were stuck against the wall, their centres being exactly at 8 inches distance from each other. On closing one eye, and viewing the central spot of one of these papers with the eye farthest from it, and then retreating 26 inches from it, the other paper became invisible. This experiment was made on 5 people, of various ages, from 10 years old to 40; and the paper disappeared to them all at about this distance, or an inch or 2 more or less: but to master Sandford the paper disappeared at about 13 inches distance from the wall. These papers were afterwards removed to 12 inches, and then to 4 inches interval between them; and by the nicest observations on repeated trials he found, that the paper, equally with one eye as with the other, uniformly disappeared to him at about $\frac{1}{2}$ the distance it did to 5 others.

Another curious circumstance is, that as large a paper disappeared to him at $\frac{1}{2}$ the distance as it did to others at the whole distance; and hence the insensible part of the centre of the optic nerve in his eyes was, as near as could be estimated, 4 times the area of the insensible part of the eyes of other people, at the same time that the angle made between the ingress of the optic nerve and the bottom of the eye was twice as great as in others.

It is easy to conceive that, in early infancy, when any object which the child

wished to inspect was presented obliquely to him, that on this first indistinct view of it, before either eye could be turned towards it, it would appear much more brilliant and distinct to the contrary eye, than to that nearest the object, as so great a part of it would now fall on the large insensible part of that eye. This must naturally induce him to view it with the opposite eye, to which it already appeared more brilliant and distinct: and this to him would be so much easier to accomplish, as the insensible part of the neglected eye was large enough to receive as great a part of an object as is usually viewed at once with accuracy, and hence would not confuse the vision of the other.

Dr. D. adds that, by wearing the artificial nose, he had greatly corrected the habit of viewing objects with the eye farthest from them; and had more and more acquired the voluntary power of directing both his eyes to the same object, particularly if the object were not more than 4 or 5 feet from him; and would, he believed, by resolute perseverance, entirely correct this unsightly deformity.

V. Cure of a Muscular Contraction by Electricity. By M. Partington. p. 97.

Miss Lingfield's head was drawn down over her right shoulder; the back part of it was twisted so far round, that her face turned obliquely towards the opposite side; by which deformity she was disabled from seeing her feet, or the steps as she came down stairs. The sterno-mastoideus muscle was in a state of contraction and rigidity. She had no material pain on this side of her neck; but owing to the extreme tension of the teguments of the left side, she had a pain continually, and often it was very violent, particularly in sudden changes of the weather. Her pulse was weak, quick, and irregular. She was subject to a great irritability, had frequently a little fever, which came on in an evening, and left her before morning; her spirits were generally exceedingly oppressed, and at times she was slightly paralytic. She dated the origin of her disorder at something more than 2 years from that period. She was suddenly seized, going out of a warm room into the cold air, with a pain on the back of her head, which admitted of small abatement for some months, contracting gradually the muscles to a melancholy deformity; and though all prudent means had been used to subdue it, and she strictly observed every article prescribed by the faculty, she was rather worse.

Mr. P. urged her to try electricity; and though the weather was tempestuous, she was electrified by him; the first time, Feb. 18, 1777. He drew strong sparks from the parts affected for about 4 minutes, which brought on a very profuse perspiration, a circumstance she had not been used to, which seemed to relax the mastoideus muscle to a considerable degree; but, as the sparks gave her a good deal of pain, he desisted from drawing them, and only subjected her

a few minutes longer to the admission of the fluid, which passed off without interruption from the pores of her skin and adjacent parts. The next time she came to him was the 24th of the same month: as she had been in the afternoon of the first day's experiment a good deal disordered, he changed the mode of conducting, and set her in a common dining-chair, while he dropped, for 5 minutes, by the means of a large discharging rod with a glass handle, very strong sparks on the mastoideus muscle, from its double origin at the sternum and clavicle to its insertion at the back of the head. She bore this better than before, and the same good effect followed in a greater degree, and without any of the subsequent inconveniencies. He saw her the 3d time on the 27th: she assured him she had escaped her feverish symptoms on evenings, and that her spirits were raised by the prospect of getting well; that, since the last time he electrified her, she had more freedom in the motion of her head than she had ever experienced since the first attack of her disorder. He persisted in electrifying her after the same manner, March 3d, 5th, 6th, 7th, and 9th; from each time she gained some advantage, and her feverish tendency and nervous irritability went off entirely.

The weather now setting in very unfavourable, and fearful of losing the advantages which had happily been reaped from these first trials, Mr. P. requested Mr. Henly, as her next-door neighbour, to electrify her every evening while she was in town, and she might, if any alteration took place, see him occasionally. Fortunately for her, Mr. H. accepted the proposal, and by his judgment and caution in the conduct of it for the next fortnight, 3 evenings only excepted, her cure was accomplished.*

VI. An Account of a large Stone near Cape Town. By Mr. Anderson. p. 102.

The stone is called by the people here the Tower of Babel, and by some the Pearl Diamond. It either takes the last name from a place near which it is situated, or it gives name to the tract of cultivated land called the Pearl. It lies on the top of a ridge of low hills, beyond a large plain, at the distance of about 30 miles from the Cape Town, beyond which, at a little distance, is a range of hills of a much greater height. It is of an oblong shape, and lies north and south. The south end is highest; the east and west sides are steep and high; but the top is rounded, and slopes away gradually to the north end, so that it may be ascended by that way, and a most extensive prospect be enjoyed. Mr.

* The method Mr. Henly pursued was, to place the lady on a stool with glass legs, and to draw strong sparks, for at least 10 minutes, from the muscles on both sides of her neck. Besides this, he generally gave her 2 shocks from a bottle containing 15 square inches of coated surface fully charged, through her neck and one of her arms, crossing the neck in different directions. This treatment she submitted to with a proper resolution; and it was attended with the desired success.—Orig.

A. could not precisely determine its circumference, but he considered it must exceed half a mile. Mr. A. found it difficult to ascertain its height; but at the south end, he thinks it is nearly equal to half its length: or, he might say, it equalled the dome of St. Paul's church.

It is uncertain whether it ought to be considered as the top of the hill, or a detached stone, because there is no positive proof of either, unless we were to dig about its base; but it would certainly impress every beholder, at first sight, with the idea of its being one stone, not only from its figure, but because it is really one solid uniform mass from top to bottom, without any interruption; which is contrary to the general character of the high hills of this country, being commonly divided, or composed of different strata, at least if we may judge from the rows of plants or shrubs which grow on the sides of the steepest, and probably are produced from the small quantity of earth interposed between them. It has indeed a few fissures, or rather impressions, which do not reach deeper than 4 or 5 feet; and near its north end a stratum of a more compact stone runs across, which is not above 12 or 14 inches thick, with its surface divided into little squares, or oblongs, disposed obliquely. Its surface is also so smooth, that it does not appear to have formerly been joined to, or separated from, any other part by violence, as is the case with many other large fragments; but enjoys the exact situation where it was originally placed, and has undergone little change from being exposed for so many ages to the calcining power of a very hot climate.

VII. On Mr. Debraw's Improvements in the Culture of Bees. By Nathaniel Polhill, Esq. p. 107.*

Mr. Polhill in this paper imagines, that Mr. Debraw's discoveries, if properly pursued, may be of considerable public utility, since those who cultivate bees for profit will, by adopting his method of compelling the common bees to produce a queen, be able to increase the number of their stocks at pleasure. He confirms, from his own observation, the existence of two sorts of drones, some no larger than the common or working bees; and he conjectures that the small drones alone are preserved by the working bees, after the breeding season is over, in order to impregnate the eggs in the spring, in preference to the large ones, because they devour less honey, which, he observes, is no inconsiderable object; few hives being so well provided as to have much to spare during the very early part of the spring season.

VIII. An Improved Method of Tanning Leather. By D. Macbride, M.D. p. 111.

The use of tanning is two-fold: first, to preserve the leather from rotting;

* See Philos. Trans., vol. 67, p. 15; or p. 125, of this abridged volume.

and 2dly, to render it impervious to water. An infusion of any strongly astringent vegetable will serve to tan leather, so far as to prevent its rotting; but if this vegetable does not contain a good deal of gum-resin, it will not answer for enabling it to keep out water: and hence it is that oak-bark, which is more abundant in the gummy resinous part than any of our common indigenous astringents, is preferred to all other substances for the purpose of tanning. The tanners prepare their bark by gently drying it on a kiln, and grinding it into a very coarse powder. They then either use it in the way of infusion, which is called ooze, or they strew the dry powder between the layers of hides and skins, when these are laid away in the tan-pits. The ooze is made by macerating the bark in common water, in a particular set of holes or pits, which, to distinguish them from the other holes in the tan-yard, are termed latches.

The first operation of the tanner is to cleanse the hides from all extraneous filth, and remove any remains of flesh or fat which may have been left behind by the butcher. The hair is next taken off; and this is accomplished either by steeping the hides for a short time in a mixture of lime and water, which is termed liming, or by rolling them up close, and piling them in heaps, where they quickly begin to heat and putrefy. The hair being loosened is scraped off, and the tanner proceeds to the operation called fleshing, which consists in a further scraping, with a particular kind of knife contrived for the purpose, and cutting away the jagged extremities and offal parts, such as the ears and nostrils. The raw leather is then put into an alkaline ley, in order to discharge the oil, and render its pores more capable of imbibing the ooze. The tanners of this country (Ireland) generally make their ley of pigeon's dung; but a more active one may be prepared from kelp or pot-ash, taking care however not to make it too strong of the ashes, nor to allow the leather to remain too long in the ley.

The oil being sufficiently discharged, the leather is ready for the ooze, and at first is thrown into smaller holes, which are termed handlers; because the hides or skins, during this part of the process, are taken up, from time to time, and allowed to drain; they continue to work the leather in these handlers, every now and then stirring it up with the utensil called a plunger, which is nothing more than a pole with a knob at its end, till they think proper to lay it away in the vats. In these holes, which are the largest in the tan-yard, the leather is spread out smooth, whereas they toss it into the handlers at random, and between each layer of leather they sprinkle on some powdered bark, till the pit is filled by the leather and bark thus laid in stratum super stratum: ooze is then poured on to fill up interstices; and the whole crowned with a sprinkling of bark, which the tanners call a heading. In this manner the leather is allowed to macerate, till the tanner sees that it is completely penetrated by the ooze: when this is accomplished (which he knows by cutting out a bit of the thickest part of the

hide) the manufacture is finished, so far as relates to tanning, since nothing now remains but to dry the goods thoroughly, by hanging them up in airy lofts built for the purpose. Such in general is the process for tanning calf-skins, and those lighter sorts of hides called butts; but the large, thick, heavy hides, of which the strongest and most durable kind of sole-leather is made, require to have their pores more thoroughly opened before the ooze can sufficiently penetrate them. For this purpose, while the hides are in the putrescent state, from being allowed to heat in the manner already mentioned, and well soaked in an alkaline ley, they are thrown into a sour liquor, generally brewed from rye, that the effervescence which necessarily ensues may open the pores. The tanners term this operation raising, as the leather is considerably swelled, in consequence of the conflict between the acid and alcali. This is an English invention: for it appears from M. de la Lande, who was employed by the Royal Academy of Sciences to write on the art of tanning, that the foreign tanners know nothing of this branch of the business: indeed, their whole process, according to his account, is slovenly, and even more tedious than our common method, and must make but very indifferent leather.

When the raising is accomplished, the leather is put into the handlers, and worked in them for the requisite time; then laid away in the vats, and there left to macerate till the tanning is found to be completely finished; which, for the heaviest kind of leather, such as this now spoken of, requires from first to last full 2 years. At least, the tanners of this country cannot make sole-leather in less time; what they are able to perform in England, I am not so thoroughly acquainted with. It is this tediousness of the process which enhances the value of leather; and the returns being so slow, the trade of tanning never can be carried on to advantage, but by persons possessed of a large capital; therefore, one sure way of increasing the number of tanners, and of course of bringing down the price of their manufacture, is to shorten the process; and if at the same time we can improve the quality of the leather, and save somewhat in the expence of tanning materials, the public will be essentially benefited in respect to one of the necessary articles of life.

Now all this can be done by pursuing the method laid down in the inclosed paper, and which may be introduced into any common tan-yard. With respect to time, it is possible, in the way that I have found out, to finish leather in a 4th part of what is required in the ordinary process; for I have repeatedly had calf-skins tanned in 2 or 4 weeks, which in the common way could not be done in less than as many months. I shall not pretend however to affirm, that the business can be carried on in the large way with such expedition; because a great deal of this abridgment of time was probably owing to frequent handling and working of the leather; but I am confident, and know it from 4 years experience,

that in the ordinary course of business, and in a common tan-yard, the tanner may save at least 4 months out of 12, produce better leather, and find his bark go much further, than in the old way of tanning.

Instructions to Tanners, for carrying on the new method of tanning, invented by Dr. Macbride, of Dublin; by which the leather is not only improved in its quality, but tanned in much less time, and with a smaller quantity of bark, than in any other method hitherto known or practised.—As the new method of tanning depends on this principle, “That lime-water extracts the virtues of oak-bark more completely than plain water;” the first thing in which the tanner is to be instructed, is the making of lime-water. 1. Provide a large vessel, in the nature of a cistern, whose depth shall be at least twice its diameter, and of a capacity adapted to the extent of the tan-yard. 2. This cistern must be fixed in a convenient corner of the yard, under a shed, and should stand so as that the liquor to be drawn off from it may run freely into the latches. 3. There must be a cock fixed in the side of the cistern, about a foot from the bottom, to let off the contents; and there must be a hole in the bottom of it, of 5 or 6 inches diameter, which is to be stopped with a plug. Let this hole open over a gutter. 4. The cistern must be covered with a flooring of boards, strong enough to bear a man’s weight; and from side to side of this lid there must be an opening of 2 or 3 feet wide. 5. If it can be so contrived that a water-pipe may be led into the cistern, it will save the servants a good deal of trouble; but if this cannot be done, a pump must be fixed in the most convenient way, for the purpose of filling it from time to time. 6. The cistern being once fixed (which is all the additional apparatus that the new method of tanning requires) the making of lime-water will be found extremely simple and easy. 7. You are first to fill the cistern with water, and then, for every hogshead that it may contain, throw in 10 or 12 lb. weight of unslaked lime. 8. Mix the lime thoroughly with the whole body of the water, by stirring it exceedingly well from the bottom, with a bucket and plunger, till you perceive that the lime is completely diffused, and the whole mixture becomes as white as milk: leave it then to settle for a couple of days, that the undissolved part of the lime may entirely subside, and the water become perfectly limpid, and clear as rock-water. The lime-water will then be fit for immediate use.

9. The cock, as already mentioned, is to be fixed at least 12 inches from the bottom of the cistern, in order that only the limpid part of the lime water may run off; and the use of the hole in the bottom, which is ordered to be stopped with a plug, is to let off the gross and insoluble remains of the lime, as often as may be found necessary to clean out the cistern. 10. When the first brewing (as it may be termed) of lime-water is all expended, you are to fill up the cistern with water a 2d time; stir up the lime from the bottom with the bucket and

plunger, so as to mix it thoroughly with the whole body of the water, as before directed, and then leave it to subside the requisite time. Thus you will have a 2d brewing of lime-water: and you may go on in the same manner to make a 3d, 4th, 5th, or perhaps a 6th, or more brewings, from the original quantity of lime; provided you find the lime-water continue sufficiently strong.

11. There are 2 ways of knowing when lime-water is sufficiently strong. The one is by the taste, and this a little practice will teach you to distinguish; the other is, by observing a certain solid scum, like the flakes of very thin ice, which collects and forms itself on the surface of the lime-water. As long as you find this solid scum floating on the top of the water in the cistern, so long you may conclude that there is no necessity for throwing in fresh lime. 12. But when the scum ceases to appear, or you find from the taste that the lime-water is not so strong as it ought to be, you must then take out the plug from the bottom of the cistern, and clear it by sweeping away the gross remains of lime: and after you have cleaned the cistern, begin your brewings of lime-water a new, and proceed in the manner already directed, as to stirring up the lime, and leaving it to settle for the necessary time, so as to have the lime-water perfectly limpid. In this manner you may go on from year to year, and constantly keep yourself in stock with respect to lime-water.

13. It is this lime-water which is now to be used in making your ooze instead of the plain common water; and this is all the difference between the old and the new method of tanning; for when the ooze is prepared, by steeping the bark in lime-water (in the latches, as you do at present, only running it through 2 latches) you are to make use of it in the very same way that you have hitherto used the common ooze. Every thing that relates to cleaning, liming, fleshing, &c. is to be conducted precisely as in the old or common method of tanning; and the goods are to be worked in the handlers for the requisite time, and then laid away in the vats, with layers and heading of bark, just as now practised, and when the leather is sufficiently penetrated with the ooze, that is, completely tanned, you will take it up, dry it, and afterwards dress it according to the different uses for which it is intended. Always observing however, that the ooze is to be turned from one latch on another before it is used, otherwise it will be apt to blacken the leather.

14. What has been hitherto said relates only to butts and calf-skins: as to sole-leather, which is prepared for the ooze by steeping it in some sour liquor, in order to open its pores, and raise it (according to the tanner's phrase) the new method requires a different practice from the old one. 15. In the old method, tanners made use of sourings brewed generally from rye, or some other grain; but these liquors are not only troublesome to brew and to ferment, but they are always uncertain as to their degree of sourness or strength, which depends on

the state of the weather, and other variable circumstances; these liquors are also exceedingly apt to rot the leather, and, without great care, may injure it very materially in its texture. 16. To obviate these inconveniences, you are to imitate the bleachers of linen, who make use of a sour prepared by diluting the strong spirit of vitriol (vulgarly, but improperly, termed oil of vitriol) with a sufficient quantity of plain water.

17. It was not without much difficulty that the bleachers could be prevailed on to quit their old sourings, made either of rye or barley, or of sour butter-milk, from a groundless fear, that the vitriolic souring would corrode their cloth; but the experience of many years has convinced them of their error, and now no other souring is used. In like manner the tanners at first may some of them be afraid to use the vitriol, but a little practice will show how far superior this souring is to what they have hitherto used. They will never find it subject to any change in respect to strength from variations of weather, or different degrees of heat; and so far from tending to rot the leather, it gives unusual firmness; and the soles which are raised by the vitriolic souring are remarkably sound, and always free from the slightest degree of rottenness. Besides, the same sour may do for many parcels of leather, by adding a little vitriol to it; and it need only be thrown away, when it becomes too dirty for use, by the frequent succession of hides.

18. A wine pint of the strong spirit of vitriol, which will not cost more than 9 or 10 pence, is sufficient for 50 gallons of water, to prepare the souring at first: therefore all you have to do, in raising the soles, is only to prepare them before-hand in the usual way; and, when they are fitted for the souring, mix up a quantity of vitriol and water, according to the number of hides required to be raised, still observing the proportion of a pint to 50 gallons, which will be enough, if the vitriol be of the due degree of strength. The hides may lie in the souring till you find them sufficiently raised, for they will be in no danger of rotting, as they would be in the common corn sourings, which in time might turn putrid, and rot the leather; whereas the vitriolic souring keeps off putrefaction.

19. When the hides are sufficiently raised, put them directly into the ooze, and go on with the tanning as in the old way; and you will see that the lime-water ooze penetrates raised leather even faster than it does butts or calf-skins, allowance being made for their different degrees of thickness. 20. Let it be now supposed that you have your cistern fixed, your lime-water prepared, and some latches full of lime-water ooze, which has been run through 2 latches, in order that the lime-water may completely spend its force on the bark; you are not to throw away what common ooze you have in stock in the yard, but only as it

shall be spent ; then indeed you are to throw it away, and supply its place with the lime-water ooze.

21. In a very few days you will perceive the difference between the activity of the two oozes, the new and old, with respect to penetrating the leather ; and thus, without any kind of loss or waste, you will get rid of all your old liquors, and come speedily into a full stock of the ooze made with lime-water ; and after you have got the new method established, your business will go in a regular course, and one parcel of goods will succeed another, as fast as you can manufacture and dispose of them. 22. Though it is possible to tan small parcels of leather, by way of experiment, by the use of lime-water ooze, in a 4th part of the time required, if only common ooze be made use of ; yet the business of a large tan-yard cannot be carried on with so much expedition : but even in large works, and in the common course of business, sole leather can be completely tanned and finished, in from 11 to 15 months, according to the different weight and thickness of the hides. Buts in, from 8 to 12 months, and calf-skins in, from 6 to 12 weeks ; in general, the tanner may save at least a 3d of the time that has hitherto been required.

23. The leather, manufactured in the new way, is of a superior quality to that of the old tannage, especially the sole-leather, which wears remarkably well, and never shows the least sign of rottenness. 24. Let it always be remembered, that the lime-water is never to be used but when it is sufficiently strong, and as clear as rock water. 25. Whenever you make fresh ooze, you must always use fresh lime-water, and run the ooze through 2 letches ; and the lime-water ooze, when spent, from lying on the leather, is never to be returned back on the bark which is in the letches (as you now return your spent ooze) but must always be thrown away, as being entirely useless ; for which purpose you must contrive a gutter in the tan-yard to carry off the spent ooze.

26. The letches ought to be under cover, lest the rain get into them and weaken the ooze ; and if the handlers are sheltered, it will be so much the better ; but it is of no importance to cover the vats, provided when the leather is laid away in them, they are kept constantly full to the brim. 27. You must always take care to have a sufficient stock of unslaked lime by you (for if it be slaked, it will not answer to make lime-water :) therefore, get your lime fresh, if possible, from the kiln, and immediately pack it in any kind of old dry casks. Weigh one of these casks, and it will enable you to ascertain the quantity of lime necessary to be thrown into the cistern each time you begin a fresh brewing of lime-water, and thus save the trouble of repeated weighings ; not that there need be much nicety about the quantity of lime, a score of pounds over or under making no sensible difference in the strength of the lime-water. 28. Any ex-

pence you may be at in procuring lime, which even in the largest tan-yards can amount but to a trifle, will be amply compensated by the saving of bark; because lime-water so completely exhausts the bark, and makes it go so much farther than when the ooze is made only of plain water. As a proof of this, you may make a pretty strong ooze from the tan or spent bark, which you now consider as completely exhausted, by infusing it in lime-water.

IX. Observations on the Population and Diseases of Chester, in the Year 1774.

By J. Haygarth, M. D. p. 131.

The facts ascertained in the following tables prove Chester to be healthy in such an uncommon degree, as will astonish those who are best acquainted with the general state of mortality in large towns. In order to deduce satisfactory and useful conclusions from these facts, it seems necessary to describe a few peculiarities in the situation of this city, which probably contribute to produce a salutary effect. The intelligent reader will remark, in the following account, that the structure of Chester prevents, in an uncommon degree, 2 principal sources of disease, stagnant moisture and putrefaction.

Chester is placed on a red, sandy, mouldering rock, which forms a rising promontory, whose summit is elevated exactly 100 feet above high-water mark, and 40 feet above the adjacent country; from this point the streets have a gentle declivity every way to the edge of the rock, whence there is a perpendicular fall of many yards from every part of the town.

The loose rock on which the town is built absorbs moisture: for being cut into filtering stones, water soon passes through its pores. The principal streets that meet in the centre of the city, are deeply excavated out of the rock, being sunk 6 or 9 feet lower than the surface of the ground. By this structure the foundations of the houses are kept perfectly dry, as the streets quickly drain off the water, and the rock absorbs all the remaining moisture. For these reasons the cellars in general are dry, a circumstance that greatly contributes to health. Stagnant water in a cellar is probably very often the unsuspected cause of putrid diseases: its pernicious influence seems to resemble, in some degree, that of bilge-water in a ship. There is a form of building peculiar to Chester, called the rows, which are covered galleries that make a complete communication between most of the principal streets. The rows are always dry and clean, even in wet and dirty weather; they moderate the heat of summer, and the coldness of winter. These uncommon advantages oftener tempt abroad persons of a delicate and valetudinary constitution, whether they be engaged in business or amusement; by which they obtain the benefit of fresh air and exercise, without incurring danger from catching cold.

The walls are near 2 miles in circumference, and surround the central part of

the city: they are dry and clean immediately after the heaviest rains. The rows form a dry communication with the walls from nearly every place within their circuit: their frequent ascents and descents: their elevated, airy situation, and varied prospects; all contribute to render walking on them peculiarly well adapted to preserve or restore health. The Dee, a large navigable river, divides a small part of the town from the rest, skirts the less, and surrounds three quarters of the larger portion. Where it makes this division, it falls over a causeway, forming a widely extended cascade, and then runs with rapidity down loose rocks; the whole descent is 13 feet. The tide always flows up to the town, where it rises, on a medium of spring tides 15 feet: the highest tides 21 feet: every new and full moon, about 6 or 8 tides flow over the causeway, and sometimes more than 20 miles above the town. Besides washing away the liquid filth, which quickly runs into the river by a short course from nearly all quarters of the town, the agitation of the waters, both by the cascade and tides, is probably of further service in purifying the atmosphere.

The air of Chester is uncommonly clear. In a register of the weather, kept for the last 4 years, there were only 6 foggy and 32 hazy mornings. In general the atmosphere on the western is much clearer than on the eastern shore of Britain, though more rain falls on the west than on the east side of the island.

That the inhabitants of Chester should have nearly an equal chance of living to twice the age of the inhabitants of Vienna, London, or Edinburgh; and that no large town, as far as inquiries have been hitherto made, should approach to a nearer proportion of longevity than as 28 to 40, are astonishing facts. The centre is by far the most salubrious part of the city: the average of deaths within the walls is only 1 in 58, a degree of longevity much superior to what in general is recorded even of the country.

Dr. Price, in his Observations on Annuities, has adduced numerous facts to prove that women live longer than men. These tables afford many confirmations of the remark. There died this year, under 20 years old, 162 males and 149 females, that is, a majority of 13 males; 52 husbands and 50 wives, that is 2 more husbands; 28 widowers and 48 widows, which is only a majority of 20 widows; though by the general survey, table 2, there are in Chester 258 widowers and 736 widows, or near 3 times the number. The total of males is 6697, of females 8016, hence there is 1319 or nearly a 5th majority of females: it may not be improper also to observe, that the women, especially in the higher and middle ranks of society, are remarkably beautiful. These facts clearly prove, that the manners and situation of Chester are peculiarly favourable to the female constitution.

Other observations may be deduced from these tables, which confirm, correct, or illustrate, various questions of importance to society. The number of mar-

ried persons in Chester is 4881, of unmarried 9832, that is, nearly $\frac{1}{3}$ is married, which is a common proportion. Upwards of $\frac{1}{2}$ of the inhabitants above 15 years old are or have been married, the proportion being as 4 to 7. Though Chester is so uncommonly healthy, yet this, like most other great towns, is unfavourable to population. Thus it appears, from the general bill for 10 years, that, on an average, one marriage produces less than 3 children. One cause of this small proportion is probably the want of manufactures, which might enable the lowest class of people to marry in earlier youth: taking the whole town, the number of persons in each family is $4\frac{1}{3}$. The inhabitants under 15 years old are 4486, that is, more than a 3d. The proportion of deaths this year to the number of inhabitants, is nearly as 1 to 27: this difference from the common degree of health is occasioned by the unusual fatality of the small-pox. The 3 tables show that the greater mortality of the summer than the winter quarter of 1774, was occasioned by the epidemic small-pox, which began in July: yet still that winter and autumn taken together, were more fatal than the spring and summer in the proportion of 326 to 220, that is, near a 6th more died in the former than in the latter portion of time.

There is a general prejudice in Chester, that it is unhealthy to inhabit the Rows; a prejudice most clearly refuted by many of the preceding observations. The Rows run along the central streets, which include incomparably the most healthy part of the town. That the centre is the most healthy part of the city; that a less proportion die annually here than in most country villages; and, as far as observations have hitherto been made, that it is probably as healthy as any spot upon earth, are surprizing facts: yet these facts are clearly evinced by the united evidence of 6 separate districts taken on a medium of 10 years.

By the tables it appears, that from birth to about 20 years of age, more males die than females; but that after that age, more females die than males.

TABLE. I.

State of Population, Small-pox, and Fevers, in 1774.

Parishes,	Families.	Inhabitants.	Males.	Females.	Married.	Widowers.	Widows.	Under 15 yrs old	Above 70 yrs old	Recovered small pox in 1774.	Dead of small- pox in 1774.	Ill of small-pox in Jan. 1775.	Not had small- pox in Jan. 1775.	Recovered fever in 1774.	Dead of fever in 1774.	Ill of fever in Jan. 1775.
St. Oswald's, ..	924	4027	1914	2113	1340	64	189	1302	143	321	40	5	350	58	2	1
John's,	774	3187	1411	1776	1057	51	190	970	153	284	52	6	218	55	4	3
Mary's,	583	2392	1097	1295	892	41	89	805	100	240	45	3	205	70	5	8
Trinity,	330	1605	730	875	485	43	95	521	65	127	24	3	97	9	—	1
Peter's,	193	920	414	506	267	8	28	221	43	52	6	—	39	15	1	3
Bridget's, ..	154	623	283	340	218	7	27	170	26	52	6	1	35	4	2	—
Martin's, ..	154	611	280	331	230	12	30	164	30	47	18	—	35	9	3	5
Michael's, ..	135	575	239	336	152	22	40	130	30	15	2	—	31	4	2	—
Olave's,	134	536	246	290	194	4	37	185	21	42	8	1	43	22	9	3
Cathedral, ..	47	237	83	154	46	6	11	18	14	3	1	—	7	1	—	—
Total	3428	14713	6697	8016	4881	258	736	4486	625	1183	202	19	1060	257	28	24

TABLE II.

The proportionable number of inhabitants that die annually in the following places.

Whites in Jamaica 1 in 5	Amsterdam..... 1 in 24	Country Parishes.
Vienna..... 1.. 19 $\frac{1}{2}$	Breslaw 1.. 25	Pais de vaud 1 in 45
London 1.. 20 $\frac{3}{4}$	Berlin 1.. 26 $\frac{1}{2}$	Country parishes in Brandenburgh 1.. 45
Edinburgh 1.. 20 $\frac{4}{5}$	Northampton 1.. 26 $\frac{1}{2}$	Others in Brandenburgh 1.. 50
Leeds 1.. 21 $\frac{3}{5}$	Shrewsbury 1.. 26 $\frac{1}{2}$	A country parish in Hants for 90 yrs. 1.. 50
Dublin 1.. 22	Liverpool 1.. 27 $\frac{1}{2}$	Island of Madeira 1.. 50
Rome 1.. 23	Manchester..... 1.. 28	Stoke Damerel in Devon. for 1 yr. 1.. 54

IX. Some Electrical Experiments. By Mr. William Swift, of Greenwich. p. 155.

Mr. Swift contrived an electrical apparatus, to show the different effects of points and balls at the upper termination of conductors, to secure houses and magazines of powder from damage by lightning. He represented the clouds by interposing 3 feet of water insulated, instead of continuing the metal from the prime-conductor; which he apprehended to be analogous to the natural clouds. The clouds, being charged, slide on a frame with a graduated edge; and, as they pass the length of the frame, they make 5 revolutions round their own axis; for they are represented by a semi-circle, the radius of which is 18 inches, consequently its extent is nearly $4\frac{1}{2}$ feet, and formed with materials well covered with metal. He placed 3 houses at a certain distance from the frame, and equally distant from each other. Each house has a conductor, and is connected with magazines of powder, called a, b, c; the reason for making the clouds a semi-circle is, that when turned back they may be charged from the machine, without affecting, or being affected by, the points or balls on the tops of the houses A, B, and c; and, by means of their motion round their own axes, he could increase or diminish at pleasure the velocity. He fixed an electrometer on one of the conductors of the machine, and put points for the upper terminations of the conductors of the houses. Having thus prepared the machine, the semi-circular cloud being turned back, that is, within; the machine is charged till the index of the electrometer rises upwards of 90° ; the cloud being then put in motion, as it slides along the frame, revolves over the house A, with its length of $4\frac{1}{2}$ feet: in its passage it empties itself, the electrometer falling to 0, but not the least explosion is perceived. The cloud then turning back in its progressive motion in the frame, is charged again while it passes on to B; at which point, by means of its motion round its axis, it revolves over the conductor B; it empties itself, the electrometer falls, and no explosion is perceived: the same thing happens in the passage over the house c.

The machine remaining in the position as before, he placed balls of $\frac{1}{4}$ of an inch diameter, at the upper terminations of the conductors of the houses A, B, c, and with these balls, the experiments proceed almost as before; that is, the matter passes off with a little hissing noise, and now and then it gives a slight explosion, the smallness of these balls differing little from points. But

when he placed balls of $\frac{3}{4}$ of an inch diameter, instead of the small ones, the cloud, every time it passed over them, made explosions, and fired the magazines a, b, c; and yet the index of the electrometer does not descend above 20° , and starts up again as suddenly as it fell.

If balls were safer at the upper ends of conductors than points, it should follow, that the larger the balls are, the greater the security; but from all these experiments he never found a shock with a point, and not always with a very small ball: but the electrical matter passes off silently with the points, and so entirely, that the electrometer falls to 5° . With balls $\frac{1}{4}$ of an inch diameter indeed it passes off with a little hissing noise, but this seldom amounts to a shock: but with balls $\frac{3}{4}$ of an inch diameter an explosion constantly happens, and the magazines are fired. To put this matter still more out of doubt, he placed a ball of 9 inches diameter on one of the conductors, and the explosion was very violent, always more certain; and yet the machine does not discharge itself, for the electrometer falls not more than 20° .

The next experiment he makes with the water-conductor is, placing the houses A, B, C, in a negative state, by connecting them with the cushion of the machine, or with the outside of a battery: when the cloud is charged and passes over the houses, with points at the upper end of their conductors, there is no explosion; the points seem to draw off all the electrical matter during the passage of the clouds of $4\frac{1}{2}$ feet long: but when, in this position of the houses, balls of $\frac{3}{4}$ of an inch diameter are placed instead of points, there is a small explosion, and a considerable residuum of the matter is left in the battery. He then changes the insulated water for wire to complete the circle: on the passage of the clouds over the houses there is a considerable explosion, whether points or balls are the upper terminations of the conductors of the houses; but no residuum is left in the battery. Hence appears the difference of effect, whether the houses stand in a state of nature, or in a negative state; and whether the conductors be made complete with wire, or water insulated.

I have, says Mr. Swift, by 16 years practice been convinced how difficult it is to draw general conclusions from any electrical experiments, and therefore it becomes me to propose my conjectures with the greatest diffidence; but I apprehend the result of many experiments show, that points at the upper termination of conductors gradually diminish or draw off the electrical matter, so as to prevent any damage to the buildings on which they are placed, by preventing a violent explosion; and that, on the contrary, balls, though perhaps they will repel the electrical matter in some degree, yet from that very circumstance probably the explosion, when it happens, is violent and attended with danger.

X. *An Account of the Island of Sumatra, &c.* By Mr. Charles Miller. p. 160.

This paper contains extracts of several letters from Mr. Charles Miller (son of the late botanic gardener) now settled at Fort Malbro' near Bencoolen; giving some account of that place, also of the interior parts of Sumatra, and of a neighbouring island never known to have been visited by any European.

The houses at Fort Marlborough, near Bencoolen, are almost all built, ceiled, roofed, and floored, with a kind of reed called bamboo, and thatched with the leaves of the sago-tree, and would all be called cottages in England, making a very mean appearance. They are placed in no kind of order; most of them are raised from the ground on wooden or brick pillars 6 or 8 feet high; within they are not much unlike a set of rooms in a college, as they consist of one large room called a hall, out of which two doors lead, the one to a bed-room, and the other to an office or study. The climate is far from being so disagreeably hot as it is represented to be, or as one might expect from our vicinity to the line; the thermometer is never lower in a morning at 6 than 69° , or higher than 76° . At noon it varies from 79° to 88° ; and at eight P.M. from 73° to 78° or 80° .

The people who inhabit the coast are Malays, who came hither from the peninsula of Malacca: but the interior parts are inhabited by a very different people, and who have hitherto had no connection with the Europeans. Their language and character differ much from those of the Malays; the latter using the Arabic character; and all the interior nations, though they differ from each other in language, use the same character. The people between the districts of the English company, and those of the Dutch at Palimban on the other side the island, write on long narrow slips of the bark of a tree, with a piece of bamboo; they begin at the bottom, and write from the left hand to the right, which I think is contrary to the custom of all other eastern nations. This country is very hilly, and the access to it exceedingly difficult, there being no possibility of a horse going over the hills. The inhabitants have almost all of them, particularly the women, large swellings in the throat, some nearly as large as a man's head, but in general as large as an ostrich's egg, like the goitres of the Alps. It is by them said to be owing to their drinking a cold white water; probably some mineral water. Near their country is a volcano: it is very mountainous, and abounds with sulphur. If this distemper be produced here by this cause, perhaps in the Alpine countries it may take its origin from a similar one, and not, as has been imagined, from snow-water: certain it is, there is no snow here to occasion it. In almost all the central parts from Moco-moco northwards, they find gold, and some iron; but this distemper is unknown there. Mr. M. met here with a rivulet of a strong sulphurated water, which was so hot, a quarter of a mile below its source, that he could not walk across it.

The country called the Cassia country lies in latitude 1° north inland of our settlement of Tappanooly: it is well inhabited by a people called Battas; who

differ from all the other inhabitants of Sumatra in language, manners, and customs. They have no religious worship, but have some confused idea of 3 superior beings; 2 of which are of a benign nature; and the 3d an evil genius, whom they style Murgiso, and to whom they use some kind of incantation to prevent his doing them hurt. They seem to think their ancestors are a kind of superior beings, attendant on them always. They have no king, but live in villages [Compongs] absolutely independent of each other, and perpetually at war with each other: their villages they fortify very strongly with double fences of camphire plank pointed, and placed with their points projecting outwards, and between these fences they put pieces of bamboo, hardened by fire, and likewise pointed, which are concealed by the grass, but will run quite through a man's foot. Without these fences they plant a prickly species of bamboo, which soon forms an impenetrable hedge. They never stir out of these Compongs unarmed; their arms are matchlock guns, which, as well as the powder, are made in the country, and spears with long iron heads. They do not fight in an open manner, but way-lay and shoot or take prisoner single people in the woods or paddy-fields. These prisoners, if they happen to be the people who have given the offence, they put to death and eat, and their skulls they hang up as trophies in the houses where the unmarried men and boys eat and sleep. They allow of polygamy: a man may purchase as many wives as he pleases; but their number seldom exceeds 8. They have no marriage ceremony; but, when the purchase is agreed on by the father, the man kills a buffalo or a horse, invites as many people as he can; and he and the woman sit and eat together before the whole company, and are afterwards considered as man and wife. If afterwards the man chuses to part with his wife, he sends her back to her relations with all her trinkets, but they keep the purchase-money; if the wife dislikes her husband, her relations must repay double the purchase-money. A man detected in adultery is punished with death, and the body eaten by the offended party and his friends: the woman becomes the slave of her husband, and is rendered infamous by cutting off her hair. Public theft is also punished with death, and the body eaten. All their wives live in the same house with the husband, and the houses have no partition; but each wife has her separate fire-place. Girls and unmarried women wear 6 or 8 large rings of thick brass wire about their neck, and great numbers of tin rings in their ears; but all these ornaments are laid aside when they marry.

They often preserve the dead bodies of their Rajas (by which name they call every freeman that has property, of which there are sometimes one, sometimes more, in one Compong, and the rest are vassals) for 3 months and upwards, before they bury them: this they continue to do by putting the body into a coffin well caulked with dammar, a kind of resin: they place the coffin in the

upper part of the house, and having made a hole at the bottom, fit to it a piece of bamboo, which reaches quite through the house, and 3 or 4 feet into the ground: this serves to convey all putrid moisture from the corpse, without occasioning any smell. They seem to have great ceremonies at these funerals; but they would not allow Mr. M. to see them. He saw several figures dressed up like men, and heard a kind of singing and dancing all night before the body was interred: they also fired a great many guns. At these funerals they kill a great many buffaloes; every Raja, for a considerable distance, brings a buffalo and kills it at the grave of the deceased, sometimes even a year after his interment; he assisted at the ceremony of killing the 106th buffalo at a Raja's grave. The Battas have abundance of black cattle, buffaloes, and horses, all which they eat. They also have great quantities of small black dogs, with erect pointed ears, which they fatten and eat. Rats and all sorts of wild animals, whether killed by them or found dead, they eat indifferently. Man's flesh may rather be said to be eaten in terrorem, than to be their common food; yet they prefer it to all others, and speak with peculiar raptures of the soles of the feet and palms of the hands. They expressed much surprize on being informed that white people did not kill, much less eat, their prisoners.

It is from this country that most of the cassia sent to Europe is procured. The cassia tree grows to 50 or 60 feet, with a stem of about 2 feet diameter, with a beautiful regular spreading head. Camphire and benjamin trees are in this country in great abundance; the former grows to the size of our largest oaks, and is the common timber in use: some of these trees are nearly 100 feet high. Its leaves are acuminate and very different from the camphire tree seen in the botanic gardens, which is the tree from which the Japanese procure their camphire by a chemical process; whereas in these trees the camphire is found native in a concrete form. Native camphire sells here at upwards of £200. per cwt. to carry to China; what the Chinese do to it, he cannot say; but, though they purchase it at £250. or £300. they sell it again for Europe at about a quarter of the money. Mr. M. never saw the flower of the camphire tree; some abortive fruit he had frequently found under the trees, they are in a cup, like an acorn, but the laciniae calycis are 4 or 5 times longer than the seed.

It is amazing, says Mr. M., how poor the Fauna of this country is, particularly in the mammalia and aves. There are abundance of the simia gibbon of Buffon: they are quite black, about 3 feet high, and their arms to the ground when they stand erect; they walk on their hind legs only. Mr. M. had seen hundreds of them together on the tops of high trees. There are several other species of the simia also. The ourang outan, or wild man (for that is the meaning of the words) he heard much talk of, but never saw any; nor any of

the natives that have seen it. The tiger is to be heard of in almost every part of this island: Mr. M. had never seen one, though he had frequently heard them when he had slept in the woods, and often seen the marks of their feet. They annually destroy near 100 people in the country where the pepper is planted; yet the people are so infatuated that they seldom kill them, having a notion that they are animated by the souls of their ancestors. Of tiger-cats there are 2 or 3 sorts; elephants, rhinoceros, elks, one or two other kind of deer, buffaloes, two or three sorts of mustelæ, porcupines, and the small hog-deer, almost complete the catalogue of the mammalia. Birds he had seen very few indeed, and very few species of insects. Ants, of 20 or 30 kinds, abound so much, as to make it impossible to preserve birds or insects. He had frequently attempted it, but in vain. He met with one instance, and one only, of a stratum of fossil shells. He had some notion that it was an observation (of Condamine he thinks) that no such thing was to be found between the tropics.

The island of Enganho, though situated only about 90 miles to the southward of Malbro', was so little known, on account of the terrible rocks and breakers which entirely surround it, that it was even doubtful whether it was inhabited: to this island Mr. M. made a voyage. With great difficulty and danger they beat up the whole south-west side of it, without finding any place to attempt to land. At last however they discovered a spacious harbour at the south-east end of the island, and Mr. M. immediately went to it in the boat, and ordered the vessel to follow as soon as possible, for it was then a dead calm. They rowed directly into this bay; and as soon as they had got round the points of an island which lay off the harbour, they discovered all the beach covered with naked savages, who were all armed with lances and clubs; and 12 canoes full of them, who, till they had passed them, had lain concealed, immediately rushed out on him, making a horrid noise: this alarmed them greatly; and as Mr. M. had only one European and 4 black soldiers, besides the 4 lascars that rowed the boat, he thought it best to return, if possible, under the guns of the vessels, before he ventured to speak with them. The canoes however, after having pursued for more than a mile, luckily stopped a little to consult together, which gave him an opportunity to escape them, as they did not care to pursue out to sea. The same afternoon the vessel came to an anchor in the bay, and they were presently visited by 50 or 60 canoes full of people. They paddled round the vessel, and called to them in a language which nobody on board understood, though he had people who understood the languages spoken on all the other islands. They seemed to look at every thing about the vessel very attentively; but more from the motive of pilfering than from curiosity, for they watched an opportunity and unshipped the rudder of the boat, and paddled away with it. He fired a musket over their heads, the noise of which frightened

them so, that they all leaped into the sea, but soon recovered themselves and paddled off.

They are a tall, well-made people; the men in general about 5 feet 8 or 10 inches high; the women shorter and more clumsily made. They are of a red colour, and have straight, black hair, which the men cut short, but the women let grow long, and roll up in a circle on the top of their heads very neatly. The men go entirely naked, and the women wear nothing more than a very narrow slip of plaintain leaf. The men always go armed with 6 or 8 lances, made of the wood of the cabbage-tree, which is extremely hard; they are about 6 feet long, and topped with the large bones of fish sharpened and barbed, or with a piece of bamboo hardened in the fire, very sharp pointed, and its concave part armed with the jaw bones and teeth of fish, so that it would be almost impossible to extract them from a wound. They have no iron or other metal that he could see, yet they build very neat canoes; they are formed of 2 thin boards sewed together, and the seam filled with a resinous substance. They are about 10 feet long, and about a foot broad, and have an outrigger on each side, to prevent their over-setting. They split trees into boards with stone wedges. Their houses are circular, supported on 10 or 12 iron-wood sticks about 6 feet long; they are neatly floored with plank, and the roof rises immediately from the floor in a conical form, so as to resemble a straw bee-hive; their diameter is not above 8 feet.

These people have no rice, fowls, or cattle, of any kind: they seem to live on cocoa-nuts, sweet potatoes, and sugar-canes. They catch fish, and dry them in the smoke; these fish they either strike with their lances, or catch in a drawing net, which they make very neatly. They do not chew betel, a custom which prevails universally among the eastern nations.

I went on shore, says Mr. M., after the vessel anchored in the bay, hoping to be able to see something of the country, and to meet with some of the chiefs. I saw a few houses near the beach, and went towards them; but the natives flocked down to the beach, to the number of 60 or 70 men, well armed with their lances, &c. and put themselves in our way; yet, when we approached them, they retreated slowly, making some few threatening gestures. I then ordered my companions to halt and to be well on their guard, and went alone towards them: they permitted me to come among them, and I gave them some knives, pieces of cloth, and looking-glasses, with all which they seemed well pleased and allowed me to take from them their lances, &c. and give them to my servant, whom I called to take them. Finding them to behave civilly, I made signs that I wanted to go to their houses and eat with them; they immediately sent people who brought me cocoa-nuts, but did not seem to approve of my going to their houses: however I determined to venture thither, and seeing a path leading

towards them, I went forward attended by about 20 of them, who, as soon as we had got behind some trees, which prevented my people seeing us, began to lay violent hands on my clothes, and endeavour to pull them off; but having a small hanger, I drew it, and, making a stroke at the most officious of them, retreated as fast as possible to the beach. Soon after we heard the sound of a conch-shell; on which all the people retired, with all possible expedition, to a party of about 200, who were assembled at about a mile distance. It was now near sun-set, and we were near a mile from our boat; and, as I was apprehensive we might be way-laid in our return if we staid longer, I ordered my people to return with all possible speed; but first went to the houses the natives had abandoned, and found them stripped of every thing; so that I suppose this party had been amusing us while others had been employed in removing their wives, children, &c, into the woods. I intended to have attempted another day to have penetrated into the country, and had prepared my people for it; but the inconsiderate resentment of an officer who was sent with me, rendered my scheme abortive. He had been in the boat to some of the natives who had waded out on a reef of rocks and called to us; they had brought some cocoa-nuts, for which he gave them pieces of cloth: one of them seeing his hanger lying beside him in the boat, snatched it and ran away; on which he fired on them, and pursued them to some of their houses, which, finding empty, he burnt. This set the whole country in alarm; conch-shells were sounded all over the bay, and in the morning we saw great multitudes of people assembled in different places, making use of threatening gestures; so that finding it would be unsafe to venture among them again, as, for want of understanding their language, we could not come to any explanation with them, I ordered the anchor to be weighed, and sailed out of the bay, bringing away 2 of the natives with me.

When at Tappanooly I saw what I find in Purchas's Pilgrim called the wonderful plant of Sombrero: his account however is somewhat exaggerated, when he says it bears leaves and grows to be a great tree. The name by which it is known to the Malays is lalan-lout, that is, sea-grass. It is found in sandy bays, in shallow water, where it appears like a slender straight stick, but when you attempt to touch it, it immediately withdraws itself into the sand. I could never observe any tentacula: a broken piece, near a foot long, which, after many unsuccessful attempts, I drew out, was perfectly straight and uniform, and resembled a worm drawn over a knitting-needle; when dry it is a coral.

The sea cocoa-nut, which has long been erroneously considered as a marine production, and been so extremely scarce and valuable, is now discovered to be the fruit of a palm with flabelliform leaves, which grows abundantly on the small islands to the eastward of Madagascar, called in our charts Mahi, &c. and by the French Les Isles de Sechelles. To these islands the French have sent a

large colony, and planted them with clove and nutmeg-trees, as they have likewise the islands of Bourbon and Mauritius.

XII. A Meteorological Diary, &c. kept at Fort St. George in the East Indies.

By Mr. William Roxburgh, Assistant Surgeon to the Hospital at the said Fort. p. 180.*

The manner in which Mr. R. kept his meteorological observations was as follows: A thermometer without doors; a barometer and thermometer within doors: these were observed 3 times a day. Also the direction and strength of the wind, and the state of the weather. He distinguished 4 degrees of strength of the wind; namely, gentle, brisk, stormy, and what they call a tufoon in India, which are marked with the numbers 1, 2, 3, and 4, besides no sensible wind, which is marked with a cypher. The state of the barometer is very uniform, varying only between 29.13 and 30.04 inches. And the thermometer without between 66, the lowest, and 91, the highest.

XIII. Experiments on Air, and the Effects of Different Kinds of Effluvia on it, made at York. By W. White, M.D., F. S. A. p. 194.

Dr. W. describes the situation of York, and the soil about it, as very marshy and unhealthy. Then observes that the highest state of the barometer in the 3 last years was 30.58; the lowest, 28.20. Thermometer in the shade, highest, 81; lowest, 8. Having no ombrometer, Dr. W. only observes, in regard to rain, that in 1774 they had 193 days in which more or less rain fell; in 1775, 232 days; and in the last year, 240. The apparatus used in making the experiments was very simple: 1st, a vessel full of water, of a proper size and figure. 2dly, A common barometer tube of a large bore, so that an ounce phial full of air, being introduced into it, occupied at a medium 134 decimal parts of an inch; and on a further addition of a half ounce phial of nitrous air, 205: this tube is graduated by inches and decimals. 3dly, Glass funnels, with necks of such a size as to enter the tube.

The air, the subject of the experiment, was conveyed into the tube, by means of the glass funnel, under water; the nitrous air is then added to it by the same method. The space occupied by them both, immediately on mixture, is noted down, as also the time by a watch; after standing the appointed time (half an hour, except where it is mentioned otherwise) the space then occupied is marked down, which being deducted from the first, gives the result of diminution sought: for example, an ounce phial of air from a putrid plum, with the addition of half of nitrous air, took up the space of 195 (part of the first being absorbed

* Now Dr. Roxburgh, author of a splendid botanical work, entitled *Plants of the Coast of Comorandel*.

by the water in its passage through it); after half an hour, still 195; so that no diminution following, it was known to be mephitic. August 30th, the same quantity of the air of his garden, with the nitrous, occupied 205; after half an hour it was diminished to 145, which being deducted gave 60, the state of the air that day: and so of the rest. The medium state of the air of the atmosphere, in upwards of 200 experiments, was 60° or 61° .

Exp. 1. Sept. 13th, it was in the worst state Dr. W. ever observed it, 58° , the barometer being 30.30, thermometer 69, with a calm, clear sky, wind s. e. air dry and sultry, no rain having fallen for above a fortnight; on the same day a slight shock of an earthquake.—*Exp.* 3. Sept. 21, a high wind cleared the air, barometer 29.50, thermometer 52° , the air was 64° . It was the same Oct. 5, the wind high and westerly. This was the purest he ever observed.—*Exp.* 4. He only observed it so good as 68° in 3 instances, Aug. 16, Sept. 20 and 29; these were all showery days, with a brisk wind.—*Exp.* 5. As to the influence of the different winds on one atmospheric air, the experiments are as yet too few to ascertain it. He generally found it the purest during westerly winds, and the worst when it blew from the easterly points.—*Exp.* 6. The difference of the air a little way out of the city, from that in the city itself, is perceptible enough. Aug. 9th, the air of the city was 59° , beyond the city walls 62° . On the 11th of the same month, the first was 60° , the last 62° .—*Exp.* 7. Common air being briskly agitated with water for half an hour, was found to be made worse. In one experiment it was reduced from 59° to 57° ; in another, from 61° to 59° ; in a 3d, from 61° to 57° ; in a 4th, from 62° to 58° . Air obtained from glazier's putty by the nitrous acid was meliorated by the same process.

In order to find the effects of animal exhalations on air, the following experiments were made. *Exp.* 8. The air of his bed at night he found to be 62° , the next morning it was reduced to 58° ; this was several times repeated. The diminution here will appear very considerable on observing that it was the effect of the breath, &c. of a single person, in a large, airy room, the bed-curtains always open, except on the side facing the window, which is quite open to large gardens, and never shut with curtains. It fully shows the unwholesomeness of small rooms, close beds, &c. especially in diseases.—*Exp.* 9. Some air which he had respired as long as could be without manifest inconvenience, was by it reduced from 62° to 40° . This illustrates the preceding experiment.—*Exp.* 10. A small piece of fresh veal was put into a phial containing 8 ounces of common air, and suffered to remain 24 hours: the flesh was then perfectly sweet, but the air was much injured, being diminished from 64° to 55° . Being left together 24 hours longer, the air was reduced to 10° , or rendered nearly mephitic; yet the flesh was not putrid, only smelling rather faint and musty. It is evident, from hence, that something had escaped from the flesh, while yet void of any putrid

smell, so as to render the air very noxious: he supposes this effluvia to be pure phlogiston. Hence it seems that this principle is capable of rising, per se, uncombined with the saline part of animal bodies, the union of which is supposed to give the putrid smell. It proves Sir John Pringle's supposition, that phlogiston, when single, is imperceptible to the smell; but it also shows it to be pestilential. In our experiments it was devoid of smell, consequently contained no mixture of volatilized acid; yet it had the common property of all putrid effluvia, that of rendering common air noxious.

Exp. 11. Air taken from within a privy was found in several experiments to be equally good with the common atmospheric air. The result of these experiments was contrary to expectation, and Dr. W. was not satisfied without making several trials. Sir John Pringle observes, that the fæces humanæ are perhaps in a natural state little if at all infectious. These experiments confirm the justness of his supposition. The recent excreta of a person in perfect health are here understood; in putrid diseases they must necessarily partake of the general state of the system, and become very noxious and infectious.

Exp. 12. The following experiments were made to discover the effects of vegetable effluvia on air. They were put into a phial of air, containing 8 ounces, immediately after being gathered out of the garden; the time of standing together half an hour, except in a few cases particularly noticed.

Flowers of ulmaria, diminished it from 63 to 52	Flowers of tree primrose, diminished it from 60 to 56
Ten-week stocks 63.. 53	Antirrhinum 60.. 57
Mignonette 60.. 54	Leaves of sage 61.. 55
Calendula vulgaris 60.. 54	Thyme 61.. 56
French ditto 60.. 55	Mint (common) 61.. 57
Nasturtium indicum 60.. 55	Ditto (pepper) 61.. 57
Carnations 60.. 56	Parsley 61.. 57

It is evident from these experiments, that vegetables, when fresh and vigorous, exhale a noxious matter in considerable quantity, which quickly renders common air noxious. This is most remarkable in the flowers, next in the leaves, and this in proportion to their firmness and texture.

Exp. 13. In the last experiments the air only stood in contact with the vegetables half an hour; let us see here what effect a longer time of standing together may have, viz. 16 hours.

Flowers of ulmaria diminished it from 60 to 2

Ten-week stocks 60. . . 1

Leaves of sage 61. . . 9

The vegetables were at the end equally sweet as when first gathered and put into the phial of air. These facts are very curious, interesting, and convincing. It is amazing that vegetables, while fresh and free from the least degree of putrescency, should have such a noxious tendency as to spoil the air, and render it not only useless but fatal to animal life, and that in so short a time. We have here

a striking example of the necessity of faithful experiments; by them alone we can add certainty to science, and develope nature in her most secret and abstruse operations; and as she is unchangeable in herself, every discovery extorted from her is immutable. For want of attention to this laborious but sole method of coming at truth, it is a pretty general opinion in the world, that even rotten vegetables are little noxious: and a late author, in a chapter on putrid fevers and infection, expressly says: "The effluvia of rotten vegetable matters have little effect in contaminating the air; from some experiments it appears, that they possess rather an antiseptic virtue." We know however, by fatal experience, that both animal and vegetable substances, when in a corrupted state, are the obvious sources of the most dreadful and alarming diseases, from the mildest putrid fever up to the plague itself. Sir John Pringle gives an instance of the gaol or hospital fever, caused by the infection of a gangrened limb. A dreadful fever was caused at Venice by a quantity of corrupted fish; and at Delft by putrid cabbages and other vegetables. Many instances of this kind may be brought, by which countries have been almost depopulated. But it is no wonder that animal and vegetable matter, when in a state of absolute corruption, should be pregnant with such dreadful effects. Instinct leads us to fly from the danger when we perceive the cadaverous smell.

The 9th, 10th, 12th, and 13th experiments demonstrate, that our senses are by no means capable of distinguishing infection, nor, by warning us of the danger, of leading us to avoid it. They show, that both animal and vegetable matter, when perfectly fresh, sweet, and devoid of putrescency, exhales somewhat of a very noxious nature, inducing a putrid state in the living body, which proves destructive to animal life. Hence in gaols, hospitals, and other crowded places, we ought not by any means to estimate their wholesomeness by the absence of disagreeable smells alone. The principle of disease may lurk there unperceived by our limited senses. The method used in these experiments is the only true one by which we may judge with some degree of safety.

The crowding together of a number of men in camps, hospitals, gaols, sick rooms, &c. will presently generate a most malignant and infectious fever; and in a very short time, especially if the place be close, unventilated, and the weather hot, the most fatal effects will follow. Of this we have a most remarkable example in the affair at Calcutta, when 146 people, in perfect health, were, by the effects of animal effluvia in a close and unventilated place, in the space of eleven hours, all destroyed except 23, and those in a high, putrid fever, of which however, by fresh air, &c. they gradually recovered. In all confined places, in proportion to their airiness, we find more or less of this. In hospitals, though the wards may give no marks of it by any apparent dirtiness or disagreeable smell, we may observe its effects; diseases which usually admit in

private practice of an easy cure, are often very tedious, and apt to assume anomalous symptoms. Healthy persons, admitted for the cure of recent wounds and other accidents, soon become pale, lose their appetite, and are generally discharged weak and emaciated, but soon recover by the benefit of fresh air. In some hospitals the cure of a compound fracture is rarely seen; in private practice, and a pure air, such cases seldom fail. Such and many more are the effects of bad air, which, though not virulent enough to cause a putrid fever in its more malignant form, is yet sufficient to excite it to such a degree as to undermine the constitutions of the patients, and render the disorders, for which they were admitted, anomalous, tedious, and fatal.

It has been demonstrated, that the effluvia of vegetables, even while perfectly sweet and fresh, are equally poisonous with those from animal substances. The vegetables were separated from their parent plant, consequently not in a growing or vegetating state.—*Exp.* 14. Being desirous of finding the effects of effluvia from ripe fruit upon air, 6 ripe gooseberries sliced were inclosed 16 hours in a phial with 8 ounces of common air: the air being then put to the test, was found to be diminished from 62° to 40°. Hence it appears that fresh fruit have, in common with other vegetable matters, a great power in polluting the air, and rendering it noxious.

Exp. 15. To find whether any part of the pernicious effects of vegetables on air in the 12th experiment might be owing to their odorous particles, the following experiments were made. In each, the quantity of inclosed air was 8 ounces, the time of standing together 16 hours.

Half a dr.	{	10 grains of musk diminished it from	63 to 62
		of Camphor	63 .. 62
		Assafoetida	62 .. 62
		Saffron	62 .. 62
		Opium	60 .. 58
		Vol. Sal. Ammoniac	60 .. 58

Musk and camphire were selected as examples of essential oils; the 1st of the animal, the 2d of the vegetable class. The assafoetida as an instance of the fetid odour; opium of the narcotic. Saffron, from its mode of preparation, is incapable of corruption while kept dry, and could give nothing but pure odour. The volatile salt was an example of the volatile odour. Hence we find, that pure odour has little if any effect, in polluting the air.

It is hence demonstrable, that the filling of rooms with nosegays and bunches of flowers is by no means a safe practice, especially in close rooms or sick chambers; their effluvia are of so noxious a nature as quickly to render the air unfit for the purposes of respiration, and cannot fail of having bad effects on sick and valetudinary people in particular. But it is also evident, that the odorous parts of vegetables, when separated by art from the putrescent, are by no means

hurtful. Hence, except in particular constitutions, or in cases where their stimulus may be hurtful, they may be safely used as agreeable odours, and to obviate the smell in sick rooms, &c. The volatile alkali, as Sir John Pringle observes, appears in this view perfectly innocent. What is here said is understood of plants gathered and separated from the roots. Dr. Priestley discovered a different property in them when in a vigorous, growing state; they then absorb from the atmosphere; but this ceases with their life, they then exhale putridity, and hasten to dissolution.

We come next to another, not less curious and important, part of our experiments; the effects of the effluvia from moist, marshy, and other kinds of soils, on air. This subject, as particularly connected with our art, regarding the endemic diseases of different countries, and a plentiful source of the most dangerous diseases, has much employed the attention of physicians and philosophers.

Exp. 16. The air over the river Ouze was constantly purer than that of his garden by 2 or 3 degrees.—*Exp. 17.* The same was observable in the air over the foss. This was at a time when, in consequence of floods, the current was pretty rapid, all the mud and marshy ground being covered to a considerable height with water.

Dr. W. next tried what effect the same waters might have on air, when confined together. Two ounces of the water were put into an 8 oz. phial, so that there were 6 oz. of air; being corked up, they were suffered to stand together 16 hours.—*Exp. 18.* The air from the Ouze water was equally good as at first; and this in several experiments.—*Exp. 19.* The same was the result in the foss water. It was perfectly free from mud, yet not so clear as the river water, and had some of the *lens palustris* swimming in it. Hence we find, that the air was not any way polluted by standing over the surface of water. Perhaps if longer time had been allowed in the 19th experiment, the *lens palustris* might have become putrid, and hurt the air.

Exp. 20. Some of the foss water was next tried; so foul as to deposit a muddy sediment on standing. In one experiment the air was reduced from 62° to 58° ; in another, from 62° to 57° ; in a 3d, from 60° to 56° .

Exp. 21. It has been observed by those physicians who have had the most opportunities of being acquainted with the diseases peculiar to low, stagnant, and fenny situations, that they seldom begin to appear till the water is so far evaporated, that the black and slimy mud begins to appear. In order to know this, the following experiments were made. Two ounces of the black stinking mud of the foss was put into the 8 ounce phial of air; being closed, they were suffered to remain together 12 hours. The air in 12 trials being put to the test, the results were as follow. In 7 experiments the air was reduced from 62° to 34° ; in 3, from 62° to 36° ; in others, from 60° to 35° . These are convincing

examples of the noxious effects of the effluvia from putrid bogs and marshes. Thus the experiments prove, that marsh effluvia are poisonous to a living animal; yet they may probably act as an antiseptic on the dead one. Fixed air is a powerful antiseptic in the one, but is deadly to the other. Nitrous air preserves all flesh from corruption after death; yet let any living animal but once breathe in it, and it instantly expires. Some of our bogs have the singular property of preserving dead bodies not only sweet but pliable for many years; but we are certain they are at the same time deadly to living animals.

Exp. 22. A 4th part of an 8 oz. phial was filled with the same mud as in the last experiment, but so much dried in the sun as to be easily rubbed into a powder, the rest being air; after being corked, they were set by for 24 hours, and in the interim frequently agitated. The air being at the end put to the test was scarcely altered, the greatest diminution in several different experiments was only from 62° to 60° . So that the air was yet quite good, though they stood double the time of that in the last experiment. Hence it is evident, that bogs and marshy grounds, when dry, or perfectly drained of their moisture, become healthy, and emit no noxious exhalations. This illustrates the observation, that such situations are not liable to produce their peculiar diseases during the dry seasons, or after being well drained. And it is observed, that in the most unhealthy of our settlements in Africa, the East and West Indies, the inhabitants are at such times healthy. But when the wet seasons begin, the scene is reversed; the air immediately becomes vitiated, polluted, and destructive; putrid fevers arise, and spread destruction over the country. The ingenious Mr. Ives gives a dreadful instance of this, and of the diabolical revenge of the Arabs, when they think themselves injured by the Turks at Bassora: by breaking down the banks of the river near that city, they lay all its environs under water. After the water is nearly evaporated, the mud and other impurities corrupting, pollute the air to such a degree, as to cause a most mortal fever in that populous city. This was the case when Mr. Ives was there: of this fever 14,000 souls perished; and of the Europeans settled there only 3 escaped with life: a most horrid mode of revenge, and a dreadful example of the deadly effects of marshes and stagnant waters in hot climates. Let us see if we can prove this by actual experiments.

Exp. 23. To the same powdered mud used in the last experiment, was added as much water as was required to bring it to the same consistency with that in the 21st experiment. This being inclosed with 6 oz. of air as before, stood 24 hours. The air had then contracted a noisome smell, like a new-cleaned ditch, and was diminished from 62° to 49° . Several experiments gave the same results: on standing longer, it was diminished from 62° to 29° . This experiment proves and illustrates the effects of moisture co-operating with unhealthy soils in pro-

ducing their pernicious effects. Moisture to a certain degree is necessary to every kind of fermentation; hence it would seem that by the falling of a certain quantity of rain on marshy grounds, a fermentation immediately commences in the putrid soil, a quantity of vitiated particles are set at liberty, by which the air is polluted. The degree of fermentation is influenced by the degree of heat and the greater or less quantity of moisture.

Exp. 24. To the mud used in the last experiment, more water was added to dilute it so that, on subsiding, a considerable height of water swam above it; it was confined with the air, and stood as in the last experiment. The air being then tried by the test, it was in no instance found further diminished than from 62° to 56° . This experiment was made with a view of discovering the effects of marshes and bogs when laid under water; and we find that their danger is in a great measure obviated by it: so that the putrid fermentation is either prevented by too much moisture, or the effluvia are absorbed in passing through the superincumbent bed of water: perhaps the cold generated by evaporation may have some effect. This fully proves the propriety of Sir John Pringle's remark, where, in giving cautions for avoiding diseases arising from putrid air, he says, "As for cantonments in marshy grounds, if the troops must remain there in the dangerous season, it will be better to float the fields entirely, than to leave them half dry; for the shallower the water the more it will corrupt, and the evaporation will be greater in proportion." How beautifully is this illustrated by the 21st, 22d, 23d, and 24th experiments! An instance of the perfect agreement of faithful observation with truth and nature.

Exp. 25. Two ounces of dirt swept from the streets were inclosed in the phial as before: after standing together 24 hours, the air was found to be diminished considerably, from 62° to 50° . Hence it appears how well the magistrates consult the health of the inhabitants, as well as the neatness of cities and large towns, by enforcing due attention to the cleaning and paving of the streets in their respective districts.

Exp. 26. The same quantity of loamy, vegetable earth, out of Dr. W.'s garden, and brought to the consistence of thick mud by addition of water, was next tried. The air was found but little worse; in one instance only diminished from 59° to 55° ; in another, from 64° to 61° . It is hence probable, that fine loamy vegetable earth contains little putrescent matter, as it gives little noxious effluvia. The addition of animal and other kinds of manures will much vary their effects in this respect.

Exp. 27. A mass of the same consistence was formed of pure clay and water, the other circumstances of the experiment being the same. The air was not found the worse by it in 6 trials: in one there was only the small difference of 62° to 61° , certainly the result of some slight inaccuracy. So that the pure

clay soils appear to be favourable to health; they emit no kind of septic or noxious effluvia.

Exp. 28. Wet sand was tried in the same manner, and found to have no noxious effect on air: from which it may be concluded, that the general notion of the salubrity of sandy soils is founded on truth.

We may now conclude with recapitulating a few inferences, which seem to be proved by the preceding experiments. 1. The atmospheric air is rendered worse by a long continuance of dry weather. 2. It is purified by rains and winds, especially westerly ones. 3. It is considerably worse in cities and large towns, than in the country, even at a small distance. 6. It is quickly poisoned by the effluvia from animal bodies, even while perfectly sweet and free from putridity. 7. Vegetable matters, when not in a growing state, have a similar effect, and in a degree equally powerful. 8. And this is not any ways owing to their aroma or odorous parts. 9. Phlogiston rises alone. 10. Phlogiston is imperceptible to the smell, per se. 11. Phlogiston is, per se, pestilential. 12. The absence of disagreeable smells is by no means a criterion of the healthful state of jails, hospitals, &c. or of their freedom from infection. 13. Mere odour does not injure the air, nor do volatile alkalies. 14. The air is generally pure over waters. 15. The air is greatly injured by the effluvia from the thick mud of bogs and marshes. 16. But this is much obviated by laying them under water. 17. Air is not hurt by such mud when perfectly dry. 18. Air is also infected by the dirt of the streets. 19. Pure loamy vegetable earth has little of such effect. 20. Air is not at all polluted by pure clay soils. 21. Nor by those of pure sand.

XIV. An Account of the Earthquake felt at Manchester and other Places, Sept. 14, 1777. By Mr. Thomas Henry, F.R.S. p. 221.

On the morning of the day on which the earthquake happened, Mr. H. was confined to his bed beyond the usual hour by a head-ache, with which he was generally troubled previous to any storms or considerable changes in the atmosphere. About 5 minutes before 11 o'clock, he was alarmed by a noise which seemed as if it might have proceeded from a large bale of goods thrown down on a boarded floor below stairs: the house shook. He called out to his wife, who was in an adjoining closet, to know what could have fallen; when instantly he was astonished by such a rattling noise at the north-east corner of the house, that he cried out that a part of the house (which had been built but a few years, and was not so firmly connected with the old part as it should have been) was fallen; and in this opinion he was immediately confirmed by a 3d and more violent crash, resembling the tumbling down of a large and lofty wall. Each of these noises was succeeded by a separate concussion. These events must have taken up the space of at least half a minute. During that time he got out of

bed, and ran to a window which commanded a view of one side of the suspected building, and to his great surprize found it standing. He then went to a window at the front of the house, where he also found every thing safe; and on being informed by several people, who had fled affrighted into the streets, that their houses and furniture had been violently shaken, he concluded the disturbance must have been occasioned by an earthquake.

His wife informed him that at the instant of the 2d explosion she had received a very smart stroke on the top of her head, and, imagining that something had fallen off a shelf, looked down on the floor and perceived it heaving under her, but could see nothing that could have given the blow. Lifting up her eyes she saw her china and every thing in the closet dancing on the shelves; and, during the 3d shock, the vibration of the walls was so great that she expected they would have fallen upon her. A pain, attended with a degree of stupor, remained in the part of her head which had been affected for several hours after. Several other persons likewise received strokes similar to electrical strokes in different parts of their bodies.

In the churches, it being in the time of divine service, the greatest confusion and terror were occasioned. The congregations, suspecting that either the galleries or the roofs were falling in, endeavoured to escape with the utmost precipitation. Several people were thrown down and trampled on, and some few had their limbs broken. Nor is it to be wondered at that they were so terrified, as the pillars and walls evidently tottered, and the motion was so great as to toll the bells in the Collegiate and St. Mary's churches. All the neighbouring towns were affected in a similar manner; but very considerable differences were observed in different parts of the same towns. The water in many places was agitated. The passengers in the duke of Bridgewater's boat, who were on the canal, did not perceive any change; but the steersman recollects, that the vessel was suddenly stopped at that time, which he could not then account for. The noise was particularly loud in those houses which were furnished with conductors; and it seems it was loudest in those parts of the houses where the conductors were fixed.

Many people complained, for several days after, of nervous pains and hysteric affections, and of sensations similar to those of persons who have been strongly electrified. Perhaps the fright might have contributed to have produced some of these effects. But Mr. H.'s head-ache, which seldom leaves him before evening, was entirely and immediately removed. Different people in the same rooms were affected in various degrees, and felt the shock more or less violently. Neither the vibration nor noise were perceived by most persons who were travelling on the roads or walking in the streets. Yet others, on looking at the houses, perceived a great undulatory motion in them. Those who stood on

moss or loose garden ground felt it heave under them very perceptibly; and others, who sat or lay on the ground, were so shocked as to be thrown forcibly out of the position they were in. To Mr. H. and several others, who observed the progress of this phenomenon coolly, 3 shocks were very clearly distinguishable. Some persons were sensible of 2, and some of 1 only.

The motion of the earthquake, at least of a rushing wind which attended it, was from south-west to north-east. It was felt at York, Lancaster, Liverpool, Chester, Birmingham, Derby, and Gainsborough; and within this circuit, the diameter of which must be 130 or 140 miles, with greatest violence in this neighbourhood, which appears to have been the centre of it.

The morning on which the earthquake happened was clear and serene. The air was so far from being sultry, that some persons who rode out early in the morning complained of the cold. The wind was easterly. At the instant of the shock it is said to have veered to the west, and to have immediately returned to its former station. On the 20th, 21st, and 22d of September much rain fell, attended with thunder and lightning. The storm was particularly violent on the 21st in the neighbourhood of Rochdale, 12 miles from hence; and early on the morning of the 22d the whole hemisphere appeared, from this place, to be involved in one general blaze.

XV. Sundry Papers relative to an Accident from Lightning at Purfleet, May 15, 1777.

1. *A Letter from Mr. Boddington, Secretary to the Board of Ordnance, to Dr. Horsley, Sec. R. S. with Two Enclosures from Mr. Nickson, Store-keeper at Purfleet, giving an Account of the Accident.*

SIR,

Office of Ordnance, May 31, 1777.

I am directed by the lieutenant-general and the rest of the principal officers of the Ordnance, to transmit to you the copies of the reports and plan received from Purfleet, on occasion of some damage done by lightning; which reports and plan they desire you will please to lay before the R. S. I am, &c.

To Dr. Horsley, Sec. R. S.

JOHN BODDINGTON.

To Sir Charles Frederick, Knt. of the Bath.

HONOURABLE SIR,

Purfleet, May 16, 1777.

Yesterday afternoon we had much rain and distant thunder; but at 6 a very heavy cloud, in passing over the house, presented us with part of its contents, which struck the north-east corner of the house on one of the cramps that held the coping stones together, forced off about a square foot of that stone and one brick, and has displaced about a cube foot of brick-work underneath. It has not been yet discovered that any of the conductors have acted during the passage of that cloud, though the flash and report were both very great. One of my

servants was out of doors by the coach-house at the same time, and narrowly escaped falling by the strength of the flash: the others were in the house, but were much frightened. My son says, that there is a dent in the cramp, on which the lightning fell, and I intend to preserve it as a curiosity. If the conductor on the house has acted, it is imperceptible as I am informed. I thought this account would be acceptable to you from, honourable sir, &c.

EDWARD NICKSON.

To the Right Hon. Lord Amherst, Lieut.-General of his Majesty's Ordnance, &c.

MY LORD,

Office of Ordnance, Purfleet, May 20, 1777.

In obedience to your lordship's commands of the 19th instant, directing me to report concerning the accident that happened to the Board's house by lightning on the 15th, I beg leave to acquaint your lordship, that on that day there had been much rain and distant thunder; but, about 6 o'clock in the afternoon, a very heavy cloud hung over the house for some time, which I looked at from the back-parlour window, and it being quite calm, made me suspect that some of our conductors might find employment from it. I had not been long at the window before a violent flash of lightning and clap of thunder came together; and, as soon as the rain would permit any body to move about, one of the labourers brought me some pieces of stone and a brick, which were struck off from the coping on the parapet-wall of the building from the north-east corner. On my son's coming home, I desired him to go and view the top of the house. On his return he told me, that the lightning had struck one of the cramps that hold the coping together, and had made a dent in the lead of the cramp, and the stone adjoining to it, as if struck by a musket-ball; that the quantity of stone thrown down might amount to about a square foot; and that it had disturbed about a cube foot of brick-work underneath: and, according to your lordship's commands, the distance from the point of the conductor on the house to the part struck has been measured by him this day, and amounts to 46 feet. All the conductors at this place are pointed, and it has not yet been discovered that any of them have acted on this occasion. If your lordship should want any farther explanation, my son waits on you with a small drawing of the elevation of the east front of the house, and of the part struck, which I hope will be satisfactory to your lordship and to the honourable board. I am, &c.

EDWARD NICKSON.

2. *The Report of the Committee appointed by the Royal Society, for Examining the Effect of Lightning, May 15, 1777, on the Parapet-wall of the House of the Board of Ordnance, at Purfleet, in Essex.*—The iron cramp on which the lightning fell was cemented into the coping stones on the parapet-wall, near the north-east corner, with lead: and on that lead, at one end of the cramp, there appeared to have been a small fusion; the end of the lead, and part of the

adjoining stone, being indented about half an inch in diameter, and a quarter of an inch deep, as though a musket-ball had been fired against them. The iron cramp was situated over a plate of lead, and the ends of it, which were inserted in the stone, came within 7 inches of the plate, which communicated with the gutter, and served as a fillet to it: this gutter was a part of the main conductor of the building. When the lightning had quitted the iron cramp, and had passed through 7 inches of stone, brick, and mortar, it fell on the corner of the plate of lead abovementioned, as appeared by the fusion of a very small portion of it discovered by pulling out the bricks, mortar, &c. on purpose to examine into this particular. From this place no further effect of the lightning could be traced; the metallic conductors to the earth having effectually performed their office. At the distance of $7\frac{1}{2}$ feet from the place stricken, a large leaden pipe went down from the gutter to a cistern of water in the yard. The rain, Mr. Nickson informed us, had fallen plentifully for some time before the stroke; so that the mortar, bricks, &c. did probably form an imperfect conductor, for the distance of 7 inches, between the iron cramp in the coping stones, and the filleting of lead abovementioned.

At the termination of the iron cramp in the coping stone, a piece of the stone, with one brick, was stricken off; and a few other bricks were loosened, and removed less than half an inch from their places. The damage done to the parapet of the building is so inconsiderable, that it would scarcely deserve notice, was it not an evident proof that the metallic communication with the earth, hath, in this case, effectually prevented any further injury.

The conducting rod on the ridge, near the centre of the house, showed no marks of its having been affected by the lightning in this case: and it is remarkable, that the surface of one of the hip-rafters, $4\frac{1}{2}$ inches in diameter, covered with lead (communicating with the gutter) and reaching within 28 inches of the place stricken, seems not to have been at all affected.

The method we would recommend of preventing similar accidents to the parapet of this building for the future, is the following: let a channel of the same size with the cramps be made from cramp to cramp in the coping stones, quite round the building: let this channel be filled with lead, and let a metallic communication by plates about 6 inches broad be made from that lead in 4 places (one at each side or corner of the parapet) to the filleting of lead which is in contact with the gutter, which gutter is part of the main conductor to the building.

W. Henley, T. Lane, E. Nairne, J. Planta.

June 19, 1777.

3. *Mr. Wilson's Dissent from the above Report.*—When this important subject was first debated in the Purfleet committee of 1772, a passage was quoted from Dr. Franklin's philosophical publications, respecting the nature of such

buildings as were secure from attacks by lightning. The passage alluded to is this: "buildings that have their roofs covered with lead or other metal, and spouts of metal continued from the roof into the ground to carry off the water, are never hurt by lightning; as, whenever it falls on such a building, it passes in the metals, and not in the walls." Franklin's Exp. p. 481. With this idea the building at Purfleet, called the Board-house, was considered by that committee to be in a similar situation, and consequently secure from such attacks, without having any other conductors than the leaden gutters, pipes, &c. As the members of that committee then present seemed to be fully satisfied with that determination, I proposed that the magazines themselves should be put into the same circumstances; otherwise there would appear to be an inconsistency in the different methods of securing those buildings. My argument had no other effect than to occasion, at the next meeting of the committee, a resolution for fixing pointed conductors to all the buildings. From this resolution I dissented, and gave in writing my reasons at large for differing in opinion, which are printed in your Transactions.

What has been the consequence since the conductors were put up? Behold! this very Board-house, which was never attacked before by lightning, hath very lately been struck, and that within a few inches of the conductor; contrary to Dr. Franklin's assertion, which positively says, that in such circumstances the lightning passes in the metals, and not in the walls. We may refine in our reasoning on the philosophy of this event as much as we please; but let me tell you, gentlemen, there is no getting rid of the fact: which, according to my judgment, appears to be truly alarming. And, as I apprehend the reputation of this learned society is greatly concerned therein, we ought immediately to avail ourselves of this providential warning, and reject an apparatus which threatens us every hour with some unhappy consequences. It is with very great concern, that I am obliged to take notice, in this society, of a house, which is of the first consequence in this kingdom, that hath pointed conductors also fixed on it: I mean the King's, our most gracious patron and benefactor's. Who were the advisers of them I know not; but as they are there, I thought it my duty to mention them.

In considering the propriety of pointed conductors, I think it necessary to observe, that increasing the number of them in any given space does not by any means, in my opinion, lessen the risk of accidents by lightning; but on the contrary (at least in many cases) a greater number of such conductors will necessarily invite a larger quantity of lightning. At Purfleet there are several of those conductors; and by the storekeeper's letter sent to the Board of Ordnance, which was lately read before us, it appears, that he himself observed a very heavy cloud hanging over the house for some time before the stroke happened.

According to Dr. Franklin's idea, this event ought never to have happened; because he says, that pointed conductors will draw all the lightning out of the clouds, and carry it away into the earth silently. This philosophy I never had any faith in, unless the quantity of lightning contained in the clouds happens to be very little, and incapable of producing any fatal consequences. I have now only to add, that I did not propose to have troubled this society any more, had I not thought, on this great occasion, it was my duty to stand forth, and give my opinion against the present report; as I know of no possible advantage to be derived from such conductors; at least none that are consistent with true philosophy, and a sincere regard to the welfare of society.

June 19, 1777,

B. Wilson.

4. *A Letter from the Board of Ordnance to Sir John Pringle, Bart., P. R. S., inclosing an Account of Mr. Wilson's Experiments on the Nature and Use of Conductors, addressed to his Majesty.*

SIR,

Office of Ordnance, Nov. 18, 1777.

Mr. Wilson having laid before this board a copy of the report made by him to his Majesty on some experiments in consequence of the accident by lightning, in May last, to one of the buildings belonging to the royal magazine of gunpowder at Purfleet: we beg leave to transmit to you a copy thereof, to be laid before the R. S.; and, at the same time, we desire the favour of their instructions, if any thing more can be done, in order to the preservation of his majesty's magazines.

We are, &c.

Amherst, Charles Frederick, Charles Cocks.

To the King.

SIR,

Your Majesty, in consequence of the accident from lightning that happened to one of the buildings at Purfleet in May last, having been graciously pleased to intimate the propriety of making some further experiments, to ascertain the best method of preventing such accidents for the future; and having also condescended to be present at the exhibition of those experiments at the Pantheon; I have presumed to address to your Majesty this faithful and circumstantial account of what was there attempted, together with some observations thereupon, as an humble testimony of my duty and gratitude for the great honour conferred on me. How far I may have succeeded in these my zealous endeavours to ascertain the most proper construction for conductors is, with the greatest deference, submitted to your majesty and the public. And whatever consequences may be derived from these experiments, I am happy in the thought of having done every thing in my power, with the utmost candour and impartiality, to investigate truth, in a question of real advantage to science, and of such importance to the public, as seems, in my humble opinion, worthy the

attention of the ablest philosophers. I am, sir, your majesty's most faithful and most dutiful subject,

Nov. 12, 1777.

Benjamin Wilson.

New Experiments and Observations on the Nature and Use of Conductors, by Benjamin Wilson, F. R. S., of the Imperial Academy of Sciences at Petersburg, of the Royal Society at Upsal, and of the Academy of Institutes at Bologna.

The experiments I propose to give an account of in this paper, were made in consequence of the accident from lightning, which happened to one of the buildings belonging to his majesty's magazine of gunpowder at Purfleet, on the 15th of May last. Soon after that event, an official and particular account having been sent by the Board of Ordnance to the R. S., a committee of the members was immediately appointed, to examine the damage done to that building, and afterwards to make a report of the same. When that report was laid before the society, I thought it my duty, in particular, to stand forth, and offer some objections to the using pointed conductors at Purfleet, or indeed any where else. This public proceeding was, I apprehended, the more necessary, as I had, on a former occasion, in the year 1772, declared my dissent from the report then made by the committee, who had recommended sharp-pointed conductors for that magazine, to be fixed 10 feet higher than the respective buildings. But though I had read the paper alluded to above, I did not apprehend that my duty was fully discharged, without trying other methods of having so serious and interesting a subject further inquired into. I had the satisfaction, soon after, to meet with sufficient encouragement to induce me to consider of some experiments, which might make the subject in dispute more intelligible.

The plan I conceived to be the most proper for this purpose, was to have a scene represented by art, as nearly similar as might be, to that which was so lately exhibited at Purfleet by nature. To carry a design of that kind into execution, it was necessary that attention should be given to the several circumstances concerned in the event at Purfleet. The most material of those circumstances I apprehend to consist in having a substitute for a thunder-cloud, as it is vulgarly called, and large enough, or sufficiently long, to admit of being charged with a considerable quantity of the matter of lightning by artificial means; and also, that this substitute should admit of being easily moved, and with any velocity the experiment required: or, at least, so as to equal the motion of a thunder-cloud. An apparatus sufficiently large for these purposes could not conveniently be put in motion: therefore I proposed to get rid of this difficulty, by moving the building itself, instead of the substitute; as that would answer the same end. In order to obtain a considerable charge of artificial lightning, I proposed to have one great cylinder covered with tin-foil, and a

wire joined to one end of it, that should, when extended properly, consist of several hundred yards. This idea leading to an expence too considerable for an individual, I presumed to hope for other assistance.

Upon an humble representation of these matters, his majesty, who is always disposed to promote every pursuit which tends to the advancement of science and the good of the public, most graciously condescended to encourage the undertaking; and, by the favour of the right honourable and honourable Board of Ordnance, I was immediately enabled to carry the intended plan into execution. Very soon after this encouragement, I procured correct drawings of the building called the Board-house at Purfleet: from these an exact model was made, excepting the windows and doors, which were omitted, because they were immaterial on this particular occasion. In this model, a strict attention was paid to those parts of the building where metal had been introduced; such as the hips and gutters of the roof and the several spouts to carry off the water. And as the north-east corner of the house was the part that suffered by lightning in May last, particular attention was paid to the two cramps at that corner, and the two spouts on the north-side. These cramps, in the model, were made of small wire, that bore nearly an exact proportion to those in the building itself; not only in regard to length and thickness, but also their distance from each other, and from the turning up of the lead appertaining to the gutter. The two spouts were represented each by a thick wire, the shorter of which communicated (at the bottom) with a cistern. This cistern resting on 2 wooden pillars, or posts, about one foot and a-half in length at Purfleet, the same circumstances and proportions were attended to, and made to correspond exactly in the model. The other wire, in conformity to, and nearly in proportion with, the other spout at the Board-house, descending from the gutter for about 7 inches, was there bent almost at right angles, and then continued on for $12\frac{3}{4}$ inches, in a line nearly horizontal, till it reached within 2 inches, or little more, of the short wire: after which it was bent again almost at a right angle, and then lengthened out to the bottom of the model, whence it communicated, by another wire, with a pump or well, in another part of the house. This kind of communication was necessary, because, consisting of metal, in that respect it was similar to the communication at Purfleet.

Besides the 2 cramps mentioned above, another parapet was made to put on occasionally, which contained all the cramps: these were properly fixed therein, and at their proper distances from each other. And the R. S. having thought proper, since the accident, to order that a metallic communication should be made between the cramps on the parapet, quite round the building, as a better security from such accidents for the future, care was taken to make a similar connection with the wire cramps, by means of small slips of tin-foil that were

pasted on the parapet of this model. On the top of the roof in the middle, conductors of different lengths and terminations were occasionally put, just as the experiments required. The scale from which this model was made, when compared with the house, was $\frac{1}{3}$ of an inch to a foot. In regard to the wood of which the model was made, I took care that it was well baked, and soaked, while hot, in drying oil, before the several parts were joined together, that it might be the more similar to the bricks and other materials of the building itself, in the power of resisting the passage of the fluid, whenever any attack of it should be made. For brick, stone, dry lime, &c. had been observed many years ago by Mr. Delaval and others to resist the passage of this fluid very considerably.

In order to move this model with the velocity required, it was necessary to have a frame of wood, of such a length as would suffer the model, with the pointed conductor on it, to be out of the reach or influence of the charge contained in the cylinder, both at its setting off, and when it had arrived at the end of its journey. To this frame, 2 upright posts of wood, $10\frac{1}{2}$ feet long, were fixed at the further end, and at a distance from each other equal to the width of the frame. On the top of these posts, and in the middle between them, were fixed 2 wheels of different diameters on the same axis. The larger took the line that was proposed to draw the model, and the lesser another line suspending 2 weights which regulated its motion. For after the heavier weight had descended so far as to bring the model directly under the substitute, it was then checked; but the less weight continuing to descend, the model moved forward with its acquired velocity, joined to the power of the less weight. And that the remaining motion might at last be overcome, without striking against the 2 posts, some narrow slips of cloth, 7 feet long, were nailed on the frame, in those parts over which the model was to pass before it reached the end. This model moved like a sledge, by means of 2 slips of wood fixed at the bottom, which ran in 2 grooves that were cut along the frame from end to end. And the line which drew the model along, was fixed very near the centre of resistance.

To construct the substitute for a cloud, I first joined together, in 15 lengths, the broad rims of 120 drums, merely to have them portable, by means of wood cut into long slips, which were fixed on their insides: but, as those drums were not accurately of a size, the several joinings were covered over with cloth, and pasted down, to make the surface throughout more even. After this, the whole number were properly covered with tin-foil, excepting 8; for these being brass, required to be covered only at their joinings. All these drums together formed a cylinder above 155 feet in length, and above 16 inches in diameter. The whole cylinder was made in 4 separate parts: 3 of those parts could easily be made to communicate, or not, with each other: the 4th, being brass, was

reserved for a different purpose. The several ends of those 4 parts were closed up with board, rounded off at the edges in every part, and covered with tin-foil likewise.

This great cylinder, consisting of the 3 parts, was suspended about 5 or 6 feet from the floor by silk lines; and formed a curve in the room, something like a horse-shoe; one end of which hung over the middle of the long frame, on which the model was proposed to move; the other, which I call the further end, was joined occasionally to the end of a long wire, that was suspended through the whole space of the room. And lest the several atmospheres round this wire, in its charged state, might, in consequence of the unavoidable returns of the wire, interfere too much with each other, it was suspended in such a manner, by silk lines also, that each length was 5 or 6 feet from its neighbour: and those that were suspended nearest to the great cylinder hung at the same distance from it. The remote end of this long wire hooked on occasionally at the end of the brass drums, which made a separate cylinder (the 4th part alluded to above) about 10 feet in length: this was suspended likewise by silk lines, and about 6 feet from the floor; but in such a manner, that the furthest end of it from the wire was within 9 or 10 feet of the great cylinder. The long wire, with the great cylinder and brass drums, made the whole of the substitute for a thunder-cloud, when they were properly charged.

The machine, employed to charge this apparatus, consisted at first of 2 large glass cylinders turned by one wheel. But as the friction arising from the 2 together rendered it difficult to work them, and the advantage gained from both in the charge itself was found to be not so considerable as might reasonably be expected, one of them only was made use of in the following experiments. The place where this machine charged the great cylinder, was about 10 or 11 feet from its nearest end. It was found expedient to be provided also with another machine; but this was employed only on particular occasions, and was generally placed at the further end of the great cylinder.

The floor of the room being of baked wood, it was necessary to have wires properly connected with the cushions of both machines along the floor, where they were joined to another wire, which communicated with the well, in order to conduct the fluid more readily than the baked wood admitted of. The whole of this apparatus, so contrived, was disposed in the great room of the Pantheon, by the favour of the proprietors, who, having heard that a large apartment was wanted, in which to show before the Board of Ordnance and the R. S. these experiments, were pleased to honour me with a very polite letter, offering the use of that elegant building for the purposes intended.

Exp. 1. The model, with a pointed conductor on it (which, in a degree of sharpness, was nearly equal to that of a common darning needle) being placed

directly under the nearer end of the great cylinder, so that the distance between the point of the conductor and cylinder was little more than 4 inches, the machine was then put into motion. After about 2 turns of the wheel, a small stream of light appeared at a little interval, between the top of the longest thick wire which represented the bent spout, and its little cistern next to the gutter, where the metallic communication was purposely interrupted. This stream continued to be visible, though the model was moved along the frame from its fixed station to more than the distance of 43 inches.

Exp. 2. When a conductor of the same length with the former, but rounded at the end, and no more than $\frac{3}{10}$ of an inch in diameter, was put in the place of the pointed one, every other circumstance continuing the same, the small stream of light appeared again; but on moving the model a little beyond the distance of 16 inches, it totally disappeared.

First Observation.—By the 1st experiment it was manifest, that the point acted on the charge all the time the model was moving through a space equal to 43 inches; and consequently was, all that time, diminishing the charge in the great cylinder. On the other hand, the 2d experiment showed, that the rounded end acted on the charge only while the model moved through a space equal to 16 inches. And from the 2 experiments compared, it appears, that a charged body is exhausted of more of the fluid by a pointed, than by a blunted conductor:

Exp. 3. If in the place of the rounded conductor a similar one was put, but about $\frac{1}{5}$ of the length, while the model stood directly under the great cylinder as before, the charge contained in it produced no appearance of light whatever.

Exp. 4. But when a pointed conductor of the same length with the last was put in its place, the small stream of light appeared, and continued visible all the time the model was moved through a space equal to $18\frac{1}{2}$ inches.

2d Observation.—These last experiments, compared with the former 2, show that a rounded conductor, little more than $1\frac{1}{2}$ feet above the highest part of a building, receives a far less quantity of the matter of lightning from a cloud fully charged, than a pointed conductor placed 10 feet above a building, circumstanced alike in every other respect. Nay, a pointed conductor of the same length with the short one that was rounded, appears, from these experiments, to collect a greater quantity of the fluid, than even the long conductor with a rounded end.

Exp. 5. On repeating the 1st experiment with 10 turns of the wheel only, the charge remaining in the great cylinder was immediately received on the hand; the sensation it occasioned was little more than perceptible.—*Exp. 6.* But on repeating the 2d experiment with 10 turns of the wheel also, the charge remaining was taken; but the sensation in this case was increased considerably.—

Exp. 7. When the 3d experiment was repeated, and with the same number of turns, the sensation was observed to be full as violent as if no such metallic interposition had been presented to it.—*Exp. 8.* On repeating the 4th experiment, and with 10 turns of the wheel also, the sensation was not near so considerable; for it seemed something less than what was experienced with the long rounded conductor in the 6th experiment.

3d Observation.—The several effects observed in the last 4 experiments agreeing so exactly with those in the first 4, prove, at least so far, that rounded ends, in these cases, received the fluid less readily, and in less quantity, than points.

Exp. 9. In the 1st experiment (when the long pointed conductor was put upon the model, and while the wheel was turning) a very small spark might be taken from the short spout; but if the hand, or a wire, was applied, which communicated with the well, and continued in contact with the short spout, or if it was connected with the long spout, the small stream of light seen before at the top of that spout now ceased.—*Exp. 10.* But this was not the case when the 3d experiment was repeated (that is, with the short rounded conductor); for this, by reason of its greater distance from the cylinder, and the nature of its termination, did not draw a sufficient quantity of the charge from the great cylinder to cause the least appearance of a stream of light, as in the former case.—*Exp. 11.* The effect however was different when a pointed conductor, of the same length as in the 4th experiment, was made use of; for then the short spout was charged nearly in the same manner as in the 1st experiment, and the stream of light at the top of the bent spout disappeared the instant a communication was made from the short spout to the earth.

4th Observation.—The 2 posts of wood on the ground, which supported the cistern at the bottom of the short spout, were therefore the true cause of these effects taking place in the short spout, by preventing a communication with the earth, and hindering the fluid, that was constantly charging the short spout, from discharging itself properly into the earth.

Exp. 12. The model (furnished with the wire of communication, and with the longest pointed conductor on it) being properly placed on the long frame, and held there in readiness to be drawn forward by the line and weight at the other end; the great cylinder was charged by 20 turns of the wheel. On letting go the model, and almost at the instant before the point came under the centre of the cylinder, it was suddenly struck with the matter of lightning, and frequently sooner. The least distance of the point from the great cylinder, when this stroke happened, measured nearly 5 inches. The quantity of charge that remained in the cylinder was very little to the sense of feeling, though taken immediately after the stroke happened.—*Exp. 13.* On putting into the place of the pointed conductor one of the same length, that was rounded at the end, and

without any other change of circumstances, the wheel having been turned the same number of times, I suffered the model to pass: the rounded end, in this case, was not struck. However, the instant after it had passed, the quantity of the charge that remained in the cylinder was taken in the same manner by the hand; on which the sensation was more violent than in the last experiment.

5th Observation.—From the last 2 experiments it appears, that though every circumstance was the same, excepting the different terminations of the 2 conductors, yet the pointed one only was struck; though they were both of the same length, and passed at equal distances from the cylinder. Hence we collect, that the quantity of lightning discharged from the great cylinder into the point, when an explosion happened, was considerably greater than the quantity discharged into the rounded end, when there was no explosion.

At the first rise of a difference in opinion, respecting the proper termination and length for conductors, I was prevailed on by some learned members of the R. S., in the year 1764, to publish my sentiments on that subject. Accordingly, in a letter addressed to the marquis of Rockingham* (after stating several reasons against the use of points, as I suppose they invited the lightning) I there recommended that conductors should not only be rounded at their ends, but be made considerably shorter than those which Dr. Franklin contended for, and indeed should not exceed the highest part of the building. In the following experiment, however, I did not place the pointed conductor below, nor on a level with the highest part of the building, but above it, even $\frac{1}{3}$ of the length of that in the 12th and 13th experiment.

Exp. 14. The model being thus furnished, and every thing else put exactly into the same circumstances as in the 13th experiment, the great cylinder was charged by 20 turns of the wheel. On letting go the model, it passed the cylinder at the distance of 7 inches, without being struck: but the charge that remained in the cylinder at the instant after the model had passed it, was so considerable, that there appeared no material difference whether the model thus circumstanced was suffered to pass or not.

6th Observation.—This last experiment shows, that a thunder-cloud may pass a conductor so circumstanced without the latter being struck, or suffering the least injury; which it will not do in other circumstances, that is, when the conductor is pointed, and raised 10 feet above the building.

Exp. 15. On repeating the 14th experiment, but with a rounded conductor, which was $\frac{3}{4}$ of the whole length of that in the 12th experiment (all other circumstances remaining the same) and after charging the cylinder by an equal number of turns, it passed also without being struck. In this case, the remain-

* Phil. Trans. vol. 54.—Orig.

ing charge in the cylinder was something less than in the last experiment.

7th Observation.—This is a further instance of the advantage derived from having rounded terminations of a given length on a building, compared with pointed ones, that are only 2 or 3 feet longer.

Exp. 16. On charging the great cylinder as before, that is, by 20 turns of the wheel, and when the model, without any conductor on it, was let go, there was no explosion in or on any part of it, during the time of its passing by the cylinder; though the model itself was properly connected with metal from the top of the roof to the bottom of it, and afterwards to the well.—*Exp. 17.* But when the experiment, with the long pointed conductor on the model, was repeated, and after the wheel had been turned an equal number of times, the point was struck as it passed the cylinder; and, at the same instant also, a very small stream-like explosion appeared between the two cramps at the corner of the model, darting as it seemed from one to the other, in a direction that was rather particular. This stream-like appearance, I apprehended, was nothing more than the effect of a small explosion in consequence of the motion of the model.

8th Observation.—The 15th experiment showed, that the corner of the model, where the 2 cramps were inserted, passed safely by the charged cylinder, without affording even the least luminous effect; and consequently proved, that in such circumstances the 2 cramps could not possibly be struck, because the charge in the cylinder remained the same, or very nearly so, after the model had passed.

9th Observation.—But in the 17th experiment, the pointed conductor was fixed in its place on the model, just like that at Purfleet; when not only the point was struck, as the model passed the cylinder; but, at the same instant, a small explosion was seen between the 2 cramps at the corner. That this light between the cramps arose from a different cause than what had been suggested by the 2d Purfleet committee, appeared from some circumstances accompanying that effect. For example, these cramps had no connection with the gutter or spouts next them, but were quite separate, and at the distance of 6 or 7 inches from any metallic communication. And it is well-known to philosophers, that lightning always passes where it meets with the least resistance; they also know that the least resistance in the present instance must have been along the conductor at the top to the hips, gutters, and spout, and so on to the wire at the bottom to the well. According to this law, the cramps themselves, then, were not properly circumstanced to receive the fluid as it passed to the earth, on account of the metallic communication, described above, being interrupted more than 6 inches.

Exp. 18. To each end of a slender substitute made of wood, about 11 feet in

length, and something less than 1 inch in diameter, was fixed a ball of the same matter. The larger of these measured 3 inches in diameter, and the lesser $1\frac{9}{10}$ inch. The larger ball was then brought usually within $1\frac{1}{4}$ inch of another ball, $1\frac{9}{10}$ inch in diameter. Being so prepared, the model was set on a table, directly under the ball at the remote end of the less substitute, with the pointed conductor on it: and all the wires were properly connected, so as to make a free communication between the model and the well. Nothing now remained but to put the machine in motion; when, after 10 turns of the wheel, the point on the model was struck at the distance of 4 inches. In the 12th experiment, where the model was in motion, the point was struck at the distance of 5 inches nearly from the cylinder. This difference of distance in these 2 experiments seemed to arise chiefly from a difference in the states of the air, it being rather unfavourable when the 18th experiment was tried.

Exp. 19. However, some time after, when the state of the air was very favourable, I repeated the last experiment, and observed that the point was struck at $6\frac{1}{4}$ inches.

Exp. 20. I now repeated the 18th experiment; and attending only to the 2 cramps at the corner, there appeared, at the instant when the point was struck, a small spark or explosion between them: which clearly showed, that the stream-like explosion, observed in the 17th experiment, was only this small spark accompanied with the circumstance of the motion of the model.

10th Observation.—From what we have now experienced it appears, that thunder-clouds, even at rest, and that strike each other at a given distance with the matter of lightning, occasion the same phenomena nearly which a single cloud produces when motion is introduced.

Exp. 21. When the distance between the two substitutes was made less in any degree than the greatest striking distance, it made a considerable difference in the effects; because the fluid in these cases passed more freely from the greater to the less substitute: and the freer it passed into the latter, the nearer they approached to be one substitute. So that bringing the 2 substitutes into contact, occasioned the same phenomena that the great cylinder did alone: that is, the rounded end would cause an explosion at a considerable distance; and the point little or none, though it was brought almost close to the substitute.—*Exp. 22.* But if motion in this case was introduced, during the contact of the 2 substitutes, the point was struck at $9\frac{1}{2}$ inches distance from the ball. The motion employed on this occasion was by the hand only, which held a proper stand with the point upon it; and this point communicated by a wire with the well.

11th Observation.—Now the nearer the 2 substitutes were brought together, the nearer they represented one cloud; and consequently the matter of lightning would pass from one to the other in these cases more readily, and without

permitting so great an accumulation to take place as was experienced in the 19th experiment, when the separated substitutes struck at the greatest distance.

Having now gone through the experiments where points were introduced, we shall next relate the several experiments where other terminations were used. By this method of proceeding, we shall be able to form a proper judgment of what kind of conductors are the most advantageous for securing buildings, &c.

Exp. 23. On repeating the 18th experiment, but with a rounded conductor on the model, every other circumstance continuing the same, and the model at rest, the greatest distance at which it was struck, by 10 or 11 turns, was not more than $\frac{3}{4}$ of an inch. Now if we compare this distance with that at which the point was struck in the 18th experiment, the proportion will be found to be less than 1 to 5.—*Exp. 24.* When the 23d experiment was repeated, and the rounded end struck at $\frac{3}{4}$ of an inch, the same kind of spark appeared between the 2 cramps as in the 20th experiment when the point was used; but in this experiment with the rounded end the spark at the cramps appeared considerably less to every observer.—*Exp. 25.* On repeating the 21st experiment where the 2 substitutes were brought into contact, every other circumstance remaining the same, the rounded end was struck at the same distance nearly as when a spark was taken by a larger metal ball, suppose 3 inches in diameter, from any part of the great cylinder when equally charged: for in this case the 2 substitutes, being in contact, made in reality but 1 great substitute.—*Exp. 26.* I now repeated the 22d experiment, where motion was introduced; and without any other change of circumstances than putting in the place of the point the rounded end. On this occasion, as well as on the former, the same person moved the stand with the rounded end on it, and with the same velocity, but not before the connected substitutes were fully charged by an equal number of turns. The instant that the rounded end approached within a certain distance of the ball at the end of the less substitute, it struck; but the explosion seemed inferior to that which the point occasioned at the distance of $9\frac{1}{2}$ inches. In this experiment the distance between the rounded end and ball was not more than $6\frac{1}{2}$ inches. From which it appears, that even in those circumstances the point was struck at a greater distance than the rounded end in the proportion of $9\frac{1}{2}$ to $6\frac{1}{2}$.

Exp. 27. It has been observed in a former part of this paper, that a great quantity of rain fell when the accident happened at Purfleet; as this circumstance seemed to be material, it was proper to put the model into a similar situation; and therefore, after removing the tin foil on the parapet, I washed the model all over with a sponge; and, while it continued in this state, the machine was put into motion: after 10 turns of the wheel, the point was struck at 5 inches distance. In consequence of this a small explosion appeared, not only between the cramps; but also another was seen, still more vivid, at the inner corner of the

parapet, nearest to the cramps, darting, as it seemed, from the gutter up to the cramps.—*Exp.* 28. On pasting tin foil on the top of the parapet quite round the model, as had been done before, and moistening the inner part of the parapet down to the gutter, the stroke was again received by the point, when the same appearances were observed as in the last experiment.—*Exp.* 29. I then made a metallic communication between the top of the parapet down to the gutter: after this the experiment was repeated. And though the point was struck at 5 inches distance, there was no spark, either between the cramps or at the inner corner next to the gutter.

12th Observation.—From these last experiments it appeared, that rain contributed to make the lateral effect greater at the corner, by forming a better communication between the cramps and the gutter, than the dry materials of brick and stone admitted of. But it also appeared that, when the communication between the gutter and cramps was rendered more perfect by a slip of tin foil that was interposed between them, that lateral effect ceased at the cramps; because a still freer passage was made for the fluid to discharge itself through; not only along the slip of metal communicating with the gutters and the tin foil quite round the parapet, but also along the tin foil down the side of the model, from which it was conveyed by the wire to the well.—*Exp.* 30. An objection having been made, that the wire communicating from the bottom of the model to the well, as it consisted of several distinct parts, occasioned a resistance at each of the junctions, and therefore constituted only an imperfect conductor; in order to try the validity of this objection, a new communication was now made from the model to the well, consisting of one entire wire, pointed at the end, with which I repeated the experiment of passing the model; and finding no sensible difference from the former results, I then, in order to apply a rounded end to this perfect communication, had the pointed conductor, which was previously made use of, soldered on to the end of the wire instead of the other point; and to this conductor so soldered I occasionally applied the small ball which was used in the former experiments, and which has hitherto been called the rounded end. With these different terminations, and thus circumstanced, all the experiments were repeated; and it was found, that the results were rather more in favour of the doctrine hitherto advanced, than before the communication was made so perfect. It having been farther objected, that the motion of the model employed in these experiments was considerably greater than the motion of a thunder-cloud, I made the following experiment.—*Exp.* 31. The weight which moved the model in the preceding experiment was gradually reduced till it was nearly balanced by the friction; and when the motion was rendered so slow as 7 feet 7 inches in 7 seconds, it was very little accelerated; and in this state the

great cylinder being charged, the model was suffered to pass: and though the velocity was less than $\frac{1}{4}$ of a mile in an hour, the point was struck.

Exp. 32. When the great ball of the less substitute was placed at the greatest striking distance from the ball at the end of the great cylinder, I fixed a needle into the under side of the remote end of the less substitute, with the point downwards: opposite to this point, and on the same stand described in the 22d experiment, was fixed another needle; so that the 2 points were opposed to each other. The space between them was varied from time to time, in order to find the greatest distance at which the lower one could be struck. On charging the great cylinder, it appeared that the greatest distance in this case was $5\frac{1}{4}$ inches.

—*Exp. 33.* Upon repeating this experiment, whilst every circumstance remained the same, excepting that, instead of the point below, a rounded end was put in its place, and after charging the cylinder again, it appeared, that the greatest distance, at which the lightning struck the rounded end, was not more than $2\frac{3}{4}$ inches. And the largeness of the spark and the loudness of the explosion appeared to be less considerable than in the 32d experiment.

During the course of this inquiry, having occasion to try some experiments in the dark, I observed a curious circumstance, which seemed to show, that a point had a far greater influence on the charged substitute, in certain circumstances, than a rounded end had when it was placed in the same situation.

Exp. 34. The circumstance alluded to was an appearance of light on the brass ball that was fixed at the end of the great cylinder, when the copper ball of the less substitute was opposed to it at the greatest striking distance, as in the 18th experiment, every other circumstance remaining the same; and while the model, with its pointed conductor, stood on the table directly under the tin ball, fixed at the remote end of the less substitute: for soon after 7 or 8 turns of the wheel, a light began to appear on the brass ball, and continued to increase in brightness till the moment it burst forth in an explosion towards the copper ball. The part of the brass on which the light appeared, was that next to the copper ball: and the general appearance of it was round, and sometimes more than half an inch in diameter.—*Exp. 35.* On repeating this experiment with a rounded end, instead of a point, and at the same distance from the tin ball, though every other circumstance continued the same, there was no such appearance.—*Exp. 36.* But when the rounded end was moved considerably nearer, that is, within $\frac{6}{10}$ of an inch, a light was visible; but then it was faint, and not more than $\frac{1}{10}$ of an inch in diameter, even at the instant before the explosion happened.

13th Observation.—By the 1st of these experiments it appears, that the influence which the point had on the whole of the fluid contained in the great

cylinder, was such as to cause a general tendency of it towards the less substitute; but, on account of the resistance which seemed to operate at the surface of the brass ball, it was there stopped, and by degrees accumulated, till such time as the accumulation was great enough to overcome that resistance. Now, according to this manner of reasoning, the point did not draw the fluid out of the great cylinder silently; but when the accumulation had got to a sufficient degree, a sudden explosion ensued, more or less violent, according to the circumstances which accompanied the experiment.

14th Observation.—From the other experiment it appears, that the rounded end had not so great an influence as the point on the charge in the cylinder; because we were obliged to bring it 5 times nearer before any light could be perceived at all; and even then it was so faint and inconsiderable in its diameter, compared with the other light produced by the influence of the point, that it manifestly confirmed the truth of the last observation. I shall now proceed to make some general deductions from what has been already related.

It seems to be clear then, that in all experiments made with pointed and rounded conductors, the rounded ones are by far the safer of the two, whether the lightning proceed from one or more clouds; that those are still more safe, which (instead of being, as Dr. Franklin recommends, 10 feet high) are very little, if at all, above the highest part of the building itself; and that this safety arises from the greater resistance exerted at the larger surface. The luminous appearance at the end of the brass ball, occasioned by the point in the 34th experiment, manifestly showed that there was an accumulation of the fluid within that part of the ball, in consequence of some resistance: for when the resistance at the surface of the brass ball was at last overcome by the influence the point had on the charge, the explosion took place immediately; and that, not only between the two substitutes, but also between the end of the less substitute and the point. A cloud, therefore, that happens to be charged, and within the striking distance of another cloud which is not charged, and also equally within the influence of a pointed conductor, must necessarily produce similar effects with those mentioned in the 34th experiment. On the other hand, clouds that are circumstanced like those above, and not within the influence of a rounded conductor, will pass quietly over such a termination, and without any explosion.

On acceleration, and its effects.—From considering the extraordinary effects which have sometimes been produced on gross matter by lightning, and the distance there frequently is between thunder-clouds and the earth, when such effects take place, I suspected that those effects might in some degree be owing to an increase of the velocity of the fluid which produced them. To try whether this was really so, it seemed necessary to have an apparatus of a far greater

length than the great cylinder: I therefore made use occasionally of the long wire which has been already described.

Exp. 37. On connecting one end of this long wire with the further end of the great cylinder, and the other with one end of the brass drums; I found, that about 6 uniform turns of the wheels, with a moderate velocity, were required to cause the appearance of a small stream of light at the top of the spout described in the first experiment, when the model, with the pointed conductor on it, stood directly under the great cylinder, but at the distance of 5 inches.

Exp. 38. When the great cylinder was unconnected with the long wire and brass drums, and while the model, with the same conductor on it, remained in its place; about 2 turns, with the same velocity, were sufficient to charge the great cylinder, so as to cause a similar appearance at the spout.—*Exp. 39.* On separating the great cylinder from a 14th part of it, the model and machine continuing in their places, it was found, that half a turn of the wheel was sufficient to charge the little cylinder, so as to cause the like appearance at the spout.

15th Observation.—Now these differences in the numbers of turns required for causing similar appearances, when the several charges were given in the 37th and 38th experiments, could not arise from a difference in the quantity of metallic matter contained in the respective substitutes; because the tin-foil which covered the great cylinder was found to be nearly 3 times heavier than the weight of the whole wire. Neither could these differences be owing to a difference in the quantity of surface of the respective substitutes; because the surface of the great cylinder was found to be 10 times greater than the surface of the wire. Those several differences must therefore depend on some other cause; and, as a true knowledge of this cause may be of some moment in the present inquiry, we must endeavour to find it out by experiments and observations.

Exp. 40. When the great cylinder, with the wire and brass drums, were charged with a very small quantity of this fluid, by the wheel being turned something less than a quarter round, there was the moment after, a visible explosion, and a sensible effect perceived at the remote end of the wire. When half a turn was given, these effects were greater; and, after a whole turn, the quantity of the fluid accumulated in this great apparatus was increased considerably.

16th Observation.—Now, something must have hindered the fluid from getting out of the cylinder and wire all the time they were charging, otherwise we should not have been able to have caused the least accumulation; for, from the nature of this fluid, there cannot be any accumulation without some resistance to occasion it. And whatever the nature of that resistance may be, experiments show, that there are certain bounds prescribed to its power of acting, and which in particular circumstances seem to be very easily surmounted.

Exp. 41. When the great cylinder and wire with the drums were fully charged, and a person, standing on the wire which communicated with the well, suddenly approached the brass drums with his hand, an explosion ensued, which indeed was neither so large, nor did it take place at so great a distance, as might have been expected; yet the person received a violent sensation, not unlike that produced by the Leyden phial, as it affected his body quite through, from the hand that took the discharge to the feet that stood on the wire.—*Exp.* 42. On repeating the above experiment, with the great cylinder only, and when it was fully charged, the explosion appeared stronger, and the distance it struck at greater, than in the other case; and yet the sensation received was not near so violent as when the long wire was connected with it.—*Exp.* 43. When the little cylinder by itself was fully charged, the effects were very inconsiderable, compared with those from the great cylinder: for, in this case, the person standing on the wire of communication was affected in his hand only, and that no farther than the wrist.

17th Observation.—When all the circumstances in the last two experiments are considered, we may safely conclude, that the difference in the sensation, produced by the two cylinders, could arise from no other cause than a difference in their lengths; the one being 14 times longer than the other, and both in other respects nearly similar; and since the sensation perceived in the 38th experiment, where the long wire was employed, was considerably greater than when the great cylinder alone was charged, we seem to have sufficient reason to apprehend that the effects of every charge, as to sensation, will be proportional to the length of the body charged; provided the charge be uniform from end to end in every experiment. Apprehending that, if some of the circumstances employed in producing the charge were varied, we might possibly obtain a greater charge than we had yet found, I made the following experiments.

Exp. 44. Instead of one machine to charge the great apparatus, I made use of two. The glass cylinders belonging to each were of the same length and diameter nearly. One of those machines was continued in its usual place, which was not far from the nearer end of the great cylinder. The other stood at the farther end of the brass drums. After connecting the long wire with the great cylinder and brass drums, in the manner before described, the wheels of both machines were put into motion, with equal and uniform velocities: and after six turns of each wheel, and waiting above 8 seconds, a person suddenly approached the brass drums with his hand; immediately an explosion took place, and a disagreeable sensation was perceived. The discharge was then made at the nearer end of the great cylinder, and there seemed to be no difference in the effect.

Exp. 45. On repeating the experiment with one machine only, and after

the same number of turns of the wheel with the same velocity, and waiting above 8 seconds also, the same person suddenly caused an explosion with the same hand. But the sensation in consequence of it was very different from the last experiment; for he declared it was little more than half as violent.

Exp. 46. I now charged the long wire only and fully, and with one machine: the explosion, in this case, appeared not very large, but of a reddish hue; and the distance it struck at was not more than one inch and a half; however the sensation across the body was at that instant sharp and violent, but not quite so disagreeable as when the great cylinder was connected with it, and similarly charged.

Exp. 47. Having procured an equal quantity of the same kind of wire, and of the same diameter, with that which was suspended and tried in the last experiment, it was placed in the form of coils on a board, fixed on the top of a long stand of glass, without having any connection with the great apparatus. These coils were then fully charged by the power of one of those machines only. The sensation they afforded, in consequence of causing sparks, was very inconsiderable, compared with what had been observed in the last experiment.

Exp. 48. The several coils of wire employed in the last experiment, and 700 yards more in coils also, were joined together at their several ends. These coils being then strung on silk lines, were drawn out into a form resembling that of a screw, and separated from each other in such a manner all along as to occupy 100 yards of silk line. The several diameters of these coils, at a mean, were about 15 inches. As I had so short a time in which to prepare and suspend them properly, the disadvantage of their touching and intersecting each other in many places could not be prevented; however I found that the sensation caused, after charging this wire, was nearly equal to that which had been experienced from the long wire in the 46th experiment.

Exp. 49. On joining the farther end of these coils to one end of the long wire, so that the whole length was in this experiment about 3900 yards, and afterwards charging the nearer end of the coils, and without the great cylinder, the sensation complained of by two indifferent persons, was twice as violent as the sensation perceived by the same persons when the long wire alone was charged.

It may be now proper to make some general observations respecting the explosion itself, and the quantity of the fluid discharged in consequence of it. After many experiments we found, that when the great apparatus was fully charged, and the motion of the wheel suddenly stopped, it appeared, that a single explosion at either end of it, instantly discharged the contained fluid; but never so effectually as to leave no remainder: for the quantity which did remain was generally sufficient to cause a 2d explosion perceptible to the sense of feel-

ing, as well as to that of sight. Now before the great explosion was caused, the fluid accumulated in the apparatus must have been diffused equally through it, in consequence of its elastic principle; and, being so circumstanced, a sudden application of the hand, or any other substance, which would open a door for the passage of the fluid into the earth, was found to discharge the greater part of that fluid; and whatever part was so discharged, the most distant particles seemed to have arrived at the point where the explosion took place, at the same time with those that were the nearest to it; because, immediately after the explosion, there was very little of the fluid remaining in the apparatus. If then the discharge of the fluid be, as it seems to be, very nearly instantaneous, the particles of it must move with velocities, and consequently with forces, very nearly proportional to those distances.

From this consideration I apprehend it will appear, why the sensation on the discharge from the long wire in the 46th experiment was more violent: and on recollecting the 37th, 38th, and 39th experiments, and the observations I made on them, I am inclined to believe that the effect depends more on the length of the metallic body, than on the quantity of its matter or surface.

It was on the idea of the velocity of the fluid being thus increased, that I apprehended gunpowder might be fired without the least appearance of a spark. The success of this experiment was an inducement to try Kunkell's phosphorus, which was made by Dr. Higgins. The moment this inflammable substance was brought very near the surface of the brass drums, it burst into a blaze; and common tinder, applied in the same manner, was set on fire the instant when it was brought so near as to touch the metal: but there was not the least appearance of a spark in any of these experiments. The method taken to fire the gunpowder was this: on a staff of baked wood a stem of brass was fixed, which terminated in an iron point at the top. This point was put into the end of a small tube of Indian paper, made somewhat in the form of a cartridge, about one inch and a quarter long, and about $\frac{2}{10}$ of an inch in diameter. When this cartridge was filled with common gunpowder, the wire of communication with the well was then fastened to the bottom of the brass stem. Being so circumstanced, and while the charge in the great cylinder and wire was continually kept up by the motion of the wheel, the top of the cartridge was brought so near to the drums as frequently to touch the metal. In this situation, a small faint luminous stream was observed between the top of the cartridge and the metal drum. Sometimes this stream would set fire to the gunpowder at the instant of the application; at others, it would require half a minute or more before it took effect. But this difference in time might probably arise from some difference in the circumstances, for any the least moisture in the silk lines, the powder, or in the paper itself, was unfavourable to the experiment. This new

method of firing gunpowder by a luminous stream of the matter of lightning surely merits the most serious attention; and more especially in those cases where pointed conductors are fixed to secure magazines of gunpowder from such accidents. In repeating the last experiment, there were one or two instances where the powder was fired without making use of the long wire; but never with the great cylinder alone: for we were obliged to connect it with the brass drums by means of a wire 10 or 12 feet long. It was however a considerable time, (10 minutes or more) before the experiment succeeded even with this last apparatus. This was not the case with tinder; for it fired pretty readily, and sometimes with the great cylinder alone.

Exp. 50. Before the apparatus was taken down in the Pantheon, attempts were made several days to fire gunpowder, but without success, except in one instance, which was attended with some difficulty. This failure seemed to be owing to a variety of causes, the chief of which appeared to be moisture that affected the silk lines, and perhaps the powder itself. However, when the coiled wires on the silk lines were properly joined to the long wire, as in the 49th experiment, it was found that gunpowder could be fired very readily. This is another proof of the increase of velocity by increasing the length of the wire. The explosion of gunpowder, in the particular manner related above, without the assistance of the Leyden charge, and even without a spark, is an effect which could not easily be deduced from reasoning on any experiments hitherto made; for though gunpowder has been frequently fired with the Leyden charge, yet there is a difference in the 2 cases.

5. *A Report of the Committee, appointed by the Royal Society, to consider of the most Effectual Method of Securing the Powder Magazines at Purfleet, against the Effects of Lightning; in Compliance with the Request of the Board of Ordnance.*—It being referred to us to consider, whether any thing more than what was formerly directed by the committee of the R. S. in the year 1772 can be done, for the preservation of his majesty's magazines at Purfleet: having attentively examined the experiments and observations of Mr. Wilson, contained in a paper referred to the society by the Board of Ordnance; and having maturely considered the subject at large, we submit it as our opinion:

1. That it is very improbable, that the powder magazines, guarded in the manner in which they are at present, should receive any damage from lightning.

2. That they would be still less liable to be injured if 3 other elevated pointed rods, similar to those already erected, were to be fixed on the roof of each of the 5 magazines, between the extreme rods, at equal distances from each other, with 3 strips of lead, about one foot in breadth, strongly connected with them, and carried down the roof, from the ridge to the eaves, on each side of the building; thence two of them to be continued into the earth, and to terminate at the

bottom of wells; one of which should be dug for that purpose nearly in the middle of each of the intervals between the magazines, deep enough to contain at least 4 feet of water. The middle strip should be connected with the iron rod over the door, hereafter to be mentioned. We also advise, that other high pointed rods be erected; 1 at each of the 4 corners, and one over each of the metal doors in the middle of the sides; which latter should be bent, so as to avoid the doors, in the same manner as those which are already placed on the outer side of the outermost magazines. All which rods should be continued into the earth, and be made to communicate with the bottom of the water of the nearest wells, by means of leaden pipes, closely connected with these iron rods. Likewise, that strips of lead be put on the coping of the end walls, and be made to communicate with the rods to be placed at the several corners as above directed.

3. But that the greatest degree of security would be attained by covering the whole roof, and the tops of the end walls of each of the 5 magazines, with lead; erecting all the additional conducting rods above directed; and forming a communication between the leaden covering of the roof, and the bottom of the wells as beforementioned.

As to the other buildings belonging to the magazines, we recommend: 1. That a pointed rod, similar to the rest, be erected at each end of the proof-house, and be united with the lead already there: also, that the lead on the roofs of the two low buildings, destined for the reception of the empty powder casks, &c. be connected with the wells by means of one strip of lead in the middle of each building, of the same breadth as those abovementioned.

2. That a pointed rod of copper, about three quarters of an inch in diameter, be erected on each of the 4 chimnies of the Board-house, reaching 5 feet above them; and be connected, by strips of lead, with the other lead on the roof of the building.

We also advise in general: 1. That the lead and rods on the several buildings, be respectively connected with the nearest wells, by the shortest metallic communication that can conveniently be formed; in particular, that the two leaden spouts of the Board-house, which do not reach the ground, but terminate in cisterns, be connected, by strips of lead, with those spouts that already communicate with water. 2. That the different pieces of which the iron rods may be composed, be strongly screwed together in close joints, having a thin plate of lead between them, as directed by the first committee. 3. That these rods be firmly fixed, and closely connected with the lead on the roofs. 4. That all the strips and pieces of lead be well fastened and soldered together, so as to make a perfect metallic communication with the bottom of the wells. 5. That the iron rods be painted, except in those places where they are to be in contact with the lead; that they be all 10 feet high; and that they be terminated with pieces of copper 18

inches long, like those already erected: and, 6. That these copper terminations be very finely tapered, and as acutely pointed as possible.

We give these directions, being persuaded, that elevated rods are preferable to low conductors terminated in rounded ends, knobs, or balls of metal; and conceiving that the experiments and reasons, made and alleged to the contrary by Mr. Wilson, are inconclusive.

March 12, 1778. J. Pringle, P.R.S.; W. Watson; H. Cavendish; W. Henly;
S. Horsley; T. Lane; Mahon; E. Nairne; J. Priestley.

*XVI. On the Arithmetic of Impossible Quantities. By the Rev. John Playfair,**
A. M. p. 318.

The paradoxes which have been introduced into algebra, and remain unknown in geometry, point out a very remarkable difference in the nature of those sciences. The propositions of geometry have never given rise to controversy, nor needed the support of metaphysical discussion. In algebra, on the other hand, the doctrine of negative quantities and its consequences have often perplexed the analyst, and involved him in the most intricate disputations. The cause of this diversity, in sciences which have the same object, must no doubt be sought for in the different modes which they employ to express our ideas. In geometry every magnitude is represented by one of the same kind; lines are represented by a line, and angles by an angle. The genus is always signified by the individual, and a general idea by one of the particulars which fall under it. By this means all contradiction is avoided, and the geometer is never permitted to reason about the relations of things which do not exist, or cannot be exhibited. In algebra again every magnitude being denoted by an artificial symbol, to which it has no resemblance, is liable, on some occasions, to be neglected, while the symbol may become the sole object of attention. It is not perhaps observed where the connection between them ceases to exist, and the analyst continues to reason about the characters after nothing is left which they can possibly express: if then, in the end, the conclusions which hold only of the characters be transferred to the quantities themselves, obscurity and paradox must of necessity ensue. The truth of these observations will be rendered evident by considering the nature of imaginary expressions, and the different uses to which they have been applied.

2. Those expressions, as is well known, owe their origin to a contradiction taking place in that combination of ideas which they were intended to denote. Thus, if it be required to divide the given line AB (fig. 2, pl. 4) $= a$ in c , so that $AC \times CB$ may be equal to a given space b^2 , and if $AC = x$, then $x = \frac{1}{2}a \pm \sqrt{(\frac{1}{4}a^2 - b^2)}$; which value of x is imaginary when b^2 is greater than $\frac{1}{4}a^2$; now

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to suppose that b^2 is greater than $\frac{1}{4}a^2$, is to suppose that the rectangle $AC \times CB$ is greater than the square of half the line AB , which is impossible. The same holds wherever expressions of this kind occur. Thus, when it is asserted that unity has the 3 cube roots $1, \frac{-1 + \sqrt{-3}}{2}, \frac{-1 - \sqrt{-3}}{2}$, no more is meant than that when the general equation $x^3 - ax^2 + bx - r = 0$ is, by a change in the data, reduced to the particular state $x^3 - 1 = 0$, x is then equal to unity only, and admits not of any other value, as it does in more general forms of the equation. The natural office of imaginary expressions is, therefore, to point out when the conditions, from which a general formula is derived, become inconsistent with each other; and they correspond in the algebraic calculus to that part of the geometrical analysis, which is usually stiled the determination of problems.

3. This however is not the only use to which imaginary expressions have been applied. When combined according to certain rules, they have been put to denote real quantities, and though they are in fact no more than marks of impossibility, they have been made the subjects of arithmetical operations; their ratios, their products, and their sums, have been computed, and, what may seem strange, just conclusions have in that way been deduced. Yet, the name of reasoning cannot be given to a process into which no idea is introduced. Accordingly geometry, which has its modes of reasoning that correspond to every other part of the algebraic calculus, has nothing similar to the method we are now considering; for the arithmetic of mere characters can have no place in a science which is immediately conversant with ideas.

But though geometry rejects this method of investigation, it admits, on many occasions, the conclusions derived from it, and has confirmed them by the most rigorous demonstration. Here then is a paradox which remains to be explained. If the operations of this imaginary arithmetic are unintelligible, why are they not also useless? Is investigation an art so mechanical, that it may be conducted by certain manual operations? or is truth so easily discovered, that intelligence is not necessary to give success to our researches? These are difficulties which it is of some importance to resolve, and on which much attention has not hitherto been bestowed. Two celebrated mathematicians, Bernoulli and Maclaurin, have indeed touched on this subject; but being more intent on applying their calculus, than on explaining the grounds of it, they have only suggested a solution of the difficulty, and one too by no means satisfactory. They alledge,* that when imaginary expressions are put to denote real quantities, the imaginary characters involved in the different terms of such expressions do then compensate or destroy each other. But beside that the manner in which this compensation is made, in

* Op. J. Bern. tom. i. N^o 70. Maclaur. Flux. art. 699—763.—Orig.

expressions ever so little complicated, is extremely obscure, if it be considered that an imaginary character is no more than a mark of impossibility, such a compensation becomes altogether unintelligible: for how can we conceive one impossibility removing or destroying another? Is not this to bring impossibility under the predicament of quantity, and to make it a subject of arithmetical computation? And are we not thus brought back to the very difficulty to be removed? Their explanation cannot of consequence be admitted; but, on attempting another, it behoves us to observe, that a more extensive application of this method, than had been made in their time, has now greatly facilitated the inquiry. We begin then with considering the manner in which the imaginary expressions, supposed to denote real quantities, are derived; and the cases in which they prove useful for the purposes of investigation.

4. Let a be an arch of a circle of which the radius is unity, and let c be the number which has unity for its hyperbolic logarithm, then the sine of the arch a , or $\sin. a = \frac{c^a \sqrt{-1} - c^{-a} \sqrt{-1}}{2\sqrt{-1}}$; and $\cos. a = \frac{c^a \sqrt{-1} + c^{-a} \sqrt{-1}}{2}$. These exponential and imaginary values of the sine and cosine are already well known to geometers; and the investigation of them, according to the received arithmetic of impossible quantities, may be as follows. Let $\sin. a = z$, then $\dot{a} = \frac{\dot{z}}{\sqrt{1-z^2}}$. To bring this fluxion under such a form that its fluent may be found by logarithms, both numerator and denominator are to be multiplied by $\sqrt{-1}$; then $\dot{a} = \sqrt{-1} \times \frac{\dot{z}}{\sqrt{(z^2-1)}}$, and (by form. 6. Harm. Men.) $a = \sqrt{-1} \times \log. \frac{z + \sqrt{(z^2-1)}}{\sqrt{-1}}$. Hence $\frac{a}{\sqrt{-1}}$, or $1 \times \frac{a}{\sqrt{-1}} = \log. \frac{z + \sqrt{(z^2-1)}}{\sqrt{-1}}$, and because 1 is the log. of c , $c^{\frac{a}{\sqrt{-1}}} = \frac{z + \sqrt{(z^2-1)}}{\sqrt{-1}}$; therefore, if both parts of the fractional index of c be multiplied by $\sqrt{-1}$, $c^{-a\sqrt{-1}} = \frac{z + \sqrt{(z^2-1)}}{\sqrt{-1}}$. Again, if the arch a be considered as negative, its sine becomes also negative, and therefore $-a = \sqrt{-1} \times \log. \frac{-z + \sqrt{(z^2-1)}}{\sqrt{-1}}$, or, $-a\sqrt{-1} = -\log. \frac{-z + \sqrt{(z^2-1)}}{\sqrt{-1}}$, and $a\sqrt{-1} = \log. \frac{-z + \sqrt{(z^2-1)}}{\sqrt{-1}}$; whence also, $c^{a\sqrt{-1}} = \frac{-z + \sqrt{(z^2-1)}}{\sqrt{-1}}$. If from this equation the former be taken away, there remains $-\frac{2z}{\sqrt{-1}} = c^a \sqrt{-1} - c^{-a\sqrt{-1}}$, whence dividing by $2\sqrt{-1}$ we have $z = \sin. a = \frac{c^a \sqrt{-1} - c^{-a\sqrt{-1}}}{2\sqrt{-1}}$. By adding together the equations a value of the cosine may be found in the same imaginary terms which were assigned above. Now by means of these expressions many theorems may be demonstrated; it may, for example, be shown, that if a and b are any two arches of a circle, of which the radius is unity, then $\sin. a \times \cos. b = \frac{\sin. a + b}{2} + \frac{\sin. a - b}{2}$. For $\sin. a = \frac{c^a \sqrt{-1} - c^{-a\sqrt{-1}}}{2\sqrt{-1}}$, and $\cos. b =$

$$\frac{c^b \sqrt{-1} + c^{-b} \sqrt{-1}}{2}, \text{ therefore, } \sin. a \times \cos. b =$$

$$\frac{c^{a+b} \times \sqrt{-1} - c^{-a-b} \times \sqrt{-1} + c^{a-b} \times \sqrt{-1} - c^{b-a} \times \sqrt{-1}}{4 \sqrt{-1}} = \frac{\sin. a + b}{2} + \frac{\sin. a - b}{2}.$$

5. Now it may be observed, that the imaginary value which has been found for $\sin. a$ was obtained by bringing a fluxion, properly belonging to the circle, under the form of one belonging to the hyperbola. It may therefore be worth while to inquire, whether a similar expression may not be derived from the hyperbola itself.

Let BAD be a rectangular hyperbola (fig. 3) of which the centre is c, and the semi-transverse axis $AC = 1$: let B be any point in the hyperbola, join BC, and let BE be an ordinate to the transverse axis. Then, if the sector $ACB = \frac{1}{2}a$, and $BE = z$, it is known that $a = \frac{z}{\sqrt{1+z^2}}$; whence $a = \log. z + \sqrt{1+z^2}$, and $c^a = z + \sqrt{1+z^2}$. But if the sector be taken on the other side of the transverse axis, a and z become negative, and $c^{-a} = -z + \sqrt{1+z^2}$. Hence $z = \frac{c^a - c^{-a}}{2}$; in like manner the absciss belonging to ACB, that is $CE = \frac{c^a + c^{-a}}{2}$. These values of the ordinates and abscissæ differ in nothing from those of the sines and cosines already found, except in being free from impossible quantities; for it is evident, that the quantity a is related in the same manner both to the circular and hyperbolic sectors. If now the ordinate a and abscissa b denote the ordinate and absciss belonging to the sectors $\frac{1}{2}a$, $\frac{1}{2}b$ respectively, then $\text{ord. } a \times \text{abs. } b = \frac{c^a - c^{-a}}{2} \times \frac{c^b + c^{-b}}{2} = \frac{c^{a+b} - c^{-a-b} + c^{a-b} - c^{b-a}}{4} = \frac{\text{ord. } a + b}{2} + \frac{\text{ord. } a - b}{2}$.

6. The conclusions in both the foregoing cases are perfectly coincident, and the methods by which they have been obtained are similar; though with this difference between them, that in the first all the steps are unintelligible, but in the last significant. If then, notwithstanding a difference which might be expected so materially to affect their conclusions, they have been equally successful in the discovery of truth, it can be ascribed only to the analogy which takes place between the subjects of investigation; an analogy so close, that every property belonging to the one may, with certain restrictions, be transferred to the other. Accordingly, every imaginary expression, which has been found to belong to the circle in the preceding calculation, is by the substitution of real for impossible quantities, or of $\sqrt{1}$ for $\sqrt{-1}$, converted into a proposition which holds of the hyperbola. The operations therefore performed with the imaginary characters, though destitute of meaning themselves, are yet notes of reference to others which are significant. They point out indirectly a method of demonstrating a certain property of the hyperbola, and then leave us to conclude from analogy that the same property belongs also to the circle. All that we are assured of by the imaginary investigation is, that its conclusion may, with all the strictness of mathematical reasoning, be proved of the hyperbola; but if from thence

we would transfer that conclusion to the circle, it must be in consequence of the principle which has been just now mentioned. The investigation therefore resolves itself ultimately into an argument from analogy; and, after the strictest examination, will be found without any other claim to the evidence of demonstration. Had the foregoing proposition been proved of the hyperbola only, and afterwards concluded to hold of the circle, merely from the affinity of the curves, its certainty would have been precisely the same as when a proof is made out by the intervention of imaginary symbols.

8. Though it might readily be concluded, that the same principle on which the foregoing investigation has been found to proceed, extends itself to all those in which imaginary expressions are put to denote real quantities, it may yet be proper to make trial of its application in some other instances. Let AB, AC, AD, AE (fig. 4) be any arches of a circle in arithmetical progression, and let m be their number; it is required to find the sum of the sines $BC, CH, \&c.$ of those arches. Let the radius $AF = 1$, $AB = a$, and the common difference of the arches, or $BC = x$: the sum of the series $\sin. a + \sin. (a + x) + \sin. (a + 2x) + \dots (m)$ is to be found. Now, because $\sin. a = \frac{ca\sqrt{-1} - c^{-a}\sqrt{-1}}{2\sqrt{-1}}$, and $\sin. a + x = \frac{c^{a+x}\sqrt{-1} - c^{-a-x}\sqrt{-1}}{2\sqrt{-1}}$, &c.; the series $\sin. a + \sin. a + x + \sin. a + 2x \dots (m) = \frac{c^a\sqrt{-1}}{2\sqrt{-1}} \times 1 +$
 $cx\sqrt{-1} + c2x\sqrt{-1} \dots (m) = \frac{c^{-a}\sqrt{-1}}{2\sqrt{-1}} \times 1 + c^{-x}\sqrt{-1} + c^{-2x}\sqrt{-1} \dots (m)$. But these series are both geometrical progressions, and the sum of the first is $\frac{c^a\sqrt{-1}}{2\sqrt{-1}} \times \frac{1 - c^{mx}\sqrt{-1}}{1 - c^x\sqrt{-1}}$; and of the second, $\frac{c^{-a}\sqrt{-1}}{2\sqrt{-1}} \times \frac{1 - c^{-mx}\sqrt{-1}}{1 - c^{-x}\sqrt{-1}}$. The sum of the proposed series therefore is

$$= \frac{c^a\sqrt{-1}}{2\sqrt{-1}} \times \frac{1 - c^{mx}\sqrt{-1}}{1 - c^x\sqrt{-1}} - \frac{c^{-a}\sqrt{-1}}{2\sqrt{-1}} \times \frac{1 - c^{-mx}\sqrt{-1}}{1 - c^{-x}\sqrt{-1}} = \frac{1}{2\sqrt{-1}} \times$$

$$\frac{c^a\sqrt{-1} - c^{a+mx}\sqrt{-1} - c^{-a-x}\sqrt{-1} + c^{a+mx-x}\sqrt{-1}}{1 - c^x\sqrt{-1} - c^{-x}\sqrt{-1} + 1} + \frac{1}{2\sqrt{-1}} \times$$

$$\frac{-c^{-a}\sqrt{-1} + c^{-a-mx}\sqrt{-1} + c^{-a+x}\sqrt{-1} - c^{-a-mx+x}\sqrt{-1}}{1 - c^x\sqrt{-1} - c^{-x}\sqrt{-1} + 1};$$

in which expression, if the sines be substituted for their imaginary values, we have $\frac{\sin. a - \sin. (a + mx) - \sin. (a - x) + \sin. (a + mx - x)}{2 \times (1 - \cos. x)} = \sin. a + \sin. (a + x) +$
 $\sin. (a + 2x) \dots (m)$. Q. E. I.

When $AB = BC$, or $a = x$, the proposed series becomes $\sin. x + \sin. 2x + \sin. 3x \dots (m)$, and its value = $\frac{\sin. x - \sin. (m + 1) \times x + \sin. mx}{2 \times (1 - \cos. x)}$.

In like manner it will be found, that the sum of the cosines of the same arches, or $\cos. a + \cos. (a + x) + \cos. (a + 2x) + \dots (m) =$
 $\frac{\cos. a - \cos. (a + mx) - \cos. (a - x) + \cos. (a + mx - x)}{2 \times (1 - \cos. x)}$; and when $a = x$, $\cos. x + \cos.$

$$2x + \cos. 3x \dots (m) = \frac{\cos. mx - \cos. (m + 1) \times x}{2 \times (1 - \cos. x)} - \frac{1}{2}.$$

9. To solve the same problem, in the case of the hyperbola, we must follow

the steps which have been traced out by these imaginary operations. Let ABE be an equilateral hyperbola (fig. 5) of which the centre is F , and the transverse axis $AF = 1$; let ABF , ACF , ADF , &c. be any sectors in arithmetical progression, and let m be their number; it is required to find the sum of all the ordinates BG , CH , DK , &c. belonging to those sectors. Let the sector $AFB = \frac{1}{2}a$, and the sector BFC , which is the common difference of the sectors, $= \frac{1}{2}x$: then BG , or ord. $a = \frac{c^a - c^{-a}}{2}$, and CH , or ord. $a + x = \frac{c^{a+x} - c^{-a-x}}{2}$, by art. 5. Therefore the series of ordinates, that is, $BG + CH + DK + \dots (m) = \frac{c^a}{2} \times (1 + c^x + c^{2x} + \dots (m) - \frac{c^{-a}}{2} \times (1 + c^{-x} + c^{-2x} + \dots (m) = \frac{c^a}{2} \times \frac{1 - c^{mx}}{1 - c^x} - \frac{c^{-a}}{2} \times \frac{1 - c^{-mx}}{1 - c^{-x}} = \frac{1}{2} \times \frac{c^a - c^{a+mx} - c^{-a-x} + c^{-a-mx}}{1 - c^x - c^{-x} + 1} =$
 $\frac{\text{ord. } a - \text{ord. } (a + mx) - \text{ord. } (a - x) + \text{ord. } (a - mx)}{2 \times (1 - \text{abs. } x)}$. When $a = x$, ord. $x + \text{ord. } 2x + \text{ord. } 3x + \dots (m) = \frac{\text{ord. } x - \text{ord. } (m + 1) \times x + \text{ord. } mx}{2 \times (1 - \text{abs. } x)}$.

In like manner it is proved that the sum of the abscissæ, that is, $FG + FH + FK + \dots (m) = \frac{\text{abs. } a - \text{abs. } (a + mx) - \text{abs. } (a - x) + \text{abs. } (a - mx)}{2 \times (1 - \text{abs. } x)}$; and when $a = x$, this expression becomes $\frac{\text{abs. } mx - \text{abs. } (m + 1) \times x}{2 \times (1 - \text{abs. } x)} - \frac{1}{2}$.

10. The coincidence of the theorems deduced in the last two articles is obvious at first sight, and if the methods by which they have been obtained be compared, it will appear, that the imaginary operations in the one case were of no use but as they adumbrated the real demonstration which took place in the other. This will be rendered more evident by considering that the resolution of the series of hyperbolic ordinates, into two others of continual proportionals, can be exhibited geometrically. For, from the points A , B , C , and D , let AM , BN , CO , DP , be drawn at right angles to the asymptote FP ; let GB produced meet FP in Q , and let BR be perpendicular to the conjugate axis FR . Then, because the triangles FRS , FMA , are equiangular, $AF : FM :: FS : FR$; hence $FR = \frac{FM}{FA} \times FS = \frac{FM}{FA} \times (FN - NB)$. For the same reason $CH = \frac{FM}{FA} \times (FO - OC)$ and $DK = \frac{FM}{FA} \times (FP - PD)$. Therefore, $BG + CH + DK = \frac{FM}{FA} \times (FN + FO + FP) - \frac{FM}{FA} \times (BN + CO + DP)$; now, FN , FO , FP , are continual proportionals, and so also are BN , FO , FP , because the sectors FBC , FCD , are equal. But in the circle no such resolution of the proposed series of sines can take place, that series being subject to alternate increase and diminution; on which account it is, that imaginary characters enter into the exponential value of the sine. Those characters are therefore so far from compensating each other in the present case, as they ought to do, on the supposition of Bernoulli and Maclaurin, that they manifestly serve as marks of impossibility. There remains, of consequence, the affinity between

circular arches and hyperbolic areas, or between the measures of angles and of ratios, as the only principle on which the imaginary investigation can proceed. It need scarcely be observed, that the exponential value of the hyperbolic ordinate may be deduced from what has been proved in this article.

11. But as the arithmetic of impossible quantities is no where of greater use than in the investigation of fluents, it is of consequence to inquire, whether the preceding theory extends also to that application of it. Let it then be required to find the fluent of the equation $\frac{y}{x^2} \mp a^2 y = Q$, where Q denotes any function whatever of x . For this purpose, the following lemma is premised: let x be any arch, and p any flowing quantity; then, if the sign \int be taken to denote the fluent of the quantity to which it is prefixed, $\sin. x \int p \cos. x - \cos. x \int p \sin. x = \frac{c^x \sqrt{-1}}{2\sqrt{-1}} \int p c^{-x \sqrt{-1}} - \frac{c^{-x \sqrt{-1}}}{2\sqrt{-1}} \int p c^{x \sqrt{-1}}$; or if $\frac{1}{2}x$ be a hyperbolic sector, ord. $x \int p \text{abs. } x - \text{abs. } x \int p \text{ord. } x = \frac{c^x}{2} \int p c^{-x} - \frac{c^{-x}}{2} \int p c^x$.

Because $\sin. x \int p \cos. x = \frac{c^x \sqrt{-1} - c^{-x \sqrt{-1}}}{2\sqrt{-1}} \int p \times \frac{c^x \sqrt{-1} + c^{-x \sqrt{-1}}}{2}$, by separating the terms we have $\sin. x \int p \cos. x = \frac{c^x \sqrt{-1}}{4\sqrt{-1}} \int p c^{x \sqrt{-1}} + \frac{c^x \sqrt{-1}}{4\sqrt{-1}} \int p c^{-x \sqrt{-1}} - \frac{c^{-x \sqrt{-1}}}{4\sqrt{-1}} \int p c^{x \sqrt{-1}} - \frac{c^{-x \sqrt{-1}}}{4\sqrt{-1}} \int p c^{-x \sqrt{-1}}$, for the same reason $-\cos. x \int p \sin. x = -\frac{c^x \sqrt{-1}}{4\sqrt{-1}} \int p c^{x \sqrt{-1}} + \frac{c^x \sqrt{-1}}{4\sqrt{-1}} \int p c^{-x \sqrt{-1}} - \frac{c^{-x \sqrt{-1}}}{4\sqrt{-1}} \int p c^{x \sqrt{-1}} + \frac{c^{-x \sqrt{-1}}}{4\sqrt{-1}} \int p c^{-x \sqrt{-1}}$. Wherefore, by collecting the sum of all the terms, we have $\sin. x \int p \cos. x - \cos. x \int p \sin. x = \frac{c^x \sqrt{-1}}{2\sqrt{-1}} \int p c^{-x \sqrt{-1}} - \frac{c^{-x \sqrt{-1}}}{2\sqrt{-1}} \int p c^{x \sqrt{-1}}$.

The demonstration in the case of the hyperbola is free from imaginary expressions; but, in other respects, is exactly similar to that which has now been given in the case of the circle.

12. Let the co-efficient of y in the proposed equation be first supposed negative, that is, let $\frac{y}{x^2} - a^2 y = Q$, and if we multiply by $c^{nx} \dot{x}$, n being a constant but indeterminate quantity, it becomes $\frac{c^{nx} y}{x} - a^2 c^{nx} y \dot{x} = c^{nx} Q \dot{x}$. Let $c^{nx} \times (\frac{A y}{x} - B y)$ be assumed for the fluent, A and B being indeterminate, and let its fluxion be taken, then,

$$\frac{A c^{nx} y}{x} + n A c^{nx} y - n B c^{nx} y \dot{x} = c^{nx} Q \dot{x}.$$

$$- B c^{nx} y.$$

Hence, by comparing the terms, we get $A = 1$, $nA - B = 0$, $nB = a^2$; therefore, $n = \pm a$, and $B = \pm a$: for n and B let the value $+a$ be substituted, and for A , its value, unity; then the assumed equation becomes

$(\frac{y}{x} - ay) \times c^{ax} = \int c^a Q \dot{x}$, or $\frac{y}{x} - ay = c^{-ax} \int c^{ax} Q \dot{x}$. Let this equation be

multiplied by $c^{mx}\dot{x}$, m being indeterminate as before, then $c^{mx}\dot{y} - ac^{mx}y\dot{x} = c^{(m-a)x}\dot{x} \int c^{ax}Q\dot{x}$. The fluent of the first member of this equation is evidently of the form $Dc^{mx}y$, the fluxion of which, viz. $Dc^{mx}\dot{y} + Dmc^{mx}y\dot{x}$ being compared with the former gives $D = 1$, and $m = -a$; therefore, $c^{-ax}y = \int c^{-2ax}\dot{x} \int c^{ax}Q\dot{x}$, or $y = c^{ax} \times \int c^{-2ax}\dot{x} \int c^{ax}Q\dot{x}$. Let $c^{ax}Q\dot{x} = \dot{z}$, and $c^{-2ax}\dot{x} = \dot{v}$; then $\int c^{-2ax}\dot{x} \int c^{ax}Q\dot{x} = \int \dot{z}\dot{v} = zv - \int v\dot{z}$; but $v = \frac{1}{2a} - \frac{c^{-2ax}}{2a}$, supposing that v and x vanish at the same time; therefore $vz - \int v\dot{z} = \frac{1}{2a} \int c^{ax}Q\dot{x} - \frac{c^{-2ax}}{2a} \int c^{ax}Q\dot{x} - \frac{1}{2a} \int c^{ax}Q\dot{x} + \frac{1}{2a} \int c^{-ax}Q\dot{x} = \frac{1}{2a} \int c^{-ax}Q\dot{x} - \frac{c^{-2ax}}{2a} \int c^{ax}Q\dot{x}$. Hence $y = \frac{c^{ax}}{2a} \int c^{-ax}Q\dot{x} - \frac{c^{-ax}}{2a} \int c^{ax}Q\dot{x}$. This value of y is sufficient for the construction of the fluent, because the quantities $\int c^{-ax}Q\dot{x}$, and $\int c^{ax}Q\dot{x}$ depend on the quadrature of the hyperbola; but if we would introduce into it the ordinates and abscisses of that curve, we need only have recourse to the foregoing lemma, from which it appears, that $y = \frac{1}{a} \text{ord. } ax \int Q\dot{x} \text{ abs. } ax - \frac{1}{a} \text{abs. } ax \int Q\dot{x} \text{ ord. } ax$.

13. Let the co-efficient of y be now supposed affirmative, or let $\frac{\ddot{y}}{\dot{x}^2} + a^2y = Q$. In this case imaginary expressions are introduced into the fluent, and the construction by the hyperbola becomes impossible. For we have then, $n = \pm a\sqrt{-1}$, from which, by proceeding as above, we get $y = \frac{c^{ax\sqrt{-1}}}{2a\sqrt{-1}} \int c^{-ax\sqrt{-1}}Q\dot{x} - \frac{c^{-ax\sqrt{-1}}}{2a\sqrt{-1}} \int c^{ax\sqrt{-1}}Q\dot{x}$; hence also, by the lemma, $y = \sin. ax \int Q\dot{x} \cos. ax - \cos. ax \int Q\dot{x} \sin. ax$. Here the quantities, $\int Q\dot{x} \cos. ax$, and $\int Q\dot{x} \sin. ax$, are assignable by the quadrature of the circle, in the same manner as $\int Q\dot{x} \text{ abs. } ax$, and $\int Q\dot{x} \text{ ord. } ax$, by the quadrature of the hyperbola; but the method of investigating them, though an illustration of the principles which we have laid down, is too well known to need to be inserted here. In like manner might the fluents of innumerable fluxionary equations, comprehended under the general form $Q = y + \frac{a\dot{y}}{\dot{x}} + \frac{b\ddot{y}}{\dot{x}^2} + \frac{d\ddot{\dot{y}}}{\dot{x}^3} + \&c.$ be deduced, and all of them would tend to prove that the arithmetic of impossible quantities is no more than a method of tracing the analogy between the measures of ratios and of angles. M. M. Euler,* and D'Alembert,† were the first to integrate such equations as the preceding, and the method employed here differs from theirs only by being better adapted to illustrate the principle which is common to them all.

* Nov. Com. Petrop. tom. iii.—Orig.

† Théorie de la Lune.—Orig.

14. The forms in the *Harmonia Mensurarum* might also be brought to confirm this theory: but, without accumulating instances any further, it may be sufficient to remark two consequences that follow from it: 1. That the only cases in which imaginary expressions may be put to denote real quantities, are those in which the measures of ratios or of angles are concerned. 2. That the property of either of those measures, so investigated, might have been inferred from analogy alone. Now both these conclusions are agreeable to experience. It does not appear, that any instance has yet occurred where imaginary characters serve to express real quantities, if circular arches or hyperbolic areas are not the subjects of investigation; and if the conclusion obtained may not be transferred from the one to the other, by a mere substitution of corresponding magnitudes; that is, of sines for ordinates, cosines for abscisses, and circular arches for the doubles of hyperbolic sectors. The affinity between the circle and hyperbola is not however so close, but that it is subject to certain limitations, from considering which, the truth of what is here asserted will be rendered more evident.

(1.) Any proposition demonstrated of hyperbolic sectors may be transferred to circular arches by substitution alone, without any change in the signs, when only abscissæ and their products enter into the enunciation, and conversely.

Thus $\text{abs. } a \times \text{abs. } b = \frac{\text{abs. } (a+b)}{2} + \frac{\text{abs. } (a-b)}{2}$; and $\cos. a \times \cos. b = \frac{\cos. (a+b)}{2} + \frac{\cos. (a-b)}{2}$. The same holds when the simple power of the ordinate is combined with any power whatever of the absciss: so in the theorems of art. 5 and 4, $\text{ord. } a \times \text{abs. } b = \frac{\text{ord. } (a+b)}{2} + \frac{\text{ord. } (a-b)}{2}$; and $\sin. a \times \cos. b = \frac{\sin. (a+b)}{2} + \frac{\sin. (a-b)}{2}$.

(2.) When an expression containing any property of hyperbolic sectors, involves in it the rectangle of two ordinates, the value of that rectangle must have a contrary sign, when a transition is made to the circle. Thus $\text{ord. } a \times \text{ord. } b = \frac{\text{abs. } (a+b)}{2} - \frac{\text{abs. } (a-b)}{2}$; but $\sin. a \times \sin. b = -\frac{\cos. (a+b)}{2} + \frac{\cos. (a-b)}{2}$. The difference which, according to this rule, is found between the powers of ordinates and of sines, may be seen in the following examples. If $\frac{1}{2}x$ denote any hyperbolic sector, then, by involving $\frac{e^x - e^{-x}}{2}$, and again substituting for the exponential quantities as in art. 5, we have,

$$(\text{ord. } x)^2 = \frac{\text{abs. } 2x - 1}{2};$$

$$(\text{ord. } x)^3 = \frac{\text{ord. } 3x - 3 \text{ ord. } x}{4};$$

$$(\text{ord. } x)^4 = \frac{\text{abs. } 4x - 4 \text{ abs. } 2x + 3}{8};$$

$$(\text{ord. } x)^5 = \frac{\text{ord. } 5x - 5 \text{ ord. } 3x + 10 \text{ ord. } x}{16};$$

and universally, if n be any number; a the co-efficient of the 2d term of a binomial raised to the power

n , b the co-efficient of the 3d, &c. and p the greatest co-efficient: when n is an even number, $(\text{ord. } x)^n = \frac{\text{abs. } nx - a \text{ abs. } (n-2) \times x + b \text{ abs. } (n-4) \times x \dots \mp p}{2^{n-1}} \pm \frac{p}{2^n}$;

but when n is an odd number,

$(\text{ord. } x)^n = \frac{\text{ord. } nx - a \text{ ord. } (n-2) \times x + b \text{ ord. } (n-4) \times x \dots \mp p \text{ ord. } x}{2^{n-1}}$. If now x de-

note an arch of a circle, by substituting and changing the signs as oft as $(\text{ord. } x)^2$ occurs in any of the preceding expressions, we get

$$(\sin. x)^2 = \frac{1 - \cos. 2x}{2};$$

$$(\sin. x)^3 = \frac{3 \sin. x - \sin. 3x}{4};$$

$$(\sin. x)^4 = \frac{3 - 4 \cos. 2x + \cos. 4x}{8};$$

$$(\sin. x)^5 = \frac{10 \sin. x - 5 \sin. 3x + \sin. 5x}{16};$$

and universally, if n be any number, p the greatest co-efficient of a binomial raised to the power n , A the co-efficient next less than p , B the co-efficient next less than A , and so on:

when n is an even number, $(\sin. x)^n = \frac{\frac{1}{2}p - A \cos. 2x + B \cos. 4x - \&c.}{2^{n-1}}$; but when

n is an odd number, $(\sin. x)^n = \frac{p \sin. x - A \cos. 3x + B \cos. 5x - \&c.}{2^{n-1}}$.

These series differ from the former only in the signs, and the arrangement of the terms: and when either n , or $n - 1$, is divisible by 4, the signs remain the same in both.

16. The reason of the foregoing rule for changing the signs is, that the rectangle under two ordinates to the hyperbola is always expressed by the difference of two abscissæ: and that if from the absciss belonging to a greater sector, be subtracted the absciss belonging to a less, the remainder will be affirmative; whereas, if from the cosine of a greater arch be subtracted the cosine of a less, the remainder will be negative. Therefore, that the rectangles, expressed by these remainders, may have the same sign, in both cases, the signs of the remainders must be different.

It appears then, that the 2d rule, as well as the first, is founded on the principle of analogy when taken with the necessary limitations, and it is likewise evident from the instances which have been produced, that those rules lead to the very same conclusions which are obtained from the imaginary values of the sine and cosine. There are however instances, in which the analogy between the circular and hyperbolic areas being wholly interrupted, neither the foregoing rules, nor any of the same kind, can be applied; but this occasions no ambiguity, for the construction required in such cases is by its nature restricted to one of the curves only. Of this kind is the Cotesian theorem, which requires the whole circle to be divided into a given number of equal parts, and therefore cannot be extended to the hyperbola where a similar division is impossible.

Others of a like nature may be derived from the general theorems already investigated; for the circle, by returning into itself, often reduces them to a simplicity to which there is nothing analogous in the hyperbola. Many examples of this might be adduced, but the two following may suffice. (1.) Let $ABCDE$ (fig. 6) be a regular polygon inscribed in a circle, and let m be the number of its sides; it is required to find the sum of the lines FA , FB , FC , &c. drawn from any point F in the circumference, to all the angles of the polygon. By the method which in art. 8 was employed to obtain the sum of the sines of a series of arches in arithmetical progression, it will be found, that the sum of the chords of the arches a , $a + x$, $a + 2x$, (m), that is, (making $FA = a$, and $AB = x$) the sum of the chords of the arches FA , FB , FC , &c. =

$$\frac{\text{cho. } a - \text{cho. } (a + mx) - \text{cho. } (a - x) + \text{cho. } (a + mx - x)}{2 \times (1 - \cos. \frac{1}{2}x)}$$
; but, in the present case, mx is equal to the circumference, and therefore $-\text{cho. } (a + mx) = +\text{cho. } a$ (the chord of an arch greater than the circumference being negative); and, for the same reason, $\text{cho. } (a + mx - x) = -\text{cho. } (a - x) = +\text{cho. } (x - a)$. Hence the general expression becomes $\frac{\text{cho. } a + \text{cho. } (x - a)}{1 - \cos. \frac{1}{2}x} = FA + FB + FC + \dots (m)$.

If therefore GK be drawn from the centre, bisecting the chord AB in H , and meeting the circumference in K , the sum of the chords, that is, $FA + FB + FC + FD + FE = \frac{AF + FB}{FK} \times GK$.

(2.) Let n be an even number, the rest remaining as above, and let it be required to find the sum of the n powers of the chords, that is, the sum of $FA^n + FB^n + FC^n \dots (m)$. By reasoning, as in the case of the sines, it will appear that, if p be the greatest co-efficient of a binomial raised to the power n ; A the co-efficient next less than p ; B the co-efficient next less A ; and so on; then,

$$(\text{cho. } a)^n = p - 2A \cos. a + 2B \cos. 2a + 2D \cos. 3a + \&c.$$

$$(\text{cho. } a + x)^n = p - 2A \cos. (a + x) + 2B \cos. 2 \times (a + x) + 2D \cos. 3 \times (a + x) \&c.$$

$$(\text{cho. } a + 2x)^n = p - 2A \cos. (a + 2x) + 2B \cos. 2 \times (a + 2x) + 2D \cos. 3 \times (a + 2x) \&c.$$

&c.

Each of these vertical columns is to be continued downward, till the number of terms be equal to m , and therefore the sum of the 2d is mp . The sum of the 3d, or of $-2A \times (\cos. a + \cos. a + x + \cos. a + 2x \dots (m))$, by art. 8, is $-2A \times \frac{\cos. a - \cos. (a + mx) - \cos. (a - x) + \cos. (a + mx - x)}{2 \times (1 - \cos. x)} =$ (because $mx =$ the circumference $- A \times \frac{\cos. a - \cos. a - \cos. (a - x) + \cos. (a - x)}{1 - \cos. x} = 0$. In like manner do the sums of all the subsequent columns vanish; and therefore, $\text{cho. } a + \text{cho. } a + x + \text{cho. } a + 2x \dots (m) = mp$. But when n is an even number, $p = \frac{\frac{1}{2}n + 1}{\frac{1}{2}n - 1} \times \frac{\frac{1}{2}n + 2}{\frac{1}{2}n - 2} \dots \times \frac{n}{\frac{1}{2}n} = \frac{1.3.5.7 \dots n - 1}{1.2.3.4 \dots \frac{1}{2}n} \times 2^{\frac{1}{2}n}$. If therefore the radius be put $= r$, and the expression be made homogeneous, we have $FA^n + FB^n + FC^n \dots (m) = m \times \frac{1.3.5.7 \dots n - 1}{1.2.3.4 \dots \frac{1}{2}n} \times 2^{\frac{1}{2}n} r^n$. Q. E. I.

This last coincides with the 41st of the curious and difficult propositions published by Dr. Stewart, under the title of general theorems; it is given there without a demonstration, but appears plainly to have been investigated, in a manner altogether rigorous, by that profound geometer. It may therefore be regarded as one of the instances, in which the conclusions of this imaginary arithmetic are verified by the geometrical analysis.

17. The two foregoing propositions being confined to the circle, and yet having been investigated by the help of imaginary expressions, may, at first sight, seem exceptions to the rule we have been endeavouring to establish. But it needs only to be remarked, that they are particular cases of certain theorems belonging both to the circle and hyperbola, and that it was into the investigation of those theorems, that the imaginary expressions were introduced.

The conclusions therefore from the whole are these: that imaginary expressions are never of use in investigation but when the subject is a property common to the measures both of ratios and of angles; that they never lead to any consequence which might not be drawn from the affinity between those measures; and that they are indeed no more than a particular method of tracing that affinity. The deductions into which they enter are thus reduced to an argument from analogy, but the force of them is not diminished on that account. The laws to which this analogy is subject; the cases in which it is perfect, in which it suffers certain alterations, and in which it is wholly interrupted, are capable, as may be concluded from the specimens above, of being precisely ascertained. Supported on so sure a foundation, the arithmetic of impossible quantities will always remain a useful instrument in the discovery of truth, and may be of service when a more rigid analysis can hardly be applied. For this reason, many researches concerning it, which in themselves might be deemed absurd, are yet not destitute of utility. M. Bernoulli has found, for example, that if r be the radius of a circle, the circumference $= \frac{4 \log. \sqrt{-1}}{\sqrt{-1}} r$; and the same may be deduced from art. 4. Considered as a quadrature of the circle, this imaginary theorem is wholly insignificant, and would deservedly pass for an abuse of calculation; at the same time we learn from it, that if in any equation the quantity $\frac{\log. \sqrt{-1}}{\sqrt{-1}}$ should occur, it may be made to disappear, by the substitution of a circular arch, and a property, common to both the circle and hyperbola, may be obtained. The same is to be observed of the rules which have been invented for the transformation and reduction of impossible quantities:* they facilitate the operations of this imaginary arithmetic, and thus lead to the knowledge of the most beautiful and extensive analogy which the doctrine of quantity has yet exhibited.

* The rules chiefly referred to are those for reducing the impossible roots of an equation to the form $A + B \sqrt{-1}$.—Orig.

XVII. Reflections on the Communication of Motion by Impact and Gravity. By the Rev. I. Milner, M.A., Fellow of Queen's College, Cambridge. p. 344.*

The theory of moving bodies was little understood by the philosophers who lived in the 16th century. They observed, that a body, once put into motion, continued to move for some time after the force was impressed; but they argued very strangely from this ordinary phenomenon. Far from considering the air as a resisting medium; they supposed, with Aristotle, and the ancients, that it was the perpetual influx of the parts of the atmosphere which continued to urge the body forward and preserve its motion. When a body is projected in any direction inclined to the horizon, the gravity of its parts is always observed to bend the direction of its motion into a curve line; and because this gravity remains invariably the same, whatever the force of projection be, in very swift motions, the figure described may approach very nearly to a right line. This last circumstance induced some of those philosophers to believe, that a cannon ball, for instance, always moves in the same straight line till its velocity is entirely destroyed; and that afterwards it descends towards the earth in a direction perpendicular to the horizon. Others thought they mended the matter by suspending the action of gravity for a certain period only; by allowing the latter part of the path to be curvilinear; and lastly, the body to descend to the earth in a straight line, as in the former case. But we, who have now seen the gradual improvements in mechanics from time to time, are not surprized, that men, in the infancy of that science, should have embraced absurd and ridiculous principles: we rather wonder, how Tartalea, the author of the notion just mentioned, was able to form any just estimate of the horizontal ranges of projectiles, and to discover their maxima. Whether, by conjecture, or probability of induction, we are unable to determine; but so it was, Tartalea affirmed, what has since been found true on unexceptionable evidence, that the amplitudes of projectiles on the horizon, are always greatest when the angles of projection are equal to 45° . But the praise of this discovery, as well as whatever else relates to the accelerated motions of bodies near the surface of the earth, is justly due to the incomparable Galileo. The theory of mechanics had received no inconsiderable improvement since the time of Archimedes, when this surprising genius appeared in the former part of the 17th century. He discarded the peripatetic philosophy; explained the whole doctrine of accelerated motion and of projectiles: in short, he so much exhausted the subject, that the best treatises we have at this day are little more than a repetition of Galileo's discoveries.

This philosopher never attempted to investigate the laws by which motion is

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communicated from one body to another. Des Cartes is the first we know of who gave any attention to the subject; and the result of his inquiries is what might reasonably be expected from so whimsical and romantic a genius; he blundered in this, as in all other cases, where he was not confined to pure mathematical reasonings. Our countryman, Dr. Wallis, made a real progress in this science, by discovering that fundamental law in the communication of motion, viz. that action is equal to re-action, and always in contrary directions: Wren, Huygens, confirmed the same thing; and the whole theory of the collision of bodies, and their mutual actions on one another, seemed to be advancing fast towards perfection. But a new opinion was now started by M. Leibnitz concerning the forces of bodies in motion. The force of a body in motion and its momentum had hitherto been considered as synonymous terms, and had alike been measured by the quantity of matter and velocity conjointly. On the contrary, Leibnitz and his followers affirmed, that the force was proportional to the quantity of matter in the moving body and the square of its velocity. It is needless to relate all that passed on both sides: so material an opposition in sentiment necessarily produced very warm contention; and, as it generally happens in other disputes, we do not hear of any conviction being produced on either side.

After surveying the arguments of the disputants, it is not easy to say, whether the agitation of the question before us has contributed to retard or advance the progress of truth and science. On the one hand, many ingenious experiments have been made, many curious problems invented and resolved, which probably would never once have been thought of by men who were in the pursuit of truth in a more cool and deliberate way: and, on the other hand, it may justly be affirmed, that the violence of prejudice and party-spirit has so much clouded the reasonings of the best writers, that we sensibly feel their influence to this day. I need not dissemble: it is a serious persuasion, that the laws by which motion is communicated are still very materially mistaken by sensible persons, that induced me to throw together the following hints, and to lay them before the R. S. The right understanding of these laws is of the last importance in practice: the good or bad success of some very expensive projects has depended on it; and certain excellent artists have been disappointed in the execution of their plans, and unable to reconcile the apparent contradiction between theory and experiment. From the length of time which has elapsed since Leibnitz first advanced his new opinions, and the abilities of philosophers who engaged in the contest, one might have expected, that the whole matter would long before this have been cleared up in a satisfactory manner; especially when we consider, that the communication of motion from one body to another is what every moment happens before our eyes, and that particular experiments are made in

this doctrine with the greatest simplicity and convenience. This part of rational mechanics however is not yet generally understood, as we may fairly presume from the difference of opinion which still subsists among the learned. I freely own, it appears to me, that no new experiments are wanting; no new geometrical reasonings or constructions: the improved parts of geometry have been already applied to the theory of motion in numberless cases, and a variety of well attested experiments have been clearly explained to us by authors. The laws of motion, in certain cases, are incontestible, and no author of eminence contradicts them: it is from a mistaken application of these laws, that a difference of opinion has arisen. It is obvious, that the laws of motion, as described by Sir Isaac Newton, may, in a certain sense, be founded on experiment; and yet, if they are extended to cases where they cannot be applied, the conclusions must still be erroneous. My design in these pages is to point out distinctly what is real in this difference of opinion from what is merely verbal, and to explain the causes of it. This, which perhaps will appear to have never been done with sufficient precision, seems to be the most effectual way of preventing mistakes. Geometry and algebra will lead us wrong, if our principles are ill founded: experiment itself, if we are not extremely careful, will deceive us in forming a general deduction, or what is called a law of nature. The controversial writings of the most able authors will embarrass and perplex our judgements; but when we have once discovered the grounds of their mutual mistakes and misapprehensions, there is reason to think, that we shall both understand the subject better than we did before, and be more on our guard for the future.

The first law of motion, as expressed by Sir Isaac Newton, is unexceptionable: nobody denies that a body perseveres in a state of rest or uniform motion in a right line, till affected by some external influence. It is the third law of motion which has produced all this confusion and perplexity. "*Actioni contrariam semper et æqualem esse reactionem: sive corporum duorum actiones in se mutuo semper æquales et in partes contrarias dirigi.*" These words of Sir Isaac Newton convey to us as clear an idea as can possibly be conceived with so much conciseness. It must however be confessed, that his illustration is not so very perspicuous. To say, that when a man presses a stone with his finger, his finger is equally pressed; and when a horse draws a stone by a cord, the horse is drawn equally backwards towards the stone; is a most indistinct and popular way of speaking, and can never make evident what was before not understood.

Some useful writers, who have copied after Sir Isaac Newton, have talked in the same way; and only increased the ambiguity by being more diffuse. Mr. Maclaurin himself, who engaged very warmly in this debate with the foreign mathematicians, and who, to say the truth, seems to have understood the

nature of the controversy better than any one else, is frequently unguarded in his expression. In chap. 2, book 2, of his account of Newton's discoveries, he is describing the laws of motion for the first time, and one naturally expects a more than ordinary precision and exactness. There he blames, very justly, the opposers of the Newtonian definition of motion for mistaking the direction in which the motion, lost or communicated, ought always to be estimated. But in p. 122, 8vo. edit. he thus expresses himself: "When two bodies meet, each endeavours to persevere in its state, and resists any change; and because the change, which is produced in either, may be equally measured by the action which it exerts on the other, or by the resistance which it meets with from it, it follows, that the changes produced in the motions of each are equal; but are made in contrary directions." I cannot possibly conceive, that so skilful and accurate a philosopher could believe, that the 3d law of motion was an inference of reason, exclusive of all experiment; and yet, if words have any meaning at all, the above quotation inclines us to think so. It is true, the change which is produced in either body may be measured by the action which it exerts on the other, or by the resistance which it meets with from the same: but what are we to understand by action or resistance, until they are explained by more intelligible terms? or, when they are explained by terms which do not necessarily imply the same thing, how do we know that their measures are equal, or that they are made in contrary directions, until these truths are established by experiments? A law of nature is not merely a deduction of reason: it must be proved, either at once and directly, by some simple and decisive experiments; or if that cannot be done, by such experiments as enable us to collect its existence by the assistance of geometry. However obvious these reflections may appear, I thought it necessary to take notice of Maclaurin's assertion; because in consequence of that and similar expressions, young philosophers are extremely puzzled in the beginning of their studies, and because I have known some, who are more experienced, affirm, that the 3d law of motion is nothing more than a definition. I now proceed to the consideration of particular cases.

Case 1.—Suppose A and B to represent the magnitudes of two spherical bodies, and a and b their respective velocities in the same direction; suppose a to be greater than b , then A will overtake B ; and if the bodies are non-elastic, they will proceed together in the same direction as one mass: if they are perfectly elastic, whatever effect has already been produced by the collision, will be repeated; and, because in the first case there is no relative velocity after the stroke, in the 2d the relative velocity before and after the stroke will be the same, and in contrary directions; and in either case, the motion lost by the striking body is found to be always equal to the motion communicated to B , and in a contrary direction. In this sense action is equal to reaction; and every experi-

ment which has yet been produced, where a clear judgment could be formed of the effect, has confirmed the same thing. All the experiments usually brought to determine the impressions made on soft bodies, as snow, clay, &c. are absolutely unfit for the purpose. The circumstances, which take place in the production of these effects, are such as we can never discover. The directions in which the particles recede, the velocities they acquire, their mutual actions on each other, and lastly the time in which these effects are performed, are all beyond the reach of computation. The other principle, that the relative velocity of A and B is not altered by the stroke, is neither to be demonstrated nor confirmed by experience; it is a direct consequence of the definition of elasticity. Again, suppose α and β to represent the respective velocities of A and B after the stroke; then from these data it is easily inferred, that $A\alpha^2 + B\beta^2 = Aa^2 + Bb^2$: for $a - b$ is equal to $\beta - \alpha$, because $a - b$ is the relative velocity before, and $\beta - \alpha$ the relative velocity after the stroke. And $Aa + Bb$ is equal to $A\alpha + B\beta$, because these quantities represent the sum of the motions before and after the stroke respectively; and from these equations the above equation is deduced; showing, that in elastic bodies the sum of the two bodies multiplied by the squares of their absolute velocities, is not altered by the stroke.

The same theorem may be demonstrated geometrically in the following manner. Let the velocities of A and B be represented by AD, AB, respectively, fig. 7, pl. 4; and let G be their centre of gravity, when placed at B and D; the velocity of A after the stroke will be represented by Bg, if Gg be taken equal to GD, and the velocity of B by AB + 2BG. From the nature of the centre of gravity, $A \times GD = B \times BG$, and $A \times GD \times 4AG = B \times BG \times 4AG = B \times (4BG^2 + 4BG \times AB)$. Add to both sides $A \times Ag^2 + B \times AB^2$, and we shall have $A \times AD^2 + B \times AB^2 = A \times Ag^2 + B \times (AB + 2BG)^2$.

We are not to wonder therefore, on making trials with perfectly elastic bodies, if any such existed, were we always to find their vires vivæ, as the foreigners express themselves, neither increased nor diminished by the stroke. They define the force of bodies in motion, or their vis viva, to be in a compound ratio of their quantities of matter, and the squares of their velocities; and certainly such a definition implies no contradiction or impossibility. The term force, in a loose and ordinary way of speaking, conveys to us no determinate idea at all, and therefore, until it be defined, is incapable of being used to any good purpose in philosophy: whether this or that definition come nearer to the general sense in which it is used indistinctly enough in common language, is entirely another question. We may go farther, and add, that in their use of the words, because the sum of the forces of elastic bodies is never affected by the stroke, it is not unnatural to say, that action is therefore equal to re-action, and that no force is lost by one body but what is communicated to the other. But

if we will go so far, and thereby change the meaning of the terms action and re-action and their measures, we ought at least to guard our readers from mistaking us, however convenient such modes of expression may appear. Because $A\alpha^2 + B\beta^2$ is equal to $Aa^2 + Bb^2$, it is true that no force is lost by A but what is communicated to B; but not in the same sense in which it was affirmed that no motion is lost by A but what is communicated to B. In that case the squares of their absolute velocities are understood; in this, their velocities reduced to the same direction. However, no material ill consequence can possibly arise from such a notion of action and re-action, as long as the question is supposed to concern only elastic bodies: but real mischief is done, and the debate ceases to be verbal, whenever the law of the equality of action and re-action is said to take place in the collisions of all sorts of bodies whatever.

Case 2.—But the truth of these remarks, and the necessity of attending to the precise use of terms, will appear in a still stronger light, if we consider the solution of a problem given by J. Bernoulli, in his *Discours sur le mouvement*. Suppose that two equal and spherical bodies, A and B, fig. 8, struck at once in the direction CD, perpendicular to the line joining the centres of A and B, with a velocity represented by a . Let the quantity of matter in c be called m , and the quantity of matter in A or B, n : let the velocity of c after the stroke be represented by x , and that of A or B, in the direction AC or CB, by y , and suppose $p : q :: \text{rad.} : \text{cosin. LCD}$. Then, because ma , the quantity of motion before the stroke, is equal to $mx + \frac{2qny}{p}$, the quantity of motion after the stroke, and ma^2 is equal to $mx^2 + 2ny^2$, because the quantity of force is not altered by the collision; he easily finds $x = \frac{p^2ma - 2q^2na}{p^2m + 2q^2n}$, and $y = \frac{2pqma}{p^2m + 2q^2n}$.

There is no problem which deserves to be more considered than this, by a person desirous of having a clear idea of the grounds of that contention which has subsisted so many years. We here see Bernoulli taking it for granted, that the quantity of force in elastic bodies is no ways affected by their mutual actions, whether direct or oblique; and the most surprizing circumstance is, that he should not so much as hint at any apparent difficulty in the present case, after he had been so very diffuse in illustrating others which were much more simple. No doubt he believed this principle to be a direct consequence of the equality of action and re-action, and therefore it is plain he could not mean the same things by those terms as we do at present. He believes no force is gained or lost by impact; he defines force by quantity of matter and square of the velocity conjointly; and in estimating the velocity, he pays no regard to the direction in which the bodies are moved. Let us not cavil at his words: we cannot mistake his meaning. The question is, how far these notions are agreeable to experience; how far they are consistent with some other principles which are incon-

testible, and which he himself has admitted: for instance, he admits it as an undoubted principle, that the quantity of motion in any system of bodies is preserved invariable, when estimated in a given direction, in all their collisions and mutual actions on one another; and in this he entirely agrees with the followers of Sir Isaac Newton. Let us attend to the consequences of these two different principles in the very case proposed by J. Bernoulli. And first, because $ma = mx + \frac{2qny}{p}$, by transposition we have $m \times (a - x) = \frac{2qny}{p}$, which is saying no more than that the motion lost by *c* is equal to the sum of the motions gained by *A* and *B*, estimated in the same direction *CD*. By a similar process, from the 2d equation, we deduce $m \times (a + x) \times (a - x) = 2nq^2$; and therefore the comparison of the two equations gives $\frac{q \times (a + x)}{p} = y$. The quantity *y* therefore, or the velocity of *A* or *B* after the stroke, must necessarily be equal to the sum of the two quantities $\frac{qa}{p}$ and $\frac{qx}{p}$. In the figure, let *CD* represent the velocity of *c* before the stroke, and *CH* the velocity after it, and let fall the perpendiculars *hn*, *DL*, on the direction *AC*. It easily appears, that *cn* is equal to $\frac{qx}{p}$, and *CL* equal to $\frac{qa}{p}$, because *CH* : *CN* :: *CD* : *CL* :: *rad.* : *cos. LCD* :: *q* : *p*. And now the whole controversy is reduced into a narrow compass; for whether the two principles assumed by this author be consistent with experience or not, it is impossible they should be consistent with each other, unless *cn* + *CL* shall be found to measure the velocity of *A* in the direction *CL*. Suppose *cr* to be the velocity of *c* after impact, when all the bodies are perfectly hard, and letting fall the perpendicular *rs*, *cs* will be the velocity acquired by *A* in that case; and, universally, the velocity acquired by *A* will be equal to $cs + \frac{cs}{m}$, if the elasticity of the bodies be to perfect elasticity as 1 : *m*. In order to determine therefore when *cn* + *CL* can possibly be equal to $cs + \frac{cs}{m}$, or, which is the same thing, *ls* + *cn* equal to $\frac{cs}{m}$, we are to consider that *ns* : *ls* :: 1 : *m* : and because *cn* is equal to $cs - sn$, $cn = cs - \frac{ls}{m}$, and it is obvious that $cs + ls - \frac{ls}{m}$ can never be equal to $\frac{cs}{m}$, unless *m* be taken equal to unity, and Bernoulli's hypothesis is plainly impossible in all cases where the bodies are not supposed perfectly elastic.

But though we confess the learned author, who first solved the problem we have been considering, deserves no commendation for proposing in a general form what ought to have been restrained to a particular case, yet it will by no means follow, that every argument which has been advanced against this doctrine is either intelligible or satisfactory. Of all the objections and experiments which have been started and contrived to refute the new opinions of the German phi-

losophers, there is none which carries a greater degree of plausibility along with it, than a celebrated invention of Mr. Maclaurin. It is extremely simple, easy to be described; and I do not find that it has ever been answered by any of the advocates for the new doctrine of forces.

Let A and B, fig. 9, be two equal bodies that are separated from each other by springs interposed between them, in a space EFGH, which in the mean time proceeds uniformly in the direction BA (in which line the springs act) with a velocity as 1; and suppose that the springs impress on the equal bodies A and B equal velocities, in opposite directions, that are each as 1. Then the absolute velocity of A (which was as 1) will now be as 2; and according to the new doctrine its force as 4; whereas the absolute velocity and force of B (which was as 1) will now be destroyed; so that the action of the springs adds to A a force as 3, and subducts from the equal body B a force as 1 only; and yet it seems manifest that the actions of the springs on these equal bodies ought to be equal, and M. Bernoulli expressly owns them to be so.* I shall only just observe, that if M. Bernoulli expressly owns, that springs, interposed between two bodies in a space, which is carried uniformly in the direction in which the springs act, will always generate equal forces in the bodies according to his own definition of that term, he talks more inconsistently than I have observed him to do: on the contrary, if I could find that he has answered this famous argument (which Dr. Jurin proposed over again in Phil. Trans. vol. 43, with a conditional promise of embracing the Leibnitzian doctrine) by simply saying, that springs he considers as motive forces, or, when the bodies are equal, as accelerating forces; and that their actions are equal, when in equal times they generate equal velocities, but not necessarily equal forces, in the equal bodies; I should not make the least scruple to own that I thought his reasoning solid and conclusive, and his distinctions a full answer to every objection of that sort.†

Case 3.—The two preceding cases are curious examples of the force of prejudice and party-spirit. In the latter particularly it does not appear that J. Bernoulli knew the preservation of the vires vivæ to be an infallible consequence of perfect elasticity in bodies; or indeed that he had any other reason for taking that principle for granted, but because he was not able to prove it. All the in-

* Book II. chap. 2. Account of Newton's discoveries.—Orig.

† No doubt Maclaurin refers to the following passage of Bernoulli, "La force du choc, ou de l'action des corps les uns sur les autres, depend uniquement de leurs vitesses respectives; or il est visible que les vitesses respectives des corps ne changent pas avant le choc, soit que le plan ou l'espace qui les contient soit sans mouvement, soit qu'il se meuve uniformement, suivant une direction donnée, les vitesses respectives seront donc encore les mêmes après le choc." This quotation puts the matter beyond dispute. It is plain that Bernoulli, though he does make use of the word action, is only speaking of the motion lost or communicated, and the relative velocities of the bodies: there is not the most distant hint at the change in their absolute forces.—Orig.

stances that are usually brought on both sides are to be treated in a similar way. The meaning of the terms must first be defined; then the principles assumed explained; and if we cannot tell at first sight, whether they are agreeable to experience or not, as is frequently the case, we must examine into their consequences by the assistance of geometry, and we shall at last arrive at some simple principle, the existence of which is necessarily implied in the original hypothesis. The collision of spherical bodies is the most simple way of communicating motion from one to another; and therefore such examples are better adapted to throw light on a disputable question, than where the suppositions are more perplexed with mechanical contrivances. Besides, when the theory of mechanics is well understood, and the foundations of error discovered, the same reasonings are easily transferred to other cases, and similar precautions applied. Indeed practical artists have little to do with the sudden communication of motion by impact. The collisions of bodies are too violent operations to enter into the composition of useful machines, in which motions are rather to be preserved by the gradual effects of weights and pressures. An accurate knowledge therefore of these effects is more essential to the interests of society; and the only way of arriving at such a knowledge, is always to distinguish those principles which nobody denies, from those others which are found to take place only in some particular circumstances. The following problem was proposed, and a solution given to it long ago, by D. Bernoulli, in the *Comment. Petrop. tom. 2.*

“Sit grave aliquod cujuscunque figuræ CBA, fig. 10, cujus centrum gravitatis sit D; ex quo et radio DM descriptus intelligatur circulus MNP, cui filum circumvolutum est PMN, cujus fili extremitati appensum sit pondus a, quod descensu suo grave CBA in gyrum agit circum centrum gravitatis D; dico velocitatem corporis a sequentem in modum determinari posse. Sit MD = a, consideretur corpus suspensum ex puncto M oscillari, esseque centrum oscillationis in o, sitque DO = b, pondus gravis totius CBA = P, pondus corporis appensi = p; altitudo ex quâ corpus a delapsus est = R; altitudo quæsitæ per quam grave aliquod cadendo acquirere possit velocitatem corporis a = z; dico fore $z = \frac{apR}{ap + bp}$, et si tempus quo corpus naturaliter cadit, per altitudinem R dicatur t, erit tempus insumptum a corpore a = $t\sqrt{\frac{ap + bp}{ap}}$, id quod experientiæ conforme esse plurimis institutis experimentis semper inveni.”

Both these conclusions are derived by this author from the principle, which they call the conservatio virium vivarum; but as he has not given the several steps of his reasoning, it may be useful to supply them here, before we proceed to make any remarks on his solution. And first, suppose the axis at D to be perpendicular to the plane of the figure, and conceive the whole body to be resolved into an indefinite number of prismatic particles, each of which is perpen-

dicular to the same plane. Let E represent the sum of all the particles multiplied by the squares of their respective distances from the axis; then E shall be equal to $p \times ab$, as is demonstrated by all the writers who treat of the centre of gyration. Let v be the velocity actually acquired by a after it has descended through the space R ; v the velocity which it would have acquired by the same descent, provided the body had fallen freely by its gravity; and because the vires vivæ are incapable of diminution or increase, we have $pv^2 = pv^2 + \frac{pabv^2}{a^2}$. For since v is the velocity of a at a certain period of its descent, and is to the velocity of any prismatic particle in the body, as the distance mD from the axis to the distance of that particle from the same, it is evident that $\frac{pabv^2}{a^2}$ will truly represent the sum of all the particles multiplied by the squares of their velocities. v^2 is therefore to v^2 as $ap + bP$ to ap , and the whole force of gravity is to the force which accelerates the motion of a in the same ratio, because in uniformly accelerated motions, when the spaces described are the same, the accelerating forces are in the duplicate ratio of the velocities. It is obvious that the motion of a is uniformly accelerated, because the velocity acquired by any descent, is to the velocity of any point in the body, always in the same ratio; and therefore the action of a on the body is the same as if both were at rest. Further, the altitude z , through which a heavy body must fall to acquire the velocity v , is plainly equal to $R \times \frac{ap}{ap + bP}$; for the altitudes z and R are inversely as the forces which generate the equal velocities. Lastly, the time of a 's descent is equal to $t \times \sqrt{\frac{ap + bP}{ap}}$; because the times are always in the sub-duplicate ratio of the spaces directly, and forces inversely.

It is now extremely easy to trace these expressions back again in a contrary order, and to show, that if these last equations are true, the original one must be true also; that $p \times v^2$ must necessarily be equal to $\frac{pabv^2}{a^2} + pv^2$, or, which is the same thing, that the body a multiplied into the square of its velocity, and added to the sum of all the products which arise by multiplying every particle into the square of its respective velocity, is equal to the body a multiplied by the square of the velocity which it would have acquired by the same descent in vacuo.

Now this is to give the argument its full force; and since the conclusions are confirmed by repeated experiments, as the author himself assures us, it is presumed, that the premises can be liable to no just exception. If we do not think, with the advocates for this doctrine, that the vires vivæ must always remain the same from the thing itself, they will force our assent by the testimony of experience, and oblige us to admit their principles when we find it impossible to deny the consequences.

A prudent philosopher is always afraid to pronounce generally concerning the existence of causes, which are attended with a variety of circumstances, and are complex in their operations. To say that the quantity of force in bodies remains invariably the same, seems to be a proposition of this kind. The mutual actions of bodies on each other, especially when their gravity is taken into the question, depends on so many considerations, and the cases which may be put are capable of such an infinite variation, that it is impossible almost to draw a general inference of this nature. Even when experiments are produced, which seem to prove the point, one is apt to suspect the universality of the conclusion, and to imagine that it may possibly be owing to some particular circumstance which we have not attended to, or been able to distinguish from others not so essential. In the example we are considering, it is clearly proved from experience, that $p \times v^2$ is equal to $pv^2 + \frac{Fv^2}{a^2}$; but whether that be true in every other case that may be conceived, can never be determined from such an experiment; nor is it possible to make any distinctions about it, until we have demonstrated its connexion with some other principle, which is more simple and less contested.

Retaining the same symbols, let F represent the force of gravity, and f the force which accelerates the body a in its motion. From what has been already shown it appears, that $F:f :: ap + bF : ap$, and $F - f : f :: bF : ap :: \frac{abFv}{a^2} : pv$; and because pv is the motion generated in a by the force f , $\frac{abFv}{a^2}$ will be the motion lost in the same body a by the diminution of its gravity. Let A be any prismatic particle of the body, and AD its distance from the axis; the velocity of this particle will be $\frac{v \times AD}{a}$; its motion $\frac{A \times AD \times v}{a}$; and, by the nature of the lever, the motion which a must lose to generate such an effect in A must be $\frac{A \times AD^2 \times v}{a^2}$. The quantity $\frac{vabF}{a^2}$ represents the sum of all the quantities $\frac{A \times AD^2 \times v}{a^2}$; and therefore the motion, which a has lost by its action on the body, is precisely equal to the motion gained by the different parts of that body after a proper allowance is made for the lengths of the levers, AD , &c.

Thus it appears, that there is no necessity, in accounting for the time of a 's descent and the velocity it acquires, of having recourse to the conservatio vis vivæ, or any such perplexed hypothesis. By pursuing the analytic method far enough, we have been led directly to that fundamental law of motion, that action is equal to re-action, and in the contrary direction.

A distinction however is always to be made between the actions of bodies when at liberty, and when they revolve about a centre or axis. In the first case, the motion lost is always equal to the motion communicated in an opposite direction: in the second, the motion lost is to be increased or diminished in the ratio of the

levers, before it will be equal to the motion communicated. The properties of the lever are well understood and easily applied, and because their evidence depends on experience, and is as firmly established as the 3d law of motion itself, it is always best to make use of those two universal principles, instead of others which are more liable to deceive us.*

In all cases concerning the motion of a single body, or system of bodies, where there is any rotatory motion, the consideration of the lever becomes requisite, and that, with a just application of the laws of motion, is sufficient for the resolution of the most arduous problems. It is now pretty well agreed on, that the neglect of this circumstance is one cause of that material error, which Sir Isaac Newton himself is supposed to have fallen into in the 39th prop. of the 3d book of his Principia.

I had several reasons for insisting so particularly on the demonstration of this 3d case. It is in itself one of the most neat and elegant problems we have; and, what is of more consequence, it admits of an experimental proof and illustration. It is obvious, that the motion of the body AMB may be made so slow, that the time of α 's descent through any assignable space may be measured to the greatest exactness. The velocity of α may also be inferred with the same ease by observing the velocity of any particular point in the body to which the velocity of α always bears an invariable ratio. Such experiments, it must be owned, seem very unfit for the first discovery of the laws of nature; though, as I have shown, it is not impossible to collect them that way; but after they are discovered, the application of them to the solution of such intricate problems is both entertaining and instructive, and then the agreement of the experiments themselves with the theory becomes a solid argument for the certainty of our principles.

We have shown, that in this case at least Bernoulli's hypothesis is founded on, and coincides with, the commonly received doctrine of motion, and therefore we can hardly entertain a doubt of the success of the experiment, supposing it had never been tried. The author himself, in the passage above quoted, tells us, that he found it so; but we need not rest on his authority: a similar experiment

* It is acknowledged, that the experiments which have been made to determine the effects of wind and water-mills do not agree with the computations of mathematicians; but this is no objection to the principles here maintained. Writers generally propose such examples with a view rather of illustrating the methods of calculation by algebra and fluxions, than of making any useful improvements in practice. They suppose the particles of the fluid to move in straight lines, and to strike the machine with a certain velocity, and after that, to have no more effect. As such suppositions are evidently inconsistent with the known properties of a fluid, we are not at a loss to account for a difference between experiment and theory; and therefore it should seem unreasonable to assert, that certain authors of reputation have neglected the collateral circumstances of time, space, or velocity, in the resolution of these problems, unless we were able to point out such omissions.—Orig.

has been lately made by Mr. Smeaton, and is described at length in p. 72, of this volume.

It does not appear, that D. Bernoulli attempted to measure any thing but the time of a 's descent through any particular space; Mr. Smeaton has given both the times of a 's descent, and the proportions of the velocities acquired, in a variety of cases. By moving the weights he makes use of nearer to, or farther from, the centre D , he alters the lengths of the levers at which the particles act, without increase or diminution of their number: he does the same with the circle or axis NMP , and consequently the lever MD ; and in every case, from the known character of that ingenious gentleman, we may presume that his numbers are safely to be relied on. His conclusions may receive some illustration from the preceding theory.

From the proportion $F : f :: ap + bp : ap$, it appears, that the force which accelerates the motion of a , or in Mr. Smeaton's figure, the weight in the scale is to the natural force of gravity, in a constant and invariable proportion, as long as the quantities a , b , p , and p , remain the same; and therefore let a descend ever so slowly, its motion will be uniformly accelerated throughout, and the spaces through which it descends will be as the squares of the velocities acquired, and the times will be as the velocities themselves; and this is agreeable to what Mr. Smeaton found them in his 2d, 3d, 5th, 6th, 8th, and 9th experiments.

The general expression for the force which accelerates the weight in the scale is $\frac{ap + bp}{F \times ap}$, and will be different according as the quantities a , p , or b , are altered; but is always easy to be determined as soon as those quantities are known. But it is impossible to determine the magnitude of the quantity b in the different cases, unless we have given the precise dimensions of the whole machine; and the specific gravity of the wood made use of; and therefore I confess myself to have been puzzled in endeavouring to reconcile the 1st and 2d, and other experiments with the theory; for though I could not doubt a moment, that the general expression for the force was rightly assigned, and would always be found consonant to experience, yet I was extremely surprised to find, that when the quantity a in the 2d experiment was made exactly one-half of what it was in the first, the time of descending through the same space came out nearly double of what it was before, and the velocity the same. Now this I knew could never happen unless the force in the first case was to the force in the 2d as 4 to 1; for when the spaces described are the same, the accelerating forces are always as the squares of the velocities, or inversely as the squares of the times. This consideration led me to inquire further into the ratio of those forces in the case described, in order to discover, if possible, whether they came any thing near that ratio, which of necessity they ought to do.

I considered that the weight of the axis and arms of the machine was inconsiderable, compared with the weight of the 2 cylinders of lead, and also that the quantity a bore a very small proportion to the length of the cylindrical arms of fir. And since the accelerating force is always as $\frac{ap}{ap + bp}$, or as $\frac{a^2p}{a^2p + abp}$, and the quantity abp or E expresses the sum of all the particles multiplied by the squares of their distances from the axis of motion, it is plain that E must far exceed a^2p ; and lastly, since the quantity E is the same both in the 1st and 2d experiment, it follows, that the forces are very nearly to each other as a^2p to $\frac{a^2p}{4}$, or as 4 to 1: and in the same way the other experiments are shown to be consistent with the theory.

I chose to premise a short account of the opinions which the philosophers before Galileo entertained concerning the motions of bodies; because their mistaken ideas of the effects of gravity are analogous to some opinions of a later date, which indeed suggested the necessity of resuming these inquiries. And as nothing in controversial matters so completely satisfies the mind as an exact knowledge of that particular which produces the dispute; I have shown, that the terms made use of to express the 3d law of motion were taken in 2 very different senses: that Sir Isaac Newton's explication of them is at best ambiguous, and Maclaurin's absolutely false. 1st. In the demonstration of the first case, we see that the assertion of Leibnitz is true in one particular instance. When 2 elastic balls move in the same straight line, the sum of their forces is not altered by collision; and it is more than probable, that this single circumstance was the cause of affixing new ideas to the terms action and re-action. For,

2d. In the 2d case, the same principle is taken for granted by J. Bernoulli. We have examined into the consequences of this author's solution, and shown that his hypothesis will prove all bodies to be perfectly elastic. As the steps by which he deceived himself are here exposed, whoever carefully attends to these 2 examples cannot easily mistake in any case that may occur. It is plain, that if any one contends for the equality of action and re-action, and explains those terms by the changes produced in the absolute forces of the bodies, the dispute is not merely verbal.

3d. When a conclusion, agreeable to experience, is deduced from any hypothesis, it does not therefore necessarily follow, that the hypothesis is universally true, not even supposing the converse of the proposition to hold. In this 3d case it is shown, what kind of answer we are to give such reasoning. The conservatio virium vivarum is never to be admitted, unless its connection with simple facts, which are incontestable, be first made out. The solution of this problem depends on this, that the motion lost is equal to the motion communicated in a contrary direction, after the property of the lever is taken into the

account; and therefore the nice agreement of Mr. Smeaton's experiments with the theory cannot fail to add fresh evidence to these established laws of nature.

I shall conclude these remarks with observing that since it is perhaps impossible to give one general answer to all the arguments which are brought in favour of the new doctrine of forces, it seemed very desirable that we should have a general rule to direct us in judging of the cases that occur in practice. It is of more consequence to the improvement of science and the good of the public, to point out the source of mistakes, and the wisest means of avoiding them for the future, than merely to confute and silence our adversaries. Some writers have considered this question as entirely verbal, and have affected to treat the advocates on both sides with the greatest contempt. Such persons save themselves a great deal of trouble, and have the credit of seeing further into the controversy than others; but after all, I am afraid the practical mechanic will receive little information or security from such speculations. Propriety of expression in these matters is not all we want. When a plan is proposed for execution, and a certain effect predicted, the grand object is, how to form a sure judgment before-hand of the event, in order to prevent unnecessary expences; and I shall think my time well employed, if these considerations appear to have the least tendency to promote so useful an end, in the opinion of that society to whose learned and zealous endeavours we owe the very first important discoveries in the year 1668, concerning the collisions of bodies.

XVIII. Observations on the Limits of Algebraical Equations; and a General Demonstration of Des Cartes's Rule for finding their Number of Affirmative and Negative Roots. By the Rev. Isaac Milner, M. A., Fel. of Queen's Col., Cambridge. p. 380.

§ 1. The investigations of the limits of equations is considered as one of the most important problems in algebra. The knowledge of them not only enables us to demonstrate many useful theorems in that science, but is also of material service in discovering the roots themselves. Mr. Maclaurin has treated this subject very fully, both in his Algebra and in the Philosophical Transactions. The substance of what he has delivered may be briefly expressed in the two following propositions. 1st. That any equation $x^n - px^{n-1} + qx^{n-2} - \&c. = 0$ being proposed, if you take the fluxion of this equation, and divide it by x , the resulting equation will have all its roots limits of the roots of the given equation. 2dly. If the terms of the proposed equation be multiplied into the terms of any arithmetical series, the resulting equation will also have its roots limits of the roots of the original equation.

§ 2. This 2d proposition, though admitted by all the eminent authors, cer-

tainly requires some restrictions. For example, the roots of the quadratic equation $x^2 - 2x - 3 = 0$ are 3, and -1 ; multiply the terms of this equation into the terms of the arithmetical progression 1, 2, 3, respectively, and the resulting equation is $1 \times x^2 - 2 \times 2x - 3 \times 3 = 0$, the roots of which are $2 \pm \sqrt{13}$, neither of which are between the roots of the given quadratic. Again, suppose the roots of the cubic equation $x^3 - px^2 + qx - r = 0$ to be a , b , $-c$, then it is possible that the equation $(l + 3m) \times x^3 - (l + 2m) \times px^2 + (l \times m) \times qx - lr = 0$ may have no root between the quantities b and $-c$; and in general, if the roots of the equation (A) $x^n - px^{n-1} + qx^{n-2} \&c = 0$ be supposed a , b , c , $-d$, $-e$, $-f$, $\&c$. where a is the greatest root, b the next, and so on in order, the equation (B) $(l + nm) \times x^n - [l + (n - 1)m] px^{n-1} + [l + (n - 2)m] qx^{n-2} \&c. = 0$ will not necessarily have any of its roots between the roots c and $-d$ of the original equation.

§ 3. It will not be difficult to see the reason of this, if we examine the demonstration usually given of this 2d proposition. The roots of the biquadratic equation $x^4 - Ax^3 + Bx^2 - cx + D = 0$ are supposed to be a , b , c , d , and the results which arise by successively substituting them for x in $4x^3 - 3Ax^2 + 2Bx - c$ are supposed to be $-R$, $+S$, $-D$, $+Z$. From which Maclaurin concludes, that when a , b , c , d , are substituted for x in the quantity $(l + 4m) \times x^4 - (l + 3m) \times Ax^3 + (l + 2m) Bx^2 - (l + m) cx + lD$, the quantities that result will become $-mRx$, $+msx$, $-mTx$, $+mzx$, where, says he, the signs being alternately negative and positive, it follows, that a , b , c , d , must be limits of the equation $(l + 4m) \times x^4 - (l + 3m) Ax^3 + \&c. = 0$.

Here it is taken for granted, that the quantities $-mRx$, $+msx$, $-mTx$, $+mzx$, are alternately negative and positive, which is not true, unless the roots a , b , c , d , be either all positive or all negative. For suppose a , b , c , to be positive* quantities, and d a negative one; then the four results will be $-mRa$, $+msb$, $-mTc$, $-mzd$.

§ 4. In general, the roots of the equation $nx^{n-1} - (n - 1). px^{n-2} + (n - 2). qx^{n-3}$ are always between the roots of the equation (A), because the roots of this last equation substituted successively for x in $nx^{n-1} - (n - 1). px^{n-2} + \&c.$ always give the resulting quantities alternately negative and positive; but when the least of the affirmative roots, and the greatest of the negative roots of the equation (A) are substituted in (B), the quantities that result will necessarily have the same sign, and therefore it is possible that no root of the equation (B) may lie

* Philos. Trans. vol. 36. Mr. Maclaurin, who is here very diffuse on this subject, never mentions any exception of this sort. In his Algebra, art. 44, part 2, he says, he shall only treat of such equations as have their roots positive; but it may be observed, that his reasoning from art. 45 to 50 holds in all equations, the roots of which are real. The theorem in p. 182 of that treatise is not general, though applied in the 11th chapter to the demonstration of Newton's rule for finding impossible roots in all equations.—Orig.

between the least of the affirmative and the greatest of the negative roots of the equation (A).

§ 5. It is possible even, that the equation (B) may have imaginary roots, at the same time that all the roots of the equation A are real, which is contrary to what all algebraical writers have thought. For instance, the roots of the equation $x^2 - 6x - 7 = 0$ are 7 and -1 , and if the terms of this equation be multiplied by 1, -1 , -3 (an arithmetical series where the common difference of the terms is equal to 2) the resulting equation will be $x^2 + 6x + 21$, the roots of which are evidently impossible.

§ 6. However, the equation (B) can never have more than 2 imaginary roots, when the roots of the equation (A) are real. For suppose these last roots to be $+a, +b, +c, +d, -e, -f$, &c. in their order from the greatest to the least, and since the results which arise from the successive substitution of these quantities are always alternately negative and positive, that case only excepted where d and $-e$ are substituted, it is manifest, that we shall always have $n - 2$ of the roots of the equation (B) which will be limits of the equation (A).

§ 7. It is remarkable, that whenever the equation A has all its terms complete, its roots real, and some of them positive, and others negative, if $l + nm$ be assumed equal to 0, the equation B will always have one of its roots either greater than the greatest affirmative root, or less than the least negative root of the equation (A). Thus, in the quadratic $x^2 + 6x - 7 = 0$, assume any arithmetical progression 0, 1, 2, the first term of which is equal to nothing, then the equation B in this case is $6x - 14 = 0$, and $x = \frac{7}{3}$, which is greater than 1, the greatest affirmative root of the assumed equation.

§ 8. The roots of the equation (A) being still supposed $a, b, c, d, -e, -f$, &c. let m be taken equal to unity, and l any positive integer whatever; then in that case, 2 of the roots of the equation B will lie between the roots d and $-e$, one of which will be positive, and the other negative.

For example, the quadratic equation $x^2 + 6x - 7 = 0$ has its roots 1 and -7 ; and if the terms of this equation be multiplied into 3, 2, 1; 4, 3, 2; or 5, 4, 3, successively, the resulting quadratic in every case will have its 2 roots between the roots of the given equation, and one of them will be positive, and the other negative.

§ 9. The equation B, which in the last article was deduced from the equation A by taking m equal to 1, and l any positive integer, may itself be treated in the same way, and the resulting equation will, *a fortiori*, have 2 of its roots between the roots d and $-e$ of the original equation, one of which will be positive, and the other negative.

§ 10. Let $x^2 - px + q = 0$ represent any quadratic equation, the real roots

of which are α and β ; suppose $x = \frac{1}{y}$, then we shall have $1 - py + qy^2 = 0$, the roots of which equation are $\frac{1}{\alpha}, \frac{1}{\beta}$. Let the root of the equation $2qy - p = 0$ be equal to $\frac{1}{\Lambda}$, then $\frac{1}{\Lambda}$ will always lie between the quantities $\frac{1}{\alpha}, \frac{1}{\beta}$, and therefore one would think at first sight that the quantity Λ must always lie between α and β . But this would be contrary to what is proved in art. 7. In the present case Λ can never lie between α and β , unless these two quantities have the same sign, and it is obvious, that the same reasoning holds in equations of higher dimensions.

These observations, as far as I know, are entirely new. The fundamental proposition (§ 4) was, in the year 1775, communicated to Dr. Waring, Lucasian professor of mathematics in this university, and by him inserted among the additions to his *Meditationes Algebraicæ*.*

§ 11. M. Euler, at the conclusion of his 13th chap. *Calcul. Different.* has given a demonstration of Des Cartes's rules for finding the number of affirmative and of negative roots in any equation, the roots of which are real. From what I have already said, his reasonings will appear inconclusive, though I freely own, that what he has done suggested the following different method. Suppose (D) $L + mx + nx^2 + px^3 \dots + x^n = 0$, then the roots of the equation (E) $m + 2nx \dots + nx^{n-1} = 0$ will be limits of the roots of the equation (D); and therefore there must be at least as many positive roots in the equation (D) as there are in the equation (E). The same may be said of the negative roots: for since every root of the equation (E) lies between the different roots of the equation (D), it is impossible that the number of roots should be less in either case. Suppose L and mx to be both positive; then since the last term in any equation is always the product of all the roots with their signs changed, the number of positive roots in each of the equations (D) and (E) must be even: therefore the number of positive roots in (D) cannot exceed the number of those in (E) by unity; but there is in (D) one root more than in (E), and consequently it must be negative.

If both the terms L and mx are negative; because then the number of positive roots in (E) and (D) are even, it follows in the same way, that there is one negative root more in (D) than there is in (E). And lastly, if the terms L and mx have different signs, for the same reasons there must be one positive root more in the equation (D) than there is in (E).

Des Cartes's rule is, that there are as many positive roots in any equation as there are changes in the signs of the terms from $+$ to $-$, or from $-$ to $+$, and that the remaining roots are negative. From what has been demonstrated it

* See the end of *Proprietates Curvarum*.—Orig.

appears, that if this rule be true in the equation (E), it must hold also in the next equation (D) of superior dimensions; and as we know that it is true in simple and quadratic equations, it must therefore be true in cubics, in biquadratics, and so on. This is one of the best rules we have in algebra. Dr. Saunderson (vol. 2, p. 683, *Algeb.*) saw such an infinity of cases in equations of high dimensions, that he scarcely hoped for a general proof. Maclaurin's method (p. 145, *Algeb.*) is plainly impracticable when the roots are numerous, and therefore this concise demonstration will perhaps be acceptable to mathematicians.

XIX. Journal of a Voyage to the East Indies, in the Ship Grenville, Capt. Burnet Abercrombie, in the Year 1775. By Alexander Dalrymple, Esq. F. R. S. p. 389.

This journal of a voyage to the East-Indies, was kept and arranged in the form of tables, the matter in which being now useless, they are omitted, but as the manner in which they were arranged was ingenious, the following explanation of the different columns is given.

Explanation of the Columns.—1st. The date. 2d. The height of the thermometer, according to Fahrenheit's scale. This thermometer belonged to Mr. Russell, and hung in the open air in the balcony.

3d. Including 4 columns, contains the register of the marine barometers, all of which, as well as the thermometers, were made by Nairne and Blunt: those marked R and D are quicksilver, of the kind usually made by them. That marked S is compounded of quicksilver, and of a lighter fluid, for the purpose of making the alterations more visible, which is a great convenience at sea; a quicksilver thermometer being fixed to it for the sake of correcting its height, the heat by which is set down in the column marked Th. next to that marked s.

4th. The weather and winds in 4 lines: 1st line from noon to 6 p. m.; 2d, from 6 p. m. to midnight; 3d, from midnight to 6 a. m.; 4th, from 6 a. m. to noon. In the column of weather, f. denotes fair; sq. squally; c. cloudy; h. hazy; r. rain; hr. hard rain; sr. small rain; dr. r. drizzling rain; sh. showers; th. thunder; l. lightning. Also the winds are set down according to the compass, without any allowance for the variation.

5th in 2. The difference between the daily alteration of latitude by account and observation; N. denoting that the observation was to the northward of the account; s. that it was to the southward.

6th in 2. The difference between the daily alteration of longitude by the account and time-keeper; w. denoting that the longitude by the time-keeper was to the westward of account; E. that it was to the eastward. The result of those differences indicates the daily effect of current; however an error in the course sailed, or distance run by log, would make the current appear different from what it really was.

7th. The longitude from Greenwich, in 7 columns. 1st. The longitude by account. 2d. The longitude by the time-keeper, which was made by Arnold, but without his late improvements. 3d. The difference between the longitudes deduced from observations of the moon and from the time-keeper uncorrected; E denoting the time-keeper to be to the east of A; w denoting time-keeper to west of A. This, admitting the time-keeper not to be liable to any sudden changes in its rate of going, indicates the precision with which the observations of the moon may be relied on, all circumstances of weather and of the ship's motion considered. 4th. The longitude by observations of the moon's distance from the sun or stars, adjusted, by the log, to the noon nearest the time of observation. 5th. The number of sights or distances observed. 6th. The object whose distance from the

moon was observed; ☉ denoting the sun; * the star; S Spica Virginis; R Regulus; A Aldebaran; At Atair; P Pollux; F Fomalhaut; An Antares. 7th. The extreme difference between the highest and lowest observation, expressed in minutes of a degree; when the seconds amount to more than 30, the next minute above is taken, otherwise the next minute below.

8th. The latitude in two columns: 1st. The latitude by account, carried on from the land, in the same manner as the longitude by account. 2d. The latitude by observation; and where the latitude could not be had by observation, it is deduced by account from the last observation, in which case it is included within [].

9th. The correct longitude from Greenwich deduced from the time-keeper corrected by the sight of lands, of which the longitudes are known, and by observations of the moon, taking a mean of the several observations of the moon made within a short period of each other. The error of the time-keeper, between the longitude corrected by sight of land or observations of the moon, is supposed to have arisen by the time-keeper having altered its rate of going uniformly between these observations; and the intermediate longitudes are determined by the time-keeper on this supposition. Where no observations of the time were made, it is deduced by the account from the last observation of the time, and is then included within [].

10th. The magnetical observations of the variation and dip, in 7 columns. 1st. The variation by azimuth; 2d. The variation by the amplitude: * before, denoting the observation to have been in the morning; * after, denoting the observation to have been in the evening. The variation was observed by the officers with the compasses belonging to the ship. 3d. The dip with the face of the instrument to the east. 4th. Ditto to the west. 5th. The mean dip of the foregoing observations. 6th. The mean corrected, or what is supposed to be the true dip. 7th. The circumstances under which the observations of the dip were made.

11th. The miles run by log.

The dip was observed with a dipping-needle belonging to the hon. Mr. Cavendish, made by Sisson. The following remarks on the dipping-needle and observations are by Mr. Cavendish.

The ends of the axis of the dipping-needle are made conical, and turn in conical holes of bell-metal, in the manner of Mr. Lorimer's needle, described in Phil. Trans. vol. 65, p. 79. The dip was constantly observed both with the face of the instrument to the east and to the west, and the poles were changed twice during the voyage, in order to see whether the needle continued well balanced. The use of this method of observing is explained in Phil. Trans. vol. 66, p. 396.

The mean dip corrected is what is supposed to be the true dip. The foundation of this correction is as follows. By the observations on July 12th, when the poles were changed, it appears, that the marked end of the needle was too heavy, so as to make that end point $\frac{1}{6}$ of a degree too low at that place; and therefore, if we suppose that the force of magnetism is equally strong in all parts of the earth, the error thus produced in other places should be to $\frac{1}{6}$ of a degree as the cosine of the dip to the radius. The observations also made when the poles were changed at Suez, agree well enough with this supposition.

The dip was observed on board the Grenville at Deptford, after her return, in the same part of the ship in which the observations were usually made, and was found not to differ more than 5' from that observed with the same needle in a pretty large garden in London, about 5 miles distant; so that the observations on board the Grenville seem to be not much influenced by the iron-work of the ship.

XX. An Essay on Pyrometry and Arcometry, and on Physical Measures in general. By J. A. De Luc, F. R. S. From the French. p. 419.

A new hygrometer was the occasion that led Mr. De L. to the present subject. That instrument was made of ivory, as a former was, but in a glass

frame; and the effects of the humor on ivory being inconsiderable, he wanted, in order to measure them correctly, to destroy the effect of heat on the frame, which he did, as in the compound pendulum, by the expansion of a rod of brass in a contrary direction. But to do this it became necessary to determine the proportion between the dilatations of brass and glass by heat, which thus led him to pyrometry.

Mr. De L. had heard the ingenious Mr. Ramsden say, that he had a notion of a pyrometer different from all that had been invented; and knowing his great skill in philosophical and mechanical matters, applied to him, and pressed him to execute his idea. The multitude of his other engagements prevented his complying with this request; and he advised Mr. De L. to look no farther for the proportions of the expansions of brass and glass than to Mr. Smeaton's experiments, which he considered as the best that had been made. On Mr. De L. desiring him to explain by what means he thought of being able to correct the faults of the ancient instruments, he did so, and told him that he proposed measuring the expansions of bodies by the micrometer of a microscope; by which means he should obviate the greatest mechanical difficulties. He added that he had made a first trial of his method a long while ago, and was assured of the success. This idea struck Mr. De L. and being very desirous of following it, he determined to undertake the execution of it himself. Hence, reflecting that he did not want absolute measures, and that it was enough to find the proportions of dilatibility between two different bodies, he was led by that idea, he says, to a very simple method, which made all the difficulties vanish, and gave him the confidence to undertake the work. Afterwards indeed, he says, he went much farther than he expected in the absolute measures themselves. Mr. De L. then delivers a very long, tedious, and desultory discourse, on a variety of miscellaneous topics, whence no satisfactory or useful conclusions are derived.

XXI. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1777. By Thos. Barker, Esq. p. 554.

This annual abstract, by Mr. Barker, is as follows.

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the House.			Abroad.			
					Hig.	Low	Mean	Hig.	Low	Mean	
Jan.	Morn.	29.82	28.83	29.36	45½	26	36½	45	14	31	1.081
	Aftern.				46	28½	37½	47½	22½	36	
Feb.	Morn.	29.71	28.54	29.23	46	31	37	43½	14	30½	2.415
	Aftern.				48	32	38	54½	24½	38	
Mar.	Morn.	29.72	28.49	29.31	56½	38	45	52	26½	38½	1.260
	Aftern.				61½	38½	46	68	35	48	
Apr.	Morn.	29.93	28.91	29.53	52	41½	45	52½	30	40	1.586
	Aftern.				54	42	47	61	43	50	
May	Morn.	29.79	28.84	29.33	59½	46	52½	59	40½	48½	1.981
	Aftern.				62½	51	54½	73	50	59½	
June	Morn.	29.90	29.12	29.48	62½	51½	57	61½	45	53½	2.966
	Aftern.				65	52½	58	70	46	62	
July	Morn.	29.91	28.74	29.42	67	56	60	63½	49	56	3.203
	Aftern.				71	56½	62	77	55	67	
Aug.	Morn.	29.97	28.90	29.54	67	53	62	64	50	56	1.290
	Aftern.				68½	59	64	76½	55	67	
Sept.	Morn.	29.90	29.21	29.62	64½	53½	59½	59½	40	50½	0.507
	Aftern.				67½	55	61	74	53	65	
Oct.	Morn.	29.80	28.20	29.32	62	46	53	57	30	45½	4.009
	Aftern.				62	46½	54½	62½	43½	54	
Nov.	Morn.	29.90	28.60	29.48	54	41½	46	53	27	39	1.581
	Aftern.				54½	42½	47	58	35½	46	
Dec.	Morn.	30.00	28.55	29.36	44½	35½	39	44	27	33½	1.720
	Aftern.				45½	36	40	47½	30	37	
Mean of all.....				29.41	50			48			23.599

XXII. Journal of the Weather at Montreal. By Mr. Barr. p. 559.

This is an account of the thermometer, with the snow and the winds, for every day of the 4 months, Dec. 1776, and January, February, March, of the year 1777. The lowest degree of the thermometer was —6.

XXIII. Extract of Meteorological Observations made at Hawkhill, near Edinburgh. By John M'Gouan. p. 564.

Lat. $55^{\circ} 58'$ } Long. $12' 42''$ in time, per
 Long. $3^{\circ} 10\frac{1}{2}'$ w. } Astronomical Observ.
 Fahrenheit's Thermometer.

	1773.		1774.		1775.		1776.	
Mths.	At 8 h. A. M.	At 2 h. P. M.	At 8 h. A. M.	At 2 h. P. M.	At 8 h. A. M.	At 2 h. P. M.	At 8 h. A. M.	Rain. Inches.
Jan.	38.56	40.30	29.10	33.00	37.80	40.80	29.24 $\frac{1}{2}$	3.262
Feb.	35.07	40.75	36.21	40.43	39.07	43.96	35.66	2.355
Mar.	42.06	48.45	37.12	43.26	40.05	44.32	40.86	1.465
April.	45.60	51.10	43.13	48.90	46.83	53.35	45.90	1.213
May.	48.57	53.13	46.61	50.84	52.74	59.61	49.29	0.626
June.	55.19	60.06	55.10	59.70	56.60	60.43	55.50	2.367
July.	57.70	61.93	57.45	63.32	59.13	67.53	59.36	3.075
Aug.	58.26	64.77	57.25	62.21	57.65	63.67	56.73	2.410
Sept.	51.29	55.83	51.70	57.80	53.26	58.87	51.80	2.755
Oct.	46.03	50.67	48.28	52.84	45.30	50.22	46.99	1.735
Nov.	38.23	42.33	38.06	42.00	37.96	41.10	40.97	2.750
Dec.	36.42	38.48	37.32	39.97	38.58	41.48	37.77	2.080
Medium of years	46.08	50.65	44.86	49.55	47.08	52.11	45.84	26.093

XXIV. Extract of a Meteorological Journal for the Year 1777, kept at Bristol, by Samuel Farr, M. D. p. 567.

Months.	Barometer.		
	Highest.	Lowest.	Mean.
January ...	30.18	29.26	29.83
February...	29.93	28.88	29.62
March	30.05	28.80	29.30
April	30.26	29.30	29.84
May	30.10	29.16	29.61
June	30.25	29.55	29.80
July	30.29	29.30	29.84
August ...	30.27	29.35	29.89
September.	30.20	29.50	29.93
October ...	30.09	28.47	29.65
November	30.28	29.04	29.86
December.	30.38	28.83	29.56

Mean of the whole.... 29.73

XXV. Journal of the Quantity of Rain that fell at Holme, near Manchester, from 1765 to 1769; and at Barrowby, near Leeds, from 1772 to 1777. By George Lloyd, Esq., F. R. S. p. 571.

Quantity of Rain at Holme, near Manchester.

Quantity of Rain at Barrowby, near Leeds.

	1765	1766	1767	1768	1769	Average.	1772	1773	1774	1775	1776	1777	Average.
	Inch.	Inch.	Inch.	Inch.	Inch.	Inches.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inches.
Jan.	1.780	0.235	0.440	1.040	2.100	1.109	2.24	2.4	2.2	2.0	1.0	1.2	1.84
Feb.	1.300	2.030	3.655	4.400	2.17	2.711	2.79	1.7	2.0	3.5	2.2	0.7	2.15
Mar.	3.300	0.800	3.234	2.030	1.15	2.103	3.49	0.3	1.1	1.6	1.3	1.9	1.62
Apr.	3.630	2.469	0.375	2.290	1.6	1.965	1.38	1.9	1.4	0.9	0.9	2.2	1.45
May	0.900	3.333	2.750	1.070	1.63	1.937	1.20	4.5	1.75	0.3	0.7	1.4	1.64
June	1.790	3.813	0.130	5.900	4.245	3.176	3.20	1.3	2.3	1.0	2.98	3.3	2.34
July	0.560	1.465	7.840	5.090	2.20	3.431	1.44	0.8	3.63	6.1	3.17	3.64	3.0
Aug.	4.200	2.075	2.660	2.947	4.55	3.286	1.63	1.825	2.0	4.2	5.0	2.76	2.9
Sept.	2.193	2.760	2.447	5.624	5.92	3.789	4.60	4.875	3.5	2.9	3.47	1.34	3.44
Oct.	7.315	3.467	0.720	1.740	1.274	2.903	2.30	1.875	0.8	4.3	1.35	4.75	2.56
Nov.	2.790	1.860	3.735	4.925	3.29	3.320	3.75	2.6	1.4	2.8	3.87	1.45	2.64
Dec.	1.800	1.455	1.200	3.470	2.925	2.170	0.80	5.0	2.1	1.0	1.1	1.5	1.92
Total	31.558	25.762	29.186	40.526	32.514	31.90	28.82	29.075	24.18	30.6	27.04	26.14	27.50

Meteorological Journal kept at the House of the Royal Society, in the Year 1777. By Order of the President and Council. p. 573.

This register was made twice in every day of the Year, and the mediums for the whole of the months and year are as below.

1777.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Inches.
January . . .	57.0	19.0	36.2	49.0	24.0	37.3	30.25	29.26	29.766	1.039
February ..	59.0	27.0	38.4	53.5	30.0	39.6	30.06	29.08	29.68	1.288
March	72.0	31.0	46.8	65.0	35.5	47.0	30.15	29.29	29.98	1.388
April.	63.0	30.0	47.1	58.5	38.0	47.3	30.35	29.45	29.93	0.787
May	70.5	44.0	56.0	64.0	51.5	54.2	30.20	29.33	29.75	4.602
June	76.0	47.5	60.0	78.0	52.0	61.4	30.42	29.65	30.10	3.754
July	84.0	54.0	63.6	75.5	58.0	65.1	30.45	29.31	29.89	4.697
August	80.0	55.0	65.9	74.0	54.5	67.3	30.45	29.50	30.19	1.075
September	76.5	42.5	60.8	70.0	53.0	62.3	30.35	29.69	30.09	0.498
October . . .	69.0	34.5	53.6	66.0	42.5	55.2	30.25	29.16	30.13	3.578
November	60.0	31.0	46.0	58.0	40.0	46.8	30.38	29.12	29.96	0.874
December	50.0	28.0	37.7	48.0	34.0	39.5	30.55	28.98	29.86	0.801
Whole Year			51.0			53.0			29.943	24.381

The mean variation needle for July was. 22° 12'.

And the Mean dip for the same month was. 72 25.

XXVI. *An Account of the Island of St. Miguel. By Mr. Francis Masson.*
p. 601.

The productions of this island differ greatly from those of Madeira, insomuch that none of the trees of the latter are found here, except the faya: it has a nearer affinity to Europe than Africa. The mountains are covered with the *erica vulgaris*, and an elegant ever-green shrub very like a *phillyrea*, which gives them a most beautiful appearance. It is one of the principal and most fertile of the Azorian islands, lying nearly east to west; its length is about 18 or 20 leagues; its breadth is unequal, not exceeding 5 leagues, and in some places not more than 2. It contains about 80,000 inhabitants. Its capital the city of Ponta del Guda, which contains about 12,000 inhabitants, is situated on the south side of the island, on a fine fertile, plain country, pretty regularly built; the streets straight, and of a good breadth.

About 4 leagues north-east from Villa Franca lies a place called the Furnas, being a round deep valley in the middle of the east part of the island, surrounded with high mountains, which, though steep, may be easily ascended on horseback by two roads. The valley is about 5 or 6 leagues in circuit; the face of the mountains, which are very steep, is entirely covered with beautiful ever-greens, viz. myrtles, laurels, a large species of bilberry, called *uva de serra*, or mountain grapes, &c. and numberless rivulets of the purest water run down their sides. The valley below is well cultivated, producing wheat, Indian corn, flax, &c. The fields are planted round with a beautiful sort of poplars, which grow into pyramidal forms, and by their careless, irregular disposition, together with the multitude of rivulets, which run in all directions through the valley, a number of boiling fountains, throwing up clouds of steam, a fine lake in the south-west part about 2 leagues round, compose a prospect the finest that can be imagined. In the bottom of the valley the roads are smooth and easy, there being no rocks but a fine pulverized pumice stone that the earth is composed of.

There are a number of hot fountains in different parts of the valley, and also on the sides of the mountains: but the most remarkable is that called the *Caldeira*, situated in the eastern part of the valley, on a small eminence by the side of a river, on which is a bason about 30 feet diameter, where the water continually boils with prodigious fury. A few yards distant from it is a cavern in the side of the bank, in which the water boils in a dreadful manner, throwing out a thick, muddy, unctuous water, several yards from its mouth, with a hideous noise. In the middle of the river are several places where the water boils up so hot, that a person cannot dip his finger into it without being scalded; also along its banks are several apertures, out of which the steam rises to a considerable height, so hot that there is no approaching it with one's hand: in other places, a person would think, that a hundred smiths bellows were blowing altogether, and

sulphureous steams issuing out in thousands of places, so that native sulphur is found in every chink, and the ground covered with it like hoar frost; even the bushes near these places are covered with pure brimstone, condensing from the steam that issues out of the ground, which in many places is covered over with a substance like burnt alum. In these small caverns, where the steam issues out, the people often boil their yams (inhames.) Near these boiling fountains are several mineral springs; two, in particular, whose waters have a very strong mineral quality, of an acid taste and bitter to the tongue.

About half a mile to the westward, and close by the river side, are several hot springs, which are used by sick people with great success. Also on the side of a hill, west of St. Ann's church, are many others, with 3 bathing houses, which are most commonly used. These waters are very warm, though not boiling hot; but at the same place issue several streams of cold mineral water, by which they are tempered, according to every one's liking. About a mile south of this place, and over a low ridge of hills, lies a fine lake about 2 leagues in circumference, and very deep, the water thick, and of a greenish colour. At the north end is a plain piece of ground, where the sulphureous steams issue out in many places, attended with a surprising blowing noise. The other springs immediately form a considerable river, called Ribeira Quente, or hot river, which runs a course about 2 or 3 leagues, through a deep rent in the mountains, on each side of which are several places where the smoke issues out. It discharges itself into the sea on the south side, near which are some places where the water boils up at some distance in the sea.

This wonderful place had been little noticed till very lately; so little curiosity had the gentlemen of the island, that scarcely any of them had seen it, till of late some persons afflicted with very virulent disorders, were persuaded to try its waters, and found immediate relief from them. Since that time it has become more and more frequented; several persons who had lost the use of their limbs by the dead palsy have been cured; and also others who were troubled with eruptions on their bodies. A clergyman, who was greatly afflicted with the gout, tried the said waters, and was in a short time perfectly cured, and has had no return of it since. Several old gentlemen, who were quite worn out with the said disorder, were using the waters, and had received great benefit from them; in particular, an old gentleman, about 60 years of age, who had been tormented with that disorder more than 20 years, and often confined to his bed for 6 months together; having used these waters about 3 weeks, had quite recovered the use of his limbs, and walked about in great spirits. A friar also who had been troubled with the said disorder about 12 years, and reduced to a cripple, by using them a short time was quite well, and went a hunting every day. There are several other hot springs in the island, particularly at Ribeira

Grande; but they do not possess the same virtues, at least not in so great a degree.

The east and west parts of the island rise into high mountains; but the middle is low, interspersed with round conic hills, all of which have very recent marks of fire; all the parts below the surface consisting of melted lava lying very hollow. Most of the mountains to the westward have their tops hollowed out like a punch-bowl, and contain water. Near the west end is an immense deep valley, like the Furnas, called the Sete Cidades, or Seven Cities. This valley is surrounded with very abrupt mountains, about 7 or 8 leagues round; in the bottom is a deep lake of water, about 3 leagues in circuit, frequented by great numbers of water fowls. This water has no mineral quality; neither are there any hot springs in the valley. All these mountains are composed of a white crumbly pumice stone, which is so loose, that if a person thrust a stick into the banks, whole waggon loads of it will tumble down. Should any person venture so far as this island for his health, a small stock of the superfluities of life only need be laid in, as the island yields every necessary. The climate is very temperate: the thermometer commonly from 70° to 75° .

XXVII. An Account of a Remarkable Imperfection of Sight. In a Letter from J. Scott to the Rev. Mr. Whisson, of Trin. Coll. Camb. p. 611.

I am very willing to inform you of my inability concerning colours, as far as I am able from my own common observation. It is a family failing: my father has exactly the same impediment: my mother and one of my sisters were perfect in all colours: my other sister and myself alike imperfect: my last-mentioned sister has 2 sons, both imperfect; but she has a daughter who is very perfect: I have a son and daughter, who both know all colours without exception; and so did their mother: my mother's own brother had the like impediment with me, though my mother, as mentioned above, knew all colours very well.

I will now inform you what colours I have the least knowledge of. I do not know any green in the world; a pink colour and a pale blue are alike, I do not know one from the other. A full red and a full green the same, I have often thought them a good match; but yellows, light, dark, and middle, and all degrees of blue, except those very pale, commonly called sky, I know perfectly well, and can discern a deficiency; in any of those colours, to a particular nicety: a full purple and deep blue sometimes baffle me. I married my daughter to a genteel, worthy man, a few years ago; the day before the marriage he came to my house, dressed in a new suit of fine cloth clothes. I was much displeased that he should come (as I supposed) in black: said, he should go back to change his colour. But my daughter said, no, no; the colour is very genteel; that it was my eyes that deceived me. He was a gentleman of the law, in a fine rich

claret-coloured dress, which is as much a black to my eyes as any black that ever was dyed. She has been married several years; no child living, and my son is unmarried; so how this impediment may descend from me is unknown. I have a general good satisfaction in the midst of this my inability; can see objects at a distance when I am on travel with an acquaintance, and can distinguish the size, figure, or space, equal to most, and I believe as quick, colour excepted.

XXVIII. An Account of Baptisms, Marriages, and Burials, during Forty Years, in the Parish of Blandford Forum, Dorset. Communicated by R. Pulteney, M.D., F.R.S. p. 615.

In 1773, there were 446 families, reckoning a workhouse of 44 persons, and three schools containing 92, as 4 families only, which gives nearly $4\frac{3}{4}$ to a family. The whole number of souls in the parish was found to be, males 1174, females 936, total 2110; but it must be remarked, that in this number were not included any of the inhabitants of the close adjoining villages of St. Mary, Blandford, Brianston, or Langton. It appears that $55\frac{1}{4}$ is nearly the average of deaths for 40 years; and the average of the last 10 years 54: hence, taking 55 for the average number, which also was the exact number of burials in the year preceding that of the survey, it follows that about 1 in 38 or 39 dies yearly; and, as it can scarcely be doubted, that the errors of a survey must be on the side of omission, it may not be too much to allow 1 in 39 only.

In the 40 years, from 1732 to 1772, the baptisms, marriages, and burials, were as below:

Total baptisms.	Marriages.	Total burials.
Males. 1133	832	Males. 1099
Females 1042		Females 1132
2175.	In all	2231

The baptisms among the Dissenters are brought into this account only during the last 10 years, males 39, females 54, total 93: if therefore the same proportion is taken for the first three decennial periods, the total of the baptisms will amount to 2454.

XXIX. Part of a Letter from Matthew Guthrie, M.D. of Petersburg, to Dr. Priestley, F.R.S. on the Antiseptic Regimen of the Natives of Russia. p. 622.*

Reading the elegant oration of Sir John Pringle, on the great merit of Captain Cook, for which old Rome would have loaded his ship with civic crowns,

* Dr. Guthrie died at Petersburg, Aug. 7, 1807; where he had long been physician to the Imperial Corps of Noble Cadets, and a Counsellor of State. He was a native of Scotland, and early in life went into the medical service of Russia.

one part of the learned president's discourse drew Dr. G.'s attention in particular, as it regarded this country, and touched on a subject to which he had long paid attention, viz. the antiseptic regimen which nature has dictated to the peasants of Russia. Nothing seems clearer than that, if nature had not taught these people habits, and given them a taste which galloping travellers treat with contempt, they must undoubtedly have sunk under the scurvy, as they are, for the greatest part of the year, exposed to the influence of those predisposing causes to putrid complaints that make the body of the Greenland seaman livid; yet under all these disadvantages such seems to be the efficacy of the regimen they observe, that putrid diseases are strangers to their huts, and the Russian boor enjoys a state of health that astonishes an inhabitant of a country where the dreadful consequences are so well known of bad air within, excessive cold without, joined to a want of fresh vegetables for a length of time.

The Russian boor lives in a wooden house, made with his own hatchet, his only instrument, in the use of which he is most dextrous: it is caulked with moss, so as to be very snug and close. It is furnished with an oven, which answers the triple purpose of heating the house, dressing the victuals, and supporting on its flat top the greasy mattress on which he and his wife lie. From over the oven, which is on one side of the room, are laid some boards reaching to, and supported by, the opposite wall, raised a little above the stove, so as to receive its heated air. On those sleep the children and secondary personages of the hut; for the oven itself is a luxury reserved for the first. Round the room runs a bench with a table in the middle, and in the corner is a sort of cupboard for the reception of saints, before whom small tapers frequently burn, or a lamp with hemp oil. During the long severe winter season, the cold prevents them from airing this habitation, so that you may easily conceive that the air cannot be very pure, considering that 4, 5, or 6 people eat and sleep in one room, and undergo, during the night, a most stewing process from the heat and closeness of their situation; insomuch that they have the appearance of being dipped in water, and raise a steam and smell in the room, not offensive to themselves, but scarcely supportable to the person whom curiosity may lead thither.

Now if it be considered, that this human effluvium must adhere to every thing in the room, especially to the sheep skins or mattress on which they sleep, the moss in the walls, &c. and that the apartment is never ventilated for 6 months at least; at the same time that these people are living occasionally on salt fish or meat, and the whole time without fresh vegetables, exposed likewise when out of doors to a severe cold atmosphere, the scorbutic tendency of which is well known: when all these circumstances are taken into consideration, if it be a fact that they are, in spite of all those predisposing causes, strangers to putrid disease, it will sufficiently justify the first assertion, that the regimen

nature has dictated to these people is most highly antiseptic, and it may be doing service to mankind to describe it minutely. This will probably give pleasure to those gentlemen who have prescribed the new regimen to the British navy with so much success.

The only part of the food of the northern people, that does not come under the description given, is salt meat and fish; the latter they eat during their fasts where fresh fish cannot be procured, at least not on terms that suit their circumstances; and there are also some places where the scarceness of fodder during the winter obliges them to live much on salt meat; yet in all these cases they manage to correct the action of this additional leaven of putridity by mixture with their prepared vegetables, in such a manner as to elude its baneful effects, which furnishes another corroborating proof of the powerful antiseptic qualities of this mode of preparation, which in fact is the main purpose of this paper.

One of their principal articles of food, and what enters into the composition of most of the Russian soups, is their sour cabbage, which you are already so well acquainted with. The 2d capital article is called quass, a liquor which not only serves them for drink, but also as sauce to a number of dishes, especially to such as have a tendency to bring on the disease which their situation threatens, and is the basis of the favourite cold soup of the north, which is made by adding cold meat cut in pieces with cucumbers, or with onions, or garlic, to a bowl of this subacid liquor. This seems to be a good method of qualifying and eating salt meat, to those that are fond of the acid taste, and should make the process in the stomach very different from what we must suppose is the case when salt beef is eaten off a biscuit, accompanied with nothing but what serves for a plate, or the suet pudding of the navy.

To prepare the common Russ quass, they take a large potful of cold water, and put into it as much rye-flour as will make a thin dough: they then place it in an oven, moderately heated, for 3 hours, and then throw it into a tub of cold water: this mixture they work till it froths, with a machine resembling the staff of a chocolate pot, but larger. To this liquor, thus prepared, is added a couple of slop-basons full of the grounds of old quass, or leaven, or, if these are not to be procured, which can hardly happen in Russia, they use as a ferment a piece of their sour bread, and cover the tub with a cloth to keep out the dust,* till the liquor has acquired a sourish taste, which marks its being ready for use. However, this depends on the temperature of the weather, as it acquires the necessary acidity sooner or later, according to the season or degrees of artificial heat that is employed. This liquor the poorest of the people drink as they draw

* Or, rather to keep in the heat,

it from the tub or cask where it is kept for use; but there is a superior kind of quass, which the better sort of people make and bottle for their common use; indeed people of the highest rank love and use it constantly.

To make the better sort of quass, or Keesla Stchee, they take one pood (36 pounds English) of rye flour, or meal, and half that quantity of ground malt, and put them into a tub made for the purpose with a close cover, pouring a kettle full of scalding water, stirring with a stick as they pour, and then cover it close up for an hour; at the expiration of which time they add boiling water in the same manner as before, till it becomes as thin as small-beer. The tub is then placed in a cool situation for some hours, the cover being kept half open with a stick; the liquor is then passed through a sieve into a cask, and 2 basons full of old quass, or the substitutes mentioned in the last receipt, are added, and the vessel placed in a cellar or cool situation for 5 or 6 days, till it acquires the subacid taste, when it is fit for bottling.

Here seems to be an elegant improvement of Dr. Macbride's infusion of malt, for the acidulous taste makes it highly palatable and refreshing; and probably there may be a virtue in this species of acidity, which is perhaps the only thing that the sweet infusion wants, to give it all the antiscorbutic qualities of sour krout, &c. as it also abounds in the antiseptic fluid fixed air, which recommends the other for medical purposes, and particularly as an antiscorbutic; at the same time that the fermentation is permitted to run on till it acquires the acid taste which every one of the efficacious vegetable preparations used in the north is possessed of, and what nearly seems to be the secret alone by which these people preserve them for a length of time, and put them on an equality with fresh vegetables, as one would be led to think by their salutary effects. The very bread that the Russians make use of has also acquired this acidity before it is judged wholesome, and adapted to their constitutions.

The manner of making the Russian rye bread.—In the morning they mix as much rye flour with warm milk, water, and a bason full of grounds of quass, or leaven, as will make a thin dough, and beat it up for half an hour with the chocolate staff before described; this they set in a warm place till night, when they add more meal by degrees, working it up at the same time with the staff, till the dough becomes stiff. They then return it to its warm situation till morning, at which time they throw in a proper quantity of salt, and work it with the hand into a proper consistence for bread, the longer this last operation is continued the better; they then place it before the fire till it rises, when it is cut into loaves, and returned once more into the warm place where it before stood, and kept there for an hour before the last part of the process, the baking, which completes it.*

For sea provision they cut the same sour dough into biscuits or rusk, and dry

them in the oven. This makes a very useful and wholesome article of food, always ready to qualify the seamen's salt provisions, which they commonly eat in the form of broth in the Russian navy, with the addition of this bread, which is put in as we do the white bread in our soups of that name, or they take off the saltiness of their sea beef by making it into soup with their prepared vegetables; but never suffer their sailors to eat it dry as they call it, being of opinion that it promotes the scurvy in the fleet. This rusk also not only answers the common purpose of bread, but when thrown into warm water produces their favourite liquor quass, with or without the addition of ground malt: and they likewise put this last article into the sour dough, with which they make a sort of rusk for the purpose of quass alone. There are prepared cucumbers which are eaten with meat in this country, and the people are remarkably fond of them. They are called salted cucumbers, as salt is the principal ingredient used in the preparation; but they have the same sourish taste so often mentioned, and seem to have their share also in the merit ascribed to the regimen at large.

To prepare the Russian salted cucumbers, they put any quantity of cucumbers into a cask, and as much cold water as covers them, with 4 or 5 handfuls of salt, some oak and black currant leaves, some dill and garlic. They then set the cask into a cool place for about 48 hours, till the liquor tastes sourish, when they pour it off from the cucumbers into a pan, and add to it 4 or 5 handfuls of salt, then boil it for about 15 minutes, and when cold return it into the cask to cover the cucumber, which they now bung up for use, and place in the cellar, where they become crisp and fit to be eaten in 3 or 4 days, and are counted a luxury by their admirers.

There are still a few other dishes to be mentioned, that seem to have the same tendency as those already described: viz. what is called sooins in Scotland, and much used by the common people there. It is an infusion of oat-meal bran in warm water, left to ferment till it acquire the sourish taste, and then strained and boiled to a consistence. Another of their dishes is composed of rye-meal, ground malt, and water, as thick as cream, which is placed all night in the oven, previously heated to a moderate degree, and in the morning a piece of sour rye bread is added to effect their favourite end, and the mess eaten when cold. Horse-radish they dry in the oven and keep all winter, which they powder, when wanted, and mix with vinegar to eat with salt fish. Turnips they preserve during the winter in dry sand (as they likewise do the large white radish;) these they put into an earthen pot with a close cover, and stew them in the oven, with their own juice alone, till perfectly soft, and then eat them with quass. When sugar is added instead of quass, they make an elegant dish, and proper in

* This is the very same process as is used in the north of England, for the like purpose, and probably in all other countries where rye-bread is used.

coughs and pectoral disorders. Oats they prepare and grind in the manner of malt, and make a sort of flummery of this meal, which they eat with quass, their favourite sauce; and sometimes milk supplies its place for these sorts of dishes.

The foregoing is the greatest part of their food and its preparation; being a regimen so consistent and uniformly calculated to ward off the disease that their situation threatens, that the most enlightened physician of our day could not have prescribed a better, and there are some articles in it which, from their cheapness and antiscorbutic qualities, might be permitted to accompany, for trial, their old northern companion sour cabbage.

However, after saying every thing of and for the food made use of by the people inhabiting the northern parts of this extended empire, we must not omit to give the share of merit due to some customs hinted at in the beginning, and which probably have their share in effecting the great end treated of in this letter. These are their clothing, baths, and manner of sleeping. In the first place, they go very warmly clothed when out of doors, though they wear nothing but a shirt and a pair of linen drawers when within; the legs and feet in particular are remarkably guarded against the cold by many plies of coarse flannel, with a pair of boots over all, at the same time that their bodies feel all the warmth of sheep-skin coats, and nothing is left open to the action of the air but the face and neck, which last although never covered, yet coughs and sore throats are seldom heard of.

Their religion happily conspires, with the unavoidable bodily dirtiness attached to their situation, to send them to their vapour baths once or twice a week: here they wash away with aqueous vapour, and afterwards with water in its condensed state, the dirt that by obstructing the pores is so well known to promote putrid diseases, at the same time that they most effectually open the cuticular emunctories, and throw off any obstructed perspiration that might have otherwise acted as a fomes to begin the septic process in the body; and lastly, they undergo nightly a degree of perspiration that enables our coachmen, for example, to sit the whole day and severe winter evening on the box, or at least out of doors, without once dreaming of what we call catching cold, as they throw off every night what may have been retained in the day, and, to use a vulgar phrase, may be said to clear out as they go. But keep them from the nocturnal luxury of their oven, and you kill them in a week.

XXX. Astronomical Observations made in the Austrian Netherlands in the Years 1773, 1774 and 1775. By Nathaniel Pigott, Esq. F. R. S. p. 637.

By a great number of observations, Mr. P. found that the medium of all gave him

50° 53' 3" for the latitude of Louvain, and its longitude 9^m 37^s in time, or 2° 24' 15", east of Paris. Also the longitude of Brussels 8^m 7^s or 2° 1' 45" east of Paris.

XXXI. Observations on the Scurvy. By Charles De Mertans, M.D. Dated Vienna, Jan. 14, 1778. p. 661.

The diseases of a great multitude of people, living in the same manner, and obliged to live on unwholesome food, are to be corrected by a correction of the food itself, and not by any medicines properly so called. Consistently with this principle, Dr. M. always thought that the salt provisions used by sea-faring people, being the principal cause of the scurvy which makes such fatal havoc among crews engaged on long navigations, it was necessary to find out some food of an opposite nature to this, capable likewise of being preserved at sea.

Salt provisions are hard of digestion; and all food, which our powers of digestion cannot reduce to a good chyle, undergoes in the primæ viæ such alterations as are proper to the respective species of it in regard to heat and humidity; consequently, the chyle produced by salt provisions partakes altogether of an animal nature tending to putrefaction. When it mixes with the blood, it increases this disposition which our fluids have of themselves; and thus, by degrees, introduces that slow putrid degeneration which we call scurvy, of which, he was persuaded, there is but one sort, different in its degrees. He was likewise persuaded that the sea and land scurvy are the same disorder, arising from similar causes, that is, living on salt meat or fish, few or no vegetables, damp houses, &c.

To prevent then or correct this alteration in the humours, we must find out some antiseptic aliments, which may keep a great while, and not be subject to be damaged by the change of climate. Dr. M. used to think that sour kroust, or fermented cabbage, so frequently used in Germany, had these qualities; that though it did not always please those who eat it for the first time, every one soon got used to it, and found it good and wholesome food; that sailors in particular were very fond of it, especially when they had no other greens. He had accordingly several conversations on the subject, 12 years ago, with Messieurs Preston and Langley, in which he expressed his wishes that sour kroust might be carried out and made part of the ships provision.

He had for some years past seen, with great pleasure, in the public papers, and the relations of travellers, that the trials he wished for had been crowned with success; and that the preservation of the healths of many crews, which have gone round the world, has been owing to sour kroust. Other methods are likewise in the food, and they consist of vegetables eaten in a state of crudity, and such as the earth affords them. He was convinced, that all the greens

used in our kitchens are much more antiscorbutic when they are raw than after they have been boiled in water,* or have gone through any other preparation by fire. He grounds his opinion on experience, the safest of all guides, and therefore relates the following facts.

He was surprised to find, during an abode of many years at Moscow, that many gentlemen, merchants, and strangers were attacked by a slow scurvy, having their gums soft, swollen, and blueish, the breath strong, and many scorbutic spots on the legs, while it was rare to find among the lower people, either of town or country, a single person with these marks. The nourishment of the former consists of a great deal of meat and fish, both salt and fresh; they seldom eat any greens, except now and then a soup made of sour cabbage, exactly resembling the German sour kroust in every thing, save that this cabbage is chopped small, whereas the sour kroust is cut according to the length of the cabbage. Their common drink is very sour small beer, called quas, besides which they drink wine, the beer of the country, English beer, and a small glass of brandy at least before every meal. They eat very little bread. The common people live all the year on this sour cabbage soup, in which they boil salt meat on common days, and salt or dried fish on meagre days and during their 4 Lents (which are more than a 3d of the year) when they add to it very stinking linseed oil instead of grease or butter. In this soup, which is called schsti, both in the meagre and other seasons, they boil meal, principally that of Saracen wheat. They eat cucumbers like the others in summer, and salt them for the winter. They also feed very much on oaten bread. The common people live in small wooden houses, generally very low, in which they get together both night and day during three parts of the year, on account of the great cold. There is little air in the room, the windows of which are very small. Here they stew together in humidity and nastiness; for, except the bath, which, as well as those mentioned first, they use once a week, they are extremely nasty. Here then are many reasons, all of which (except the constant use of sour cabbage and bread) should make them more subject to the scurvy than the people of fashion, or those who live at their ease; a constant use of salt meat or fish (for they esteem neither so much when they are fresh) much more brandy, filth and damp in their house, less change of clothes or linen. It appeared to him that, exclusive of the daily use of the sour cabbage, which he considers as the most powerful of all preservatives, they were indebted for their safety to the great quantity of raw greens, such as onions, leeks, radishes, turnips, peas in the pod, and others, which they eat. The berries of vaccinium, with others much resembling them, called kloukna, which are of the size of a small cherry and

* Perhaps it is because they lose a great deal of fixed air by ebullition.—Orig.

very acid, are, together with apples, strawberries, and raspberries, almost the only fruits of these countries.

In the Foundling Hospital, of which he was a physician, there were every winter several scorbutic patients. This hospital was built near the conflux of 2 rivers, in a place the soil of which had been raised at a great expence. As near back as the year 1770 there were still stagnated waters to be seen in the place; but only a part of the children lived there, the remainder lived in a stone house, situated on an eminence in the neighbourhood. The usual symptoms of the scurvy on these children were, the swelling of the gums, the nauseous breath, a great languor and dejection; they used to become cachetic, and of a leaden colour. In process of time the swelling of the gums increased; they were used to assume a livid colour: pustules were formed in the mouth, the infection of the breath became most intolerable, the gums and all the inside of the mouth became gangrenous, the jaw bones were carious, the fall of the teeth followed, and the bones of the alveoli fell to pieces. The sick could scarce stir, though they had as yet no fever, and had a very good appetite. The legs of some among them were from the first covered with scorbutic spots, and dried crusts, like scales; others only had these symptoms after the mischief had made a great progress. Most of them had their legs swelled. In some, the flexor tendons of the legs became shorter, and stiffened in such a manner that they were forced to keep always in a lying posture, with the legs near the thighs. In 2 cases the same thing had happened to the arms.

The gangrene of the gums and mouth, as well as the caries of the bones, used insensibly to increase to such a degree, that the bones of the alveoli and the spongy part of those of the upper jaw, used to fall out. The mischief however usually made a slow progress; there often elapsed a fortnight, and sometimes more, after the beginning of the gangrene of the mouth, and caries of the bones; and many months between the first symptoms and the stage of the disorder above described. Even in this stage, dreadful as it was, they still took nourishment sufficient, and even much more than it would be thought possible they should have taken in such a situation. It was impossible however they should live long in such a state, and death soon put an end to their torments.

The mode of treatment which he commonly made use of in curing the greater part, provided the mischief had not made a considerable progress in the spongy bones of the upper jaw, was this: the first thing he did was to put them on a vegetable diet, and order them soups, with a great many greens dressed in light broth, such as sour cabbage, carrots, turnips, and onions, &c. to which he added stewed onions and sorrel. The drink of the larger sort was quas or sour small beer; the lesser ones (none of which were ever seized with the scurvy

under 2 years old) drank water. During the spring, those who had the scurvy took, in proportions suitable to their ages, a drink made of whey, in which were infused antiscorbutic plants, such as cochlearia, nasturtium aquaticum, becca bunga, acetosa. This infusion was sweetened with plain syrup, or syrup of sugar. Besides this, in the course of the day, they used a gargle, made of an infusion of herbs, rue, sage, agrimonia in water, to which was added spirit of cochlearia, and honey of roses. When the gangrene began to shew itself at the mouth, besides the remedies abovementioned, they used to take a strong decoction of bark, part of which decoction was also added to the gargle. The gangrened parts were also touched with honey of roses, mixed with a small quantity of spirit of sea salt. This method of treatment had succeeded perfectly well the first 3 years; so that the greater part of the sick, as well adults as infants, were commonly cured in 3 weeks or a month, when the distemper was not far advanced. It was in spring and winter that the scurvy used to be most fatal.

In autumn 1770, the foundling children, who remained in town to the number of 1000,* were lodged, contrary to his advice, in the wing of the house finished but about a year before. In a climate where the summer is so short, new walls made of bricks take a great time in drying, and this house was situated on a soil which had been a bog a few years before. Notwithstanding all the precautions that could be taken, a damp was felt in the room the whole winter. The scurvy showed itself early in the spring, and he had many more children ill than in the preceding seasons. The violent symptoms were also much more frequent. Many had gangrenous pustules at the mouth, the jaw bones were carious in some; the limbs, particularly the legs of many, were drawn up and stiff. He put all these sick persons in the wooden house, which had already served many years as an hospital for the scurvy, and gave them the food and medicines above-mentioned; but the disorder was more stubborn than ever it had been, and all he could do could hardly keep it down. In the middle of May, seeing that the remedies he had formerly tried were unsuccessful, he began to think of other methods. The reflections communicated above, which he had made on the diet of the lower people, determined him to give his patients those vegetables raw which they had before been used to eat boiled. In consequence he ordered them, every morning, radishes, sweet turnips, carrots, and young onions, which they eat like apples. At dinner, besides the soup and greens as usual, they eat sallad with a little vinegar and a very little oil; in the afternoon the same roots as in the morning, and at night, greens and sallad. The remedies were continued as before. In a few days all the bad symptoms decreased: those who were at the worst, and had been ill for some time, began to get better; those who had been but slightly

* The greater part of the sucking children were at nurse in the country.—Orig.

seized were soon well; so that at about a month's end there only remained a few of those who had been the worst, and they too were getting well at a great pace. This change for the better was apparent in all a very few days after they had begun to eat the raw vegetables. He had not at that time read the observations of the English physicians and surgeons on malt, or he should certainly have made use of it. Quas comes near it, with this difference only, that it is not drunk in a state of fermentation. It is a species of sour small beer, to which, instead of hops, they add the wild mint.

The same method of treatment was attended with success in 1772 and 1773; in both which springs he had scorbutic patients with the same symptoms, but not in such numbers as in 1771, when there were near 60, because the house, having now dried, was become very wholesome, and because the soil had been again considerably raised.

He would not propose carrying out vegetables on a voyage for the whole crew, because that, in order to preserve them, they must be kept in dry sand, which (if not altogether impracticable) would be extremely difficult in such large quantities, not to add that even then a great part would be spoiled: but might it not be possible to provide a certain quantity of carrots, turnips, &c. and stow them in sand, in a part of the ship where they might not be exposed to get damp or wet, whence they might be given in such cases as the sour kroust alone would be found insufficient to cure? for he thinks that these, joined to an infusion of malt, would soon get the better of the disorders. But if this cannot be so well done at sea, it is obvious, that the cure of the scorbutic persons will be much accelerated, if raw vegetables are given them as soon as they come on shore; a mode which will have the additional advantage of shortening the stations ships are obliged to make, for recruiting their sick. Nature will of herself dispose the sick to make use of this remedy, especially as the stomach is never affected by it.

In Austria, as well as several other parts of Germany, the people eat sour turnips, which are prepared in the same manner as the sour kroust; that is, after having been chopped thin, salt is put to them, and they are left to ferment. They are put in tubs, and keep from one year to another. He proposes this vegetable as a valuable addition to the antiscorbutic regimen of seafaring people. It has nearly the same taste as sour kroust, and will probably be found to have the same virtues: and if so, though it should have no other advantage, it will at least vary the diet, which is itself no inconsiderable advantage on a long voyage.

XXXII. Comparison between Sir George Shuckburgh's and Colonel Roy's Rules for the Measurement of Heights with the Barometer; in a Letter to Col. Roy, F. R. S., from Sir George Shuckburgh, Bart., F. R. S. p. 681.

Since the printing of your ingenious memoir on the subject of measuring heights with the barometer, I have been naturally led to a comparison of your

rules and observations with my own; and am not more pleased than surprised at the general correspondency of our results, which carries with it the appearance of one and the same series of experiments, rather than of distinct observations made with different instruments, in different countries, and by different persons. That the standard temperature of zero on the scale of the thermometer should be found by each of us to fall in the same point to within $\frac{1}{3}$ of a degree is, I think, truly surprising; and I doubt not will evince to Mr. de Luc the strong probability there is of the necessity of correcting his rules. But though in this essential and fundamental part of the inquiry we agree, there are however some little circumstances in which we differ; and it is the subject of this letter, sir, to point out to you the degree of our differences. The 2 chief causes of our differences are, the expansion of quicksilver and the expansion of air. I shall begin with the equation for quicksilver. The mean temperature of ordinary barometrical observations will generally be found to lie between 40° and 70° on Fahrenheit's thermometer: now the mean expansion in this range, according to your observation, is ,0323 inch on a column of 30 inches for 10° of heat; by my table it is only ,0304 inch: the difference ,0019 inch is equal to about 20 inches in the result of the height, when the temperature of the 2 barometers differs 10° , and this may reasonably be expected only in a height of 3000 or 4000 feet. In an observation on Mount Etna, one of the greatest accessible heights in Europe, the difference of temperature at the top and bottom might amount to 30° , and this would occasion a difference of about 5 feet; which may be reckoned inconsiderable in a height of 11,000 feet. In fact, in an observation on this mountain by Mr. Dessaussure it amounted to only $3\frac{1}{2}$ feet. I may add, that your equation makes the computed height less than mine.

I proceed to the expansion of air. Your equation is various according to the circumstances, the difference therefore of our results will, according to the circumstances, be various. The following table will give the quantity of this difference, viz. it shows how much your result is + or - mine on 1000 feet, according to different pressures of the atmosphere and different temperatures. The first column to the left hand contains the mean heat of the column of air, between the 2 barometers; the figures in the horizontal line at top are the mean height of the 2 barometers, or mean pressure of the atmosphere; the common point of meeting in the different columns gives the difference of our result in feet, according to the respective circumstances.

Mean heat.	Mean height of the two barometers in inches.										
0	30	29	28	27	26	25	24	23	22	21	20
32	0	0	0	0	0	0	0	0	0	0	0
42	+1	0	-1	-2	-3	-4	-5	-6	-7	-8	-9
52	+2	0	-2	-4	-6	-8	-10	-12	-14	-16	-17
62	+5	+2	-1	-4	-7	-10	-13	-16	-19	-22	-25
72	+8	+4	0	-4	-10	-12	-17	-21	-25	-29	-33

Thus, if the mean height of the barometer were 27 inches, and the temperature 52° , the difference of the results would be 4 feet in a thousand; how far, therefore, this is of moment is left to the judgment of the observer. I conclude lastly, sir, with a comparison of your actual observations made in Great Britain computed after my tables, deduced from a series of observations made in Savoy. I have indeed only collected 16 of your observations; but as I have chosen such as presented themselves as most proper, either on account of their heights or temperatures, I imagine these will be very satisfactory. From these it seems, that the error of my tables, from a mean of all these observations, amounts to $+\frac{2.0}{10000}$; of yours, to $+\frac{1.4}{10000}$; but it must be remarked, that the standard temperature, from which I compute, is $31^{\circ}.24$ of Fahrenheit, whereas in your computations it is assumed at $32^{\circ}.0$; this difference of $0^{\circ}.76$ is equal to $\frac{1.8}{10000}$ in the correction for the expansion of the air: if then we were to set out from the same zero, viz. 32° , which I have proposed in my memoir, the error of my tables, according to your observations, would become only $\frac{2.0}{10000}$, that of yours remaining $\frac{1.4}{10000}$. I would by no means from hence conclude, that any preference is to be given to the former, but would say, that in most practical observations, in these countries at least, it is indifferent which method is used. These same comparisons also afford us another piece of information, viz. that under similar conditions the density of the atmosphere is the same, whether under the parallel of 46 or 56 degrees of latitude. Till, therefore, more accurate observations than those of Mr. Bouguer can be obtained in the neighbourhood of the equator, I should be extremely cautious how I admitted a latitudinal equation; nor do I think the single observation, related in Lord Mulgrave's Voyage towards the North Pole, of sufficient authority itself to establish such a theory on.

I shall now beg leave to conclude with what I flatter myself will not here appear improper, a new rule for reducing the observations, and which I hope will be found particularly commodious, as it requires no logarithms, nor any other than the following short table, which may be engraven on the scale of a thermometer, and therefore, always accompanying the instrument, will serve for computing the observations on the spot, if the height should not exceed 4000 or 5000 feet, which will frequently be found very satisfactory.

Ther.		
32°	85.86 ft.	The value of $\frac{1}{10}$ of an inch of quicksilver on the barometer, expressed in feet in the atmosphere when the barometer stands at 30 inc. according to the different temperatures.
35	87.49	
40	88.54	
45	89.60	
50	90.66	
55	91.72	
60	92.77	
65	93.82	
70	94.88	
75	95.93	
80	96.99	

The adjoined table gives the value of $\frac{1}{10}$ of an inch on the barometer in feet in the atmosphere, when the quicksilver stands at 30 inches, for every 5 degrees of temperature from 32° to 80° ; and for any other height of the barometer it will be in the inverse ratio of that height to 30 inches. Thus, let A be the mean height of the two barometers in inches; x the difference of

the two barometers in 10ths of an inch; β the number taken out of the adjoined table; x the height in feet: we have then the following expression, $\frac{30\alpha\beta}{A} = x$, the height required.

XXXIII. An Account of the Calculations made from the Survey and Measures taken at Schihallien, in order to ascertain the Mean Density of the Earth. By Charles Hutton, Esq., F. R. S. p. 689.

The survey from which these calculations have been made, was taken at and about the hill Schihallien in Perthshire, in the years 1774, 1775, and 1776, by the direction, and partly under the inspection, of the Rev. Nevil Maskelyne, D. D., F. R. S., and astronomer royal, by whom the manner of making the survey has already been fully explained in the Philos. Trans. for 1775. I have therefore only to give an account of the measures of the lines and angles, and of the calculations which I have raised from them with all possible care and faithfulness, for the purpose of determining the measure of the ratio of the mean density of the earth to that of water or any other known matter. These calculations were naturally and unavoidably long and tedious; and the more so as the business was in a manner quite new, which laid me under the necessity of inventing and describing such modes of computation as should be proper to be applied in so important and delicate a business. Having, at length, with close and unwearied application for a considerable time, completed all the calculations; I have, in the following sheets, drawn up an account of those operations, with the results arising from them; and have accompanied them with such drawings as are necessary to illustrate the descriptions. I have also inserted a synopsis of the measures which were taken of the lines and angles; from which any person may at any time satisfy himself of the truth of the computations that have been made, and are here described. These measures I have here immediately subjoined, before proceeding to describe the computations made from them.*

First is given a synopsis of the horizontal and vertical angles that were observed at the principal points in making the survey about Schihallien.—These angles are very numerous, and occupy many pages.

Then are particularly described the two principal bases which were accurately measured, as foundations on which every thing else must depend: these were one on the south side of the hill, and the other on the north. And first of the

* This very curious and important paper, containing a full account of all the measures and calculations, here drawn up by Dr. H. at the desire of the R. S., concerning a problem of the very first consequence in physics, viz. to determine the mean density and mass of the whole globe of the earth, being necessarily very long, extending through full 100 pages of the original, we must content ourselves with an abstract only of the principles, and the novel methods of calculation, with the chief results of the whole.

base in Glenmore, the valley on the south of Schihallien; in which, after adding together all the repetitions of the 20 foot measuring rods, with the numerous small intervals between them, and reducing the whole on account of the difference of temperature and the gradual wearing of the brass standard, there results 3011.417 feet for the corrected measure of this base, or the true length of the line called RB in the survey.

The 2d base, in the vale of Rannoch, on the north side of mount Schihallien, was likewise measured in the same manner, and when reduced on all the above mentioned accounts, comes out finally 5895.399 feet, for the correct measure of that base, called $\alpha\gamma$. Besides these two principal bases, several other shorter lines were also measured, the better to connect together those bases and the several parts of the mountain.

Having now obtained, to a great degree of accuracy, the measured lengths of two lines which were to serve as bases for all the future calculations, the next consideration was how to make the properest use of them. Every other line or distance, drawn or conceived to be drawn, must be calculated from them by the help of the angles observed, either at their extremities, or at all the other points and stations in the survey and plan. As these two bases are situated in the low parts of the country, from which but a very few of the other principal stations are visible, one method evidently is to compute immediately from these bases such of the great lines in the survey whose extremities are visible from them; and then from these calculated lines to compute others next to them, and so on quite around and within the whole figure. In this manner several values of each line will arise, both from the double computations by the two measured bases, and from the various sets of triangles which can be formed from the numerous horizontal angles which were observed at the several stations. But in this mode of computation, after great labour and pains, Dr. H. had frequently the mortification to find that the several values of the same lines would differ so greatly one from another, that it was often very doubtful whether he could rely on any of them, or even on the mean among them all. These differences arose from the small errors in the observed angles, which in some degree are unavoidable; and indeed they were so small, that the sum of the angles of the several triangles, which were used in the calculation seldom differed by more than a minute or 2 from 180° . But in a long connected chain of triangles, dependent on each other, the effects of such small errors at length become too great to be tolerated in a computation requiring much accuracy. Another method is, first to compute from both bases the length of the line KN extended along the ridge of the hill from east to west, and from it, as a secondary base, compute all the other lines in the plan. This method admits of much more accuracy than the former, supposing this secondary base to be truly assigned; because that, from the elevated

and central situation of this line, all or most of the other points in the survey are visible from one or both of its extremities, by which it happens that the other lines are mostly determinable from it alone, without so close a connection with each other as in the other method of computation. By both of these methods then, and by all the triangles furnished by each of them, he computed all the principal lines in the plan, and either took a mean among the several values of each, or else selected out of them such as from various circumstances he judged it safest to rely on, as nearest the truth. The trigonometrical computations were always accurately made by the common numbers, and generally repeated by logarithms, and the result of every proportion determined to 2 or 3 places of decimals.

The mean among a great number of ways of computation from the south base, gives the horizontal distance of the secondary base from κ to $\nu = 4052.2$, and the mean of all the results from the north base $\alpha\gamma$ gives $\kappa\nu = 4058.9$, and the mean between these two gives 4055.5 for the mean distance of κ and ν . And this value of $\kappa\nu$ was used in computing most of the other lines in the survey, which were very numerous, and were then set down in a table.

From the first 3 lines, or bases, and the horizontal angles observed at the several stations, a very large and accurate plan of the whole survey was constructed, forming a map of 4 feet long by 4 feet broad, which was verified in every part by the measures of the computed lines, both those above-mentioned and others, and they were generally found to agree very exactly, according to the scale by which the plan was constructed. The use of this large map was to receive and admit of the distinct and accurate exhibition of the figures in their true places, expressing the number of feet in elevation or depression with respect to each observatory of every point and section of the ground whose elevation or depression might be observed. But before proceeding to the computation and construction of the points in the sections, Dr. H. here abstracts the numbers which express the relative elevation of the principal original points in the survey, being the extremes of the lines whose lengths are above abstracted. These few numbers are the results of the calculation of several hundreds of triangles conceived in a vertical position, their bases being either the horizontal lines above-mentioned, or other lines drawn as diagonals between many distant points in the survey, according to the number of vertical angles which had been observed; and of these bases, whether real or imaginary, each generally afforded 2 vertical triangles, as the angles of elevation and depression were taken alternately at both ends of the lines. It is scarcely necessary to remark, that all these triangles are right-angled, the common base being one of the sides about the right angle, and the other the difference in altitude between the two given points or extremes of the base; and this difference in altitude is found from the application of this

proportion, as radius is to the tangent of the angle of elevation or depression, so is the given base to the altitudinal difference between the two given points, exclusive of the height of the theodolite or other instrument, which was afterwards allowed for. From the resolution of all these triangles, and taking the means of the many corresponding results, were obtained the numbers, which show how many feet the points denoted by the letters standing against them are below the level of the point *N* or the western cairn on the ridge of the hill. They are all referred to this point *N* at the western extremity of the ridge of the hill, because it is the most elevated point in the whole survey.

These depressions, and those of several other principal points, were first carefully computed by means of various different bases, as so many places whence the sections were to commence.

These sections are very numerous, made in all directions from the primitive points before-mentioned, and many of them extended to great distances, indeed far beyond the bounds of the plan, so as to include the nearest hills and valleys of the surrounding country. They are mostly made in vertical planes in the manner described in the article of the Philos. Trans. before referred to, excepting some few, which are level sections in planes parallel to the horizon, and some indeed irregular, being neither vertical nor horizontal. To compute the relative altitude of each point in these sections, it is evident, requires the resolution of 2 different triangles, viz. a horizontal triangle by which its place in the plan is ascertained, and a vertical triangle of which one side is the elevation or depression of the point. Of these sections there are above 70, containing near 1000 points, whose places in the plan and relative altitudes have been computed; so that the number of triangles, whose numeral resolutions have been performed in the course of this business, amounts to several thousands. Before the abstract of the computation of the sections, is set down at large the calculation of one of them, to show the manner in which they have been computed in the readiest and easiest way. After which is given a large table of the results of all these calculations, comprising 72 sections, and some thousand points in the sections, whose altitudes or depressions are arranged in order, and illustrated by drawings of appropriate figures.

Having finished the computation of the relative altitudes of all the points in the sections, the next consideration is how they are to be applied in determining the attraction of the hill. In whatever manner this last mentioned operation may be performed, it is evident, that all the points and sections with their altitudes must be entered in the plan. Therefore, having accurately constructed a large plan of the ground, as before mentioned, containing all the principal lines or bases, at the extremities of which either vertical or horizontal angles were taken, from them are then determined in this plan the places of all the other

points in the sections, whether vertical, horizontal, or irregular. These places or points were determined by drawing lines from each extremity of the base so as to form with it angles equal to those which were observed on the ground for each corresponding pole; the intersections of these lines are the places of the poles, which having marked with a fine dot or point of ink, and written close to each point the proper number expressing its relative altitude, and cleaned the paper by rubbing out the lines forming the angles by which the points were determined, there remained only the points with the figures expressing their altitudes distinctly exhibited in the plan.

It remains now to apply all the foregoing calculations and constructions to the determination of the effect of the attraction in the direction of the meridian. And here it soon occurred, that the best method was to divide the plan into a great number of small parts, which may be considered as the bases of as many vertical columns or pillars of matter, into which the hill and the adjacent ground may be supposed to be divided by vertical planes, forming an imaginary group of vertical columns, something like a set of basaltine pillars, or like the cells in a piece of honey-comb; then to compute the attraction of each pillar separately in the direction of the meridian; and lastly, to take the sum of all these computed effects for the whole attraction of the matter in the hill, &c. Now the attraction of any one of these pillars on a body in a given place may be easily determined, and that in any direction, to a sufficient degree of accuracy, because of the smallness and given position of the base; for, on account of its smallness, all the matter in the pillar may be supposed to be collected into its axis or vertical line erected on the middle of the base, the length of which axis, as the mean altitude of the pillar, is to be estimated from the altitudes of the points in the plan which fall within and near the base of the pillar: then, having given the altitude of this axis, with the position of its base, and the matter supposed to be collected into it, a theorem can easily be given by which the effect of its attraction may be computed. But to retain the proper degree of accuracy in this computation, it is evident that the plan must be divided into a great number of parts, perhaps not less than 1000 for each observatory, in order that they may be sufficiently small, and by this means forming about 2000 of such pillars of matter, whose attractions must be separately computed, as mentioned above. The labour and time necessary for such computation, it is evident, would be very great, perhaps not less than those employed in all the preceding computations of the sections, and all the other points and lines concerned in this business. For this reason it was desirable to obtain a theorem or method by which the attractions of the small and numerous pillars might be computed with the same degree of accuracy, but with less expence of labour and time than when computed separately as above-mentioned. And in this inquiry the success has been equal to

his wishes, having at length met with a method by which the business has been effected in perhaps $\frac{1}{4}$ or $\frac{1}{5}$ of the time that would have been required in the other way.

Of all the methods of dividing the plan into a great number of small parts, he found that to be the most convenient for the computation, in which it is first divided into a number of rings by concentric circles, and these again divided into a sufficient number of parts by radii drawn from the common centre, that centre being the observatory where the plummet is placed on which the effect of attraction is to be computed. By this means the plan is divided into a great number of small quadrilateral spaces, 2 of the opposite sides of which are small portions of adjacent circles, and the other 2 are the intercepted small parts of 2 adjacent radii, as appears by fig. 11, pl. 4; in which, for the present, let the circles and their radii be supposed to be drawn at any distances whatever from each other, till it shall appear from the theorem to be investigated what may be the properest distances and positions of those lines. In this figure A is the observatory, AN the meridian, WAE an east-and-west line, BCDE one of the little spaces, and F its centre or the foot of the axis of the pillar whose base is BCDE; the figure AWNEA being a horizontal or level section through the point A. Join A, F, and with the centre A describe the mid circle GFH. Let a denote the length of the axis on the point F, or the mean height of the pillar on the base BD; and s the sine of the angle of elevation of that pillar as observed at A, to the radius I, or $s = \frac{a}{\sqrt{(a^2 + AI^2)}}$. Then will the magnitude of that column, or its quantity of matter, be expressed by $(BC + ED) \times \frac{1}{2}BE \times a$, which is supposed to be all collected into the axis; consequently, if the attraction of each particle of matter be in the reciprocal duplicate ratio of its distance, the attraction of the matter in the pillar, so placed, on the plummet at A, in the direction of the meridian AN, will be $\frac{BC + ED}{2AF} \times BE \times a \times \frac{s}{a} \times c = \frac{BC + ED}{2AF} \times BE \times sc = \frac{GH}{AF} \times BE \times sc$ nearly, supposing F to be equally distant from BC and ED, and c the cosine of the angle FAN to the radius I.

But $\frac{GH}{AF} \times c$ is nearly equal to d the difference of the sines of the angles BAN, CAN, as is thus demonstrated. Draw GK, FL, HM, perpendicular, and GP parallel to AW; and draw the chord GH. Then AK, AM are the sines of the angles GAN, HAN, to the radius AF, their difference being $KM = GP$; also FL is the cosine of FAN to the same radius: consequently $GP : FL = d : c$. But the triangles LFA, PGH are equiangular, and therefore $GP : FL = GH : AF$. Consequently $GH : AF = d : c$; or $\frac{GH}{AF} \times c = d$. This equation is accurately true when GH is the chord of the arc; and as the small arc differs insensibly from its chord, the same equation is sufficiently near the truth when GH is the arc itself. Substituting now d

instead of the quantity $\frac{GH}{AF} \times c$ in the theorem above, it will become $BE \times ds$ for the measure of the attraction of the pillar whose base is BD in the direction AN . Which is as easy and simple an expression for the attraction of a single pillar as can well be desired or expected.

But to make the application of this theorem still more easy to the great number of small pillars concerned in this business, let us suppose BE and d to be constant or invariable quantities, and then it is evident that we shall have nothing more to do but to collect all the s 's or sines of elevation of all the pillars into one sum, and then multiply that sum by the constant quantity $BE \times d$, by which there will be produced the measure of the attraction of all the pillars, or of the whole part of the ground on one side of WE . Now BE will be made to become constant, by making the circles equi-distant from each other, or by taking the radii in arithmetical progression. And d will be constant, by drawing the radii so as to form with AN angles whose sines shall be in arithmetical progression; for then d is the common difference of the sines of those angles. Hence then we are easily led to the best manner of dividing the plan into the small spaces, viz. from the centre A describe a sufficient number of concentric and equidistant circles; divide the radius AI of any one of them into a sufficient number of equal parts, and from the points of division erect perpendiculars to meet the circle; then through the points of intersection draw radii, and they will divide the circles in the manner required.

In a computation of this kind, we need only calculate the attraction of the matter above the plane or horizon of each observatory, and the attraction of so much matter as is wanting to fill up the vacuity below that plane lying between it and the surface of the lower part of the hill. For the south observatory, the attraction of the southern parts that are above it must be subtracted from that of the northern parts, to obtain the attraction of the whole towards the north; that is, the southern elevations are negative, and the northern ones affirmative. The contrary names take place with respect to the depressions, or the vacuities below the plane of the observatory; for if the whole space below this horizontal plane were full of matter to an equal extent both ways, its attraction need not be computed, as those on the contrary sides would mutually balance each other; but since there are unequal vacuities on each side, it is evident, that the attraction of the matter that might be contained in them must be deducted from the other two equal quantities, to leave the real attraction of those two sides; then subtracting the remainder on the south side from that of the northern side, there will at last remain the joint effect of all the matter below the plane in the northern direction: but as the one remainder is to be subtracted from the other, the two equal quantities may be omitted in both, and only the effects of the vacuities brought into the account, which being twice subtracted, their signs be-

come contrary to those of the parts above the horizontal plane; that is, the effect of the southern vacuity is affirmative, and that of the northern one negative. But for the northern observatory, when the attraction towards the south is to be found, the contrary names take place; that is, in the elevations the southern parts are affirmative, and the northern parts negative; but in the vacuities or depressions, the northern parts are affirmative, and the southern ones negative.

According to the foregoing method the plan of the ground was divided into 20 rings, by equidistant concentric circles, described about each observatory as a centre; and each quadrant was divided into 12 parts or sectors, by lines forming, with the meridian, angles whose sines are in arithmetical progression; by which means the space in each quadrant was divided into 240 small parts, making almost 1000 of such parts in the whole round for each observatory, or near 2000 for the two observatories. This was judged to be a sufficiently great number of parts to afford a very considerable degree of accuracy; or at least that number was as great, and the parts as small, as was well consistent with the degree of accuracy afforded by the number of points whose relative altitudes had been determined.

In this division the common breadth of the rings, or the common difference of the radii, is $666\frac{2}{3}$ feet; and the common difference of the sines of the angles formed by the radii and the meridian, is $\frac{1}{12}$ of the radius; and consequently those angles are expressed in degrees and minutes as here follows, viz. $4^{\circ} 47'$, $9^{\circ} 36'$, $14^{\circ} 29'$, $19^{\circ} 28'$, $24^{\circ} 37'$, $30^{\circ} 0'$, $35^{\circ} 41'$, $41^{\circ} 48'\frac{1}{2}$, $48^{\circ} 35'$, $56^{\circ} 26'\frac{1}{2}$, $66^{\circ} 26'\frac{1}{2}$, $90^{\circ} 0'$.

Of this kind were made two large plans, one divided for each observatory, from which were estimated the mean altitudes of the pillars erected on the spaces into which they are divided. These altitudes are easily estimated when several of the points fall near and in the small spaces or bases, especially when they are near the middle of them; but, numerous as the points are, there were many bases in which none at all were contained, nor even near them. This circumstance at first gave much trouble and dissatisfaction, till he fell upon the following method by which the defect was in a great measure supplied, and by which he was enabled to proceed in the estimation of the altitudes both with much expedition and a considerable degree of accuracy. This method was the connecting together by a faint line all the points which were of the same relative altitude: by so doing, he obtained a great number of irregular polygons lying within, and at some distance from each other, and bearing a considerable degree of resemblance to each other: these polygons were the figures of so many level or horizontal sections of the hills, the relative altitudes of all the parts of them being known; and as every base or little space had several of them passing through

it, he was thus enabled to determine the altitude belonging to each space with much ease and accuracy. In this estimation he could generally be pretty sure of the altitude to within 10 feet, and often within 5, which on an average might be about the 100th part of the whole altitude; and when we consider that the number of such estimated altitudes is very great, and that it is probable the small errors among them would nearly balance each other, the defect of those that might be reckoned too little being compensated by the excess in those which might be taken too great, we need not hesitate to pronounce, that the error arising from the estimation of the altitudes is probably still much less than that part.

It was necessary to determine these altitudes of the pillars, in order to compute the sines of the angles of elevation subtended by them, as the theorem requires the use of these sines; and the very easy method used in deducing the latter from the former is explained after registering the altitudes of all the pillars as they were computed. This register consists of 16 tables, viz. 4 quadrants of spaces in the altitudes, and 4 in the depressions, for each observatory, as specified in the titles of them. The numbers are feet, like all the other dimensions. The numbers on the same horizontal line from left to right are such as are all in the same ring; and those in one and the same vertical column are in the same sector, or between the same two radii; the number of the ring, counted from the common centre, is written in the left-hand margin; and the number of the vertical column, or distance of the space or sector from the meridian, at the top; also the radius of each ring, that is, the line from the common centre to the middle of the ring, is written on the same line with it, in the right-hand margin. It may be further remarked, that in such little spaces as were cut through by the boundary line between elevations and depressions, thus making but a part of such spaces in each of those denominations, each space was accounted as a whole one; but then the mean altitude or depression in each part was diminished in the proportion of the whole space to the part of it so included in the boundary. The altitudes and depressions are set down first with respect to the southern observatory o, and then for the northern observatory p; and in each, the altitudes are placed first.

After these 16 long tables, Dr. H. adds, it remains now to find the sines of the vertical angles subtended by all the foregoing altitudes and depressions, since the sum of these sines is what we are in quest of. Each altitude or depression is the perpendicular of a right-angled triangle, of which the given radius standing on the same line with it in the right-hand margin is the base, or other side about the right angle; and by the resolution of the right angled triangle, for each perpendicular, the same number of corresponding sines will be found. But with such data the tangent of the angle is much easier to be found than the

sine, and the analogy for that purpose is this, as the base : to the perpendicular :: 1 (radius) : the tangent required, which will therefore be found by barely dividing the given perpendicular by the base ; and if we find this number in its proper column in a table of sines and tangents, on the same line with it, in the column of sines, will be found the sine of the angle required. This seems to be the easiest way of resolving all the triangles when computed separately. But as the labour would be very great in performing so many hundreds of arithmetical divisions, &c. either by logarithms, or by the natural numbers, instead of it, the following method, proposed by the Hon. Mr. Cavendish, was adopted, being a much more expeditious way of obtaining the sum of the sines required. This method consists in finding, in a very easy manner, the difference between each tangent and its corresponding sine, from the given base and perpendicular, and then, subtracting the sum of all the differences from the sum of the tangents, there remains the sum of the sines. Several advantages attend this method of proceeding : for, to find the tangents we need not divide every perpendicular separately by its corresponding base, but add together all the perpendiculars that are on the same line, and divide their sum by their common base, which is the radius of the middle of the ring, and is placed on the same line with them towards the right hand ; for thus we shall have little more than a 12th part of the number of divisions to perform : also a great part of the tangents are so small that they do not at all differ from their corresponding sines, in the number of decimals that it is necessary to continue the computations to, in all which cases the trouble of finding the difference is saved ; and those differences which it is necessary to compute, are very readily found by inspection on a peculiar kind of sliding rule, which was constructed for this purpose. By which rule were computed all the differences which were necessary to be found, and placed in their proper squares formed by the meeting of the horizontal and vertical lines, or rings and sectoral spaces, in a following set of 16 tables, which correspond to the foregoing set of 16, each to each, according to the number of them.

After this follow the 12 columns of differences before mentioned, which are succeeded by one or more columns containing the sums of each line of these differences, which sums being added together, their total is placed at the bottom of them ; and this total is the sum of all the differences between the sines and the tangents, and it is therefore subtracted from the total of the tangents in the 4th column, when there remains the sum of the sines as required.

Having now obtained the sums of the sines for the several quadrants, the next business is to collect them together, and deduct the negatives from the affirmatives. And this may be done either for each observatory separately, or

for both together. It is here done separately, in order thence to discover also the ratio of their effects.

And first for the southern observatory o.				Secondly, for the northern observatory p.			
Affirmatives.		Negatives.		Affirmatives.		Negatives.	
1.. 24.795 N. W. } Alt.		3.. 2.374 S. W. } Alt.		11.. 25.078 S. W. } Alt.		9.. 0.019 N. W. } Alt.	
2.. 19.792 N. E. } Alt.		4.. 0.375 S. E. } Alt.		12.. 20.261 S. E. } Alt.		10.. 0.090 N. E. } Alt.	
7.. 24.806 S. W. } Dep.		5.. 13.534 N. W. } Dep.		13.. 25.637 N. W. } Dep.		15.. 2.774 S. W. } Dep.	
8.. 29.213 S. E. } Dep.		6.. 12.356 N. E. } Dep.		14.. 26.161 N. E. } Dep.		16.. 5.610 S. E. } Dep.	
98.606 = sum of affirm.		28.639 sum.		97.137 = sum of affirm.		8.493.	
28.639 = sum of negat.				8.493 = sum of negat.			
69.967 = effective sum of the sines for o.				88.644 = effective sum of the sines for p.			
				69.967 = the same for o.			
				158.611 = the sum of the sines for both obs.			

From these numbers it appears, that the effect of the attraction at the northern observatory, is to that at the southern one, nearly as 70 is to 89, or as 7 to 9 nearly. This difference is to be attributed chiefly to the effect of the hills on the south of the southern observatory, which were considerably greater and nearer to it than those on the back of the northern observatory. For though the southern observatory was placed 273 feet above the level of the northern one, which removed it considerably more above the centre of gravity of the hill than the latter, it was at the same time placed considerably nearer than the other to the middle in a horizontal direction; so that probably the one difference nearly balanced the other; and accordingly we find that the sum of the affirmative altitudes for o is 44.587, and of those for p 45.339, which differ by only a 45th part nearly.

It only remains now to multiply the sum of the sines by the common breadth of the rings, and by the common difference of the sines of the angles made by the meridian and the several radii. It has already been observed, that the former is $666\frac{2}{3}$, and the latter $\frac{1}{12}$; therefore $\frac{1}{12} \times 666\frac{2}{3} = \frac{200}{9} = \frac{500}{9}$ is their product: consequently, $158.611 \times \frac{500}{9} = 8811\frac{2}{3}$ nearly, is the sum of the two opposite attractions made by the hill, &c. at the two observatories.

In order now to compare this attraction with that of the whole earth, this body may be considered as a sphere, and the observatories as placed at its surface; since the very small differences of these suppositions from the truth, are of no consequence at all in this comparison. Now the attraction of a sphere, on a body at its surface, is known to be $= \frac{2}{3}cd$, where d is = the diameter of the sphere, and $c = 3.1416$ = the circumference of the circle of which the diameter is 1. But cd is = the circumference of the circle to the diameter d ; and therefore the attraction of a sphere will be expressed by barely $\frac{2}{3}$ of its circumference; which is a theorem well adapted to the present computation. The length of a degree in the mean latitude of 45° , is 57028 French toises (see p. 327 Phil. Trans. 1768 :) and the same result nearly is obtained by taking a mean among

all the measures of degrees there set down, that mean being 57038 toises. Dr. H. therefore uses the round number 57030 as probably nearer the truth. This number being multiplied by 6, the product 342180 shows the number of French feet in one degree; but, by p. 326 of the same volume, the lengths of the Paris and London feet are as 76.734 to 72, that is, as 4.263 to 4; therefore, as $4 : 4.263 :: 342180 : 364678 =$ the English feet in one degree; and this being multiplied by 360 the whole number of degrees, there results 131284080 feet for the whole circumference, which are equal to $24864\frac{1}{2}$ miles, making $69\frac{1}{15}$ to a degree in the mean latitude. Lastly, $\frac{2}{3}$ of 131284080 give 87522720 for the measure of the attraction of the whole earth.

Consequently, the whole attraction of the earth is to the sum of the two contrary attractions of the hill, as the number 87522720 to 8811 $\frac{2}{3}$, that is, as 9933 to 1 very nearly, on supposition that the density of the matter in the hill is equal to the mean density of that in the whole earth.

But the Astronomer Royal found, by his observations, that the sum of the deviations of the plumb-line, produced by the two contrary attractions, was 11.6 seconds. Hence then it is to be inferred, that the attraction of the earth is actually to the sum of the attractions of the hill, nearly as radius to the tangent of 11.6 seconds, that is, as 1 to .000056239, or as 17781 to 1; or as 17804 to 1 nearly, after allowing for the centrifugal force arising from the rotation of the earth about its axis.

Having now obtained the 2 results, namely, that which arises from the actual observations, and that belonging to the computation on the supposition of an equal density in the two bodies, the two proportions compared must give the ratio of their densities, which is that of 17804 to 9933, or 1434 to 800 nearly, or almost as 9 to 5. And so much does the mean density of the earth exceed that of the hill.

Thus then we have at length obtained the object which we have been in quest of through the very laborious calculations that have been described in this paper, and in the survey and measurements from which these computations were made; namely, the ratio of the mean density of all the matter in the earth, in comparison with the density of the matter of which the hill is composed. And that ratio we have found to be equal to the ratio of 9 to 5. And, for the reasons beforementioned, it seems we may rest satisfied, that this proportion is obtained to a considerable degree of proximity, probably to within the 50th part, if not the 100th part of its true magnitude. Another question however still arises, namely, what is the density of the matter in the hill? Is its mean density equal to that of water, of sand, of clay, of chalk, of stone, or of some of the metals? For, according to the matter, or different sorts of matter, of which it is formed, and according as it is constituted with or without large vacuities, its

mean density may be greater or less, and that in a degree which is not certainly known. A considerable degree of accuracy in this point could perhaps only be obtained by a close examination of the internal structure of the hill. And the easiest method of doing this would be to procure holes to be bored in several parts of it, from the surface to a sufficient depth, after the manner that is practised in boring holes to the coal mines from the surface of the ground; for by such operation it is known what kind of strata the borer is passed through, together with their dimensions and densities. The proper mean among all these would be the mean density of the hill, as compared to water or to any other simple matter; and thence we should obtain the comparative density of the whole earth with respect to water: but in the present instance, we must be satisfied with the estimate arising from the report of the external view of the hill; which is, that to all appearance it consists of an entire mass of solid rock. It is probable therefore that we shall not greatly err, if we assume the density of the hill equal to that of common stone; which is not much different from the mean density of the whole matter near the surface of the earth, to such depths as have actually been explored either by digging or boring. Now the density of common stone is to that of rain water as $2\frac{1}{2}$ to 1; which being compounded with the proportion of 9 to 5 above found, there results the ratio of $4\frac{1}{2}$ to 1 for the ratio of the densities of the earth and rain water; that is to say, the mean density of the whole earth is about $4\frac{1}{2}$ times the density of water. But since this calculation has been completed, Mr. Playfair, the learned professor of natural philosophy in the university of Edinburgh, has made a mineralogical survey of the hill Schihallien, by which he has discovered that the varieties of rock of which it consists may be reduced to 3 kinds; a granular quartz, which occupies all the middle part of the mountain; a micaceous schistus which encompasses the former nearly all round like a zone, to within 600 feet of the bottom; and lastly a calcareous zone, which may be said to surround the mountain at its base. Though there is some irregularity in the disposition of these zones, this is at least a general idea of the structure of the mountain that does not differ greatly from the truth. By what Mr. Playfair could conjecture, the mean specific gravity of the whole would be about 2.7, that of one stratum being about 2.64, another about 2.75, and some rocks as high as 3, and even 3.2. On the whole then, it appears not unreasonable to suppose the mean specific gravity of the mountain to be from 2.7 to 2.75 or $2\frac{3}{4}$. Now $\frac{9}{4} \times 2\frac{3}{4}$ gives $\frac{9}{2}$ or almost 5; that is, under these circumstances, the medium density or specific gravity of the whole mass of the earth, in proportion to that of water, is nearly as 5 to 1, or that it is 5 times the weight of water.

To what useful purposes the knowledge of the mean density of the earth, as above found, may be applied, it is not the business here to show. Dr. H. there-

fore concludes this paper with a reflection or two on the premises above delivered. Sir Isaac Newton thought it probable, that the mean density of the earth might be 5 or 6 times as great as the density of water; and we have now found, by experiment, that it is very little less than what he had thought it to be: so much justness was even in the surmises of this wonderful man! Since then the mean density of the whole earth is about double that of the general matter near the surface, and within our reach, it follows, that there must be somewhere within the earth, towards the more central parts, great quantities of metals, or such like dense matter, to counterbalance the lighter materials, and produce such a considerable mean density. If we suppose, for instance, the density of metal to be 10, which is about a mean among the various kinds of it, the density of water being 1, it would require 16 parts out of 27, or a little more than one-half of the matter in the whole earth, to be metal of this density, in order to compose a mass of such mean density as we have found the earth to possess by the experiment: or $\frac{4}{15}$, or between $\frac{1}{3}$ and $\frac{1}{4}$ of the whole magnitude will be metal; and consequently $\frac{2}{3}$, or nearly $\frac{2}{3}$ of the diameter of the earth, is the central or metalline part.

Knowing then the mean density of the earth in comparison with water, and the densities of all the planets relatively to the earth, we can now assign the proportions of the densities of them all as compared to water, after the manner of a common table of specific gravities. And the numbers expressing their relative densities, in respect of water, will be as annexed, supposing the densities of the planets, as compared to each other, to be as stated in Mr. De Lalande's astronomy.

Water	1
The sun	$1\frac{4}{15}$
Mercury.....	$10\frac{1}{6}$
Venus.....	$6\frac{1}{3}$
The earth	5
Mars	$3\frac{5}{7}$
The moon	$3\frac{5}{11}$
Jupiter	$1\frac{1}{6}$
Saturn	$\frac{5}{11}$

Thus then we have brought to a conclusion the computation of this important experiment, and, it is hoped, with no inconsiderable degree of accuracy. But it is the first experiment of the kind which has been so minutely and circumstantially treated; and first attempts are seldom so perfect and just as succeeding endeavours afterwards render them. And besides, a frequent repetition of the same experiment, and a coincidence of results, afford that firm dependance on the conclusions and satisfaction to the mind, which can scarcely ever be had from a single trial, however carefully it may be executed. For those reasons it is to be wished, that the world may not rest satisfied barely with what has been done in this instance, but that they will repeat the experiment in other situations, and in other countries, with all the care and precision that it may be possible to give to it, till a uniformity of conclusions shall be found, sufficient to establish the point in question beyond any reasonable possibility of doubt. What has

been already done in the present case will render any future repetition more easy and perfect. But improvements may be made, perhaps both in the mode of computation and in the survey; in the latter, especially, there certainly may. Some improvements of this kind have been hinted at in some parts of this paper, which with others are here collected together, that they may readily be seen in one point of view. They are principally these. Procure one base, or more if convenient, very accurately measured, in such situation, that as many more points as possible in the survey may be seen from it. Assume as many principal or eminent points and objects as may be proper and convenient; and from each one of them measure the angles formed by all the rest that can be seen, both horizontal and vertical angles, and repeat these observations, if convenient, with the instrument varied or reversed, taking the means among the several quantities of each angle. Take then as many sections of the ground, and as far extended in all directions, as the time and circumstances will possibly admit. Of the sections, those that are horizontal or level are the best, as they require no calculation; procure therefore as many as possible of them. In vertical sections observe the vertical angles, not in the plane of the section, but at some other point of which the bearing is also taken from the beginning of the section line, and where the horizontal angles of the poles are taken, for the reasons beforementioned. And it will be a still further convenience if the section be made in such direction as to form a right angle with the line drawn to the point or station from which the vertical angles of the poles are observed, as may be seen from what was before said. It might perhaps be proper to make some experiments on a valley instead of a hill, taking two observatories at the two opposite sides of it, both for the greater variety in this interesting problem, and because also the survey would be more easily made, on account of the ground being more in view at each station than in the case of a hill, which generally hides more than half the compass from the observer. In computing the relative altitudes of all the principal stations, let the operations be performed mutually both backwards and forwards, that is, from both of every two objects, having for that purpose observed at each of them the vertical angle of the other, namely, both the angle of elevation and the angle of depression, and take the mean between the two computed differences of altitude; for this excludes the necessity of making the proper allowances for refraction, and for the curvature of the earth; since the effect of each of these is balanced and corrected by that of the counter observation. But as to those points in the sections which are far distant from the observer, and where great accuracy is required, it may be proper to make the allowance for refraction and curvature, as there is generally no back observation by which their effects may be balanced. These are the chief hints which at present occur, besides the general information to be derived by the

computer from the perusal of the modes of computation that have been described in this paper. As to the surveyor, he will strike out other convenient ways of measurement adapted to the circumstances with which the nature of the survey may happen to be attended.

*XXXIV. An Account of the Blue Shark. By W. Watson, Jun. M.D.,
F. R. S. p. 789.*

The fish was taken on the coast of Devonshire. It had got into shallow water, by which accident Mr. Martin, a great lover of natural history, and who happened to be on the shore, was enabled to drag it out by the tail, and to kill it on the spot. Linnæus places this animal in the class of amphibia, under the name of *squalus glaucus*, and makes use of Artedi's description, viz. *squalus fossula triangulari in extremo dorso, foraminibus nullis ad oculos*. As this fish is well described by Rondeletius and others, I shall only subjoin the following remarks. There are 2 triangular dents at the origin of the tail, both above and below; that which is on the back is the larger and deeper. No orifices are to be seen behind the eyes, as is usual with fishes of this genus. Two white membranes, one to each eye, perform the office of eye-lids. They are placed beneath under the external integuments, and move upwards when they cover the eyes. It is furnished with 5 rows of teeth; these are triangular, and finely serrated. The body is of a fine blue colour, dark on the back, lighter on the sides; the belly and all the under part of the fish white; the fins and tail of a dirty blue; the colour of the blue part is exactly represented by different shades of indigo blue. When the head was placed downwards, a pretty large white pouch came out of its mouth. Ælian supposes this to have served as an asylum to its young brood in time of danger. The length of the fish from the tip of the nose to the end of the tail was 6 feet 8 inches; the length of the pectoral fin 1 foot 4 inches. It was a female, and weighed 55 pounds. As I have never been able to see an accurate drawing of this fish, and as Mr. Pennant, in his *British Zoology*, wishes a farther account may be given of it, I thought it not unworthy of the Society's notice. The fish itself was stuffed, and is now at the British Museum.*

XXXV. A Description of the Exocætus Volitans; or Flying Fish. By Thomas Brown, Surgeon, near Glasgow. p. 791.

This specimen was of the middling size, about 9 inches long, and full 4 round at the thickest part. From the largeness of the head, and the body being neither prominent above nor on the sides, the eyes are situated in such a manner as to discover their danger or prey almost all around them; but when they are

* It is well represented in the splendid *Ichthyology* of Dr. Bloch, pl. 86.

pushed out of their sockets, which the fish is capable of doing considerably, their sphere of vision is greatly increased. The skin is uncommonly firm for the size of the animal, and the scales large and thick. As they have no membrane to shade the eye, they are not able to cover the pupil in any of its motions. The wing is no other than a large pectoral fin, composed of 7 or 8 ribs or pinions, the largest of which, being uppermost, reaches almost to the tail; the rest, gradually shortening to the bottom, are connected by thin membranous pellucid films or webs from their roots, which spring near the gills to the very summit, where they lose themselves in slender points: at their thickest ends, approaching each other, they unite in a line, which, in correspondence with the form of the gills, is nearly the segment of a circle; though they are there connected, it is in such a manner as to allow of being drawn a little asunder, which separation is considerable at the other extremity. The united ends are grooved or hollowed, to receive a ridge or protuberance of the scapula, forming a joint capable of little motion, excepting backward and forward; in the one case the wing lies close to the side; in the other, it is moved from the side forward, forming an acute or right angle with the body of the fish; but neither at this time expanded. These two motions are performed, I presume, in common swimming.

The fore part of its body, from near the back bone downward to the bottom, where it terminates in a point, is fortified just behind the gills by a flat bone on each side, which answer all the purposes both of clavicles and scapulæ in land animals: they are firmly united before, or at the inferior part where they are narrower, and running upward, widening as they approach the back, they become somewhat hollow towards the body, and a little convex outwardly at the broadest part; but towards the gills the edge of the bone on each side is turned outward, like the cape of a garment, to form a smooth surface for them, and at the same time to give lodgement to a strong muscle under it, which fills the whole space, on the superior part of the bone; for on its posterior part the articulation is made with the wing.

Just above the point the scapula is smooth and hollowed, in the manner of a crescent, to allow a tendon to pass from a small muscle which lies on the inferior part of it, next to the body of the fish. The upper part of the ridge that forms the joint, and is received by, or articulated with the wing, is rounded and somewhat enlarged, over which the strong tendon, bound down by a ligament, together with some fibres of the muscle lodged under the inverted edge of the bone, is obliged to pass, and, going over the joint, is inserted into the root of the strongest and uppermost pinion; near to which place, the tendon, passing in the semi-lunated part of the scapula beforementioned, as over a pulley, is also inserted a little way beyond the joint. By the action of these two muscles,

pulling in opposite directions, though both upwardly, at the same time that the lower pinions are kept down by the muscles on the anterior, by those on the posterior and inferior part of the scapula; I say, the effect of the action of these two muscles is, to pull the pinions upward, and at a greater distance from one another, or in other words to expand the wing; for the joint does not allow of any motion upward, and if it did, it would not in the least influence the size of it. The other muscles that lie on the external, internal, and inferior parts of the scapula; together with several small ones that run backward, also serve to move the wing backward and forward. This scapula and wing, with all its apparatus of muscles, can be easily divided, except at the superior part, from the muscles that form the fore part of the body of the fish, being only connected by a cellular medium.

The globe of the eye is large in proportion to the animal; the pupil large too, and nearly, if not altogether, circular. The cornea is less transparent than in the generality of fishes; the fore part of the globe is a good deal flattened, as if a segment or portion had been cut off, for so small a part of the aqueous humour is contained between the cornea and iris, which is of a silver colour, that they are nearly in contact. The optic nerve, though at its egress from the skull it is united by a common external membrane with that of the other eye, does not seem blended with it; this nerve, which is very large, pierces the external coat on the bottom of the ball, but not in the centre; it enters on the side of the axis next the fish's body. The external tunic into which the muscles are immediately inserted, and which gives strength and figure to the whole, is very firm, tough, and almost horny: when the eye is boiled, it seems to have a continuation of fibres, and indeed is of the same colour with the septum of the eye or iris, the cornea separating readily from it, and having then the appearance of the small segment of a great circle or globe, applied to the great segment or side of a much smaller one. All the bottom of the ball is covered with this strong membrane, except in the posterior part, where it becomes abruptly much thinner, more pliant, and of a shape nearly resembling the space left by the union of 4 circles, or a kind of square with its sides bent inward; in the centre of this the optic nerve enters, close to the side of which an opening, like a pin-hole, appears, through which I imagine a small artery passes. The crystalline humour, both in the recent and boiled subject, is entirely spherical; in one it had the appearance of bottle glass; in the other it was bright as crystal. When boiled it seemed to be attached to the vitreous humour, which was not then coagulated; it had an oblong blackish substance fixed to it, like the fragment of a blood vessel, which I could with difficulty separate.

In the fresh fish, the bottom of the eye, except where the optic nerve entered like a small elevated white speck, was laid over with a downy pearl-

coloured paint; a part of which, on squeezing out the vitreous humour, sometimes floats on its surface. On removing this, a black soft painting appeared, which in the bottom of the globe, and some way round the entrance of the nerve, had a reddish cast, or streaks of red, buried in it: these were masses of fine blood vessels, which had probably sprung from the small perforation before-mentioned. In the boiled eye, these paints were not much altered, except the red part, which, like all coagulated blood, was now become dusky. On the back part of the iris, or rather the posterior part of the aqueous humour, it was only covered over with the black coloured pigment. The muscles of the eyes were remarkably strong, broad, and distinct; for in small fishes they are in general so pappy and tender, that it is very difficult to examine them with accuracy.

Their throat or swallow is formed of an oblong, rounded protuberance on the back part of the fauces, and a receiving hollowed substance on the fore part, both plentifully armed with small tenter hooks, pointing backward. They seem to have no remarkable dilatation in the canal of the bowels, in the manner of a stomach; but one tube passes directly from the mouth to the anus, on the upper or anterior part of which lies the heart; on the lower or posterior the liver and gall-bladder; and on the sides of this last are situated the rows, which consist of 2 lobes. On each side, and at some little distance from the heart, is a pale ash-coloured substance, somewhat resembling the lungs of small birds, which seem to join at the back, and to run united all along the hollow depression there as far as the anus. These parts were so very tender, and so little fit for examination with the hands or knife, that it was impossible to discover their use, or to trace any communication they might have with the throat. Nostrils they have, and I could pass a hog's-bristle through them, by the palate, into the mouth.

In the recent ones the abdomen was near $\frac{2}{3}$ full of air. At the basis of the skull I found 2 little flat snow-coloured bones, irregular and rough, such as we find in cod and many other sea fishes. On examining the wings after being some time exposed to the air, I find they become so dry, and the fine thin intervening membrane so rigid, that it is difficult to expand them without violence; at the same time that the motion of the whole wing backward and forward is nothing impaired; this circumstance, which only happens after the fish has been a considerable time out of the water, may have given rise to the common tradition among sea-faring people, that it can fly no longer than its wings are wet, and that in its flight it skims along the surface and dips, skims and dips again, with no other purpose than to moisten and keep them in a flying trim. That in the course of one flight, at least once, twice, or perhaps thrice, it slightly touches the water is certain; but the whole is performed in so small a space of time,

and its continuance in the air is of so short duration, that even in the driest, warmest weather, little is to be apprehended from the too great rigidity of the wings. In my opinion, though this circumstance of moistening them may be of some use, and a secondary advantage, yet they seem to touch the water for a more important purpose, for the same reason that a diver or swimmer, when below the surface of another element, is very frequently obliged to emerge into his own. It may also be of some use in giving the animal new force and vigour for another departure.

But as flying is only a sudden expedient, in order to escape the jaws of their enemies, and by no means their natural or usual mode of existence, there seems not to be any particular or remarkable apparatus necessary for a long subsistence, nothing is wanted but the power of motion in our atmosphere, and the drying of their wings appears to be the only inconvenience they are likely to suffer. Hence it is, that in every other part of their frame and structure, small provision is made by all-bountiful nature for this transmigration. In flying, not only their fins and wings are much expanded, but also their tail; they skim along the surface of the deep with great velocity, somewhat in the manner of a swallow, but in straight lines, and from the blackness of their backs, the whiteness of their bellies, and forked expanded tails, they have much the same appearance.* They can fly 50, 60, or more yards at one stretch, and repeat it a 2d or even a 3d time, only the slightest momentary touch of the surface that can be conceived intervening.

They are seldom solitary, but rise in flocks or shoals.† In taste they somewhat resemble a mackerel. They are driven out of their own element by the shark, the porpoise, the albacore, the bineto, and dolphin, to become a prey in ours to the booby, the man of war, and tropic bird; but I suspect their vision in air is not very distinct, as they often in their flight fall a ship-board, or strike against whatever happens to be in their way; as was the case with all these I examined: and indeed the form of the crystalline humour of the eye seems to countenance this opinion, being of the same spherical figure with that of the greatest part of those fishes that altogether inhabit the watery element.

XXXVII. Experiments on Electricity, being an Attempt to shew the Advantage of Elevated Pointed Conductors. By Mr. Edward Nairne, F.R.S. p. 823.

A difference of opinion prevailed some time ago, and has of late been revived,

* Since writing the above, I find the ancients were acquainted with this species; Pliny mentions it under the name of the *Hirundo*.—Orig.

† We found them in greatest quantities between the latitude of 15° and 10° north, from 20° to 30° west of the meridian of London; but they abound between the Tropics in many other places of the vast Atlantic, as well as in the Indian Ocean.—Orig.

in regard to the termination of conductors for the preservation of buildings from the effects of lightning. Some gentlemen think that they should not terminate in a point, but be blunted; and also that they should not exceed the highest part of the buildings;* they also think, that to prevent lightning from doing mischief to great works, high buildings, and large magazines, the several buildings should remain as they are at top, that is, without having any metal above them, either pointed or not, by way of a conductor; but that on the inside of the highest part of such a building, and within a foot or 2 of the top, it may be proper to fix a rounded bar of metal, and thence continue it down along the side of the wall to any kind of moisture in the ground.† Others again are of a directly contrary opinion; thinking a conductor should not only terminate in a point, but be considerably elevated above the highest part of the building.‡ As it most certainly would be of great consequence to mankind to know which is the most eligible of these opinions, I have attempted, by what I could learn from the artificial lightning of our electrical machines, to determine which method is best to secure buildings from the effects of lightning: whether I have succeeded I leave to the judgement of others to decide from the following experiments and observations, which are submitted with all due deference.

In the apparatus used in the following experiments, the diameter of the glass cylinder was 18 inches; the length of the conductor, which was of wood covered with tin foil, was 6 feet, and its diameter 1 foot. At the end of this conductor was screwed a brass ball, called *c*, of $4\frac{1}{2}$ inches diameter. This conductor when charged by the glass cylinder, being intended to represent a cloud charged with electricity or matter of lightning, will, for distinction sake, be called the artificial cloud, in the following experiments. There was a brass rod, called *d*, on a stand covered with tin foil, having a good metallic communication with the earth; at one end of this rod were screwed other rods, terminating with different sized balls, or a rod terminating with a point. This rod was moveable in a socket, in order that it might be placed with its termination at different distances from the ball at the end of the artificial cloud. As the terminations on this rod were to receive, from our artificial cloud, the stroke or sparks of our artificial lightning, it will be called the receiving rod in the following experiment. The receiving rod with its stand was intended to represent a conductor to a house, on which different terminations might be placed.

Before relating the experiments it may be proper first to premise, that electric

* Mr. Wilson's new Experiments on the Nature and Use of Conductors, p. 7.—Orig.

† Mr. Wilson's Letter to the Marquis of Rockingham, Phil. Trans. vol. 54, p. 247.—Orig.

‡ Ibid. p. 203.—Orig.

fire, drawn off gradually from an electric cloud, was never known to do any mischief, if the substance drawing it off had a good metallic communication with the moist earth; and that when any damage is done, it is occasioned by a stroke of lightning, or in other words the electric fire of the charged cloud suddenly discharged through that body.

Exp. 1. Mr. N. screwed a brass ball, of 4 inches diameter, at the end of the rod *D*, then placed it nearly in contact with the ball *c*, at the end of the artificial cloud; on charging the artificial cloud, the electric fire struck from the ball *c* to the ball at the end of the rod, and continued striking all the while it was gradually removing to the distance of $17\frac{4}{10}$ inches, and sometimes on to 19 or 20 inches.

Exp. 2. The apparatus remaining as in the last experiment, he changed the ball of 4 inches diameter on the rod *D*, and in its stead screwed a ball of one inch diameter; he then placed this very near to the ball *c* as before: on charging the artificial cloud, the electric fire now struck to the ball at the end of the rod *D* of 1 inch diameter, and continued striking while it was gradually removing to the distance of about 2 inches. It then gave over striking, and was succeeded by a hissing noise and a continued light on the 1 inch ball, while it was removing very gradually from the ball *c*, till the distance between the two balls was about 10 inches; the hissing noise then ceased, and the light disappeared on the inch ball. It now began to strike again, and continued striking to the inch ball all the time it was very gradually removed, till the distance was about $14\frac{3}{10}$ inches; and sometimes would continue to strike to $16\frac{3}{10}$ inches.

Exp. 3. The apparatus remaining as in the last experiment, the ball of 1 inch diameter was changed for one of $\frac{3}{10}$ of an inch diameter. This small ball was also placed nearly in contact with the ball *c*: on charging the artificial cloud, the electric fire struck to this ball, and continued striking to it while it was very gradually removed to the distance of half an inch; beyond that, it would not strike to it. But the ball was luminous all the while it was removed beyond the striking distance as far as 33 inches.

Exp. 4. The apparatus remaining as in the last experiment, but only changing the ball of $\frac{3}{10}$, for a wire about $3\frac{1}{2}$ inches long, terminating in a point: on charging the artificial cloud, he could not now get the electric fire to strike the point, though the point was almost in contact with the ball *c*; but when it was about half a tenth of an inch distant from it, then the electric fire ran in a very small stream to the point; but beyond that distance, though moved very gradually, it was only luminous, and continued so at the point all the while it was gradually removing to the distance of 6 feet from the ball *c*, at the end of the artificial cloud.

Exp. 5. The apparatus still remaining, only changing the wire, for the ball of 4 inches diameter, used in the first experiment, having now a small hole through it. Mr. N. then put into this hole a wire, leaving the end, which terminated in a fine point, projecting out only a 10th of an inch beyond the surface of the ball, and directly pointing to the ball c: on charging the artificial cloud, the ball with the point being first placed nearly in contact with the ball c, it was then gradually removed; but not at any distance would it strike to the ball, or the point projecting out of it. The point was luminous at the distance of 30 inches.

Exp. 6. Every thing remained the same as in the last experiment, except only that he now pressed in the point, till it was even with the surface of the 4 inch ball: on charging the artificial cloud, the electric fire now struck to the ball at any distance, from being nearly in contact, all the while it was very gradually removed to as far as $17\frac{1}{4}$ inches, though before in the last experiment, where the point projected from the ball only the 10th of an inch, it would not strike at any distance.

Exp. 7. The apparatus remaining as in the last experiment, Mr. N. took a ball of $3\frac{1}{2}$ inches in diameter, which had a small hole through it, and screwed it to a hollow brass stem. He then put into this hole one end of a wire, and the other end, which was pointed, projected one inch beyond the surface of the $3\frac{1}{2}$ inch ball. This ball and stem, with the pointed wire to it, he fixed to a stand covered with tin-foil; having a good metallic communication with the earth, he placed this stand so that the point was directly opposite to the side of the artificial cloud; and exactly at 5 feet distance from it: then, on charging the artificial cloud, the greatest striking distance from the ball c to the ball of 4 inches diameter, on the receiving rod D, was found to be $16\frac{7}{10}$ inches.

Exp. 8. Every thing continued as in the last experiment, only now he drew the wire out of the ball and stem so far that the point projected 9 inches beyond it; on charging the artificial cloud, the greatest striking distance now was found to be $6\frac{8}{10}$ inches.

Now, in order to see how far a point, or different sized balls fixed on the stand, and having a very small separation in the metallic communication with the earth, would visibly act to carry off the electric fire of the artificial cloud, Mr. N. made the following experiment.

Exp. 9. He took a stick of common sealing-wax, and having fixed a screw to each end, he pasted a slip of tin-foil the whole length of the surface, and having made a separation of the foil of about a 50th of an inch, he screwed the pointed wire into one end, and the other end of the wax to the brass rod, where the ball with the point projecting from it was placed in the last experiment. He

also removed the other stand with the ball, to which the artificial cloud likewise struck in the same experiment; the artificial cloud was then charged, and the stand being placed in such a manner that the point was directly opposite to the side of the artificial cloud; it was then removed till he found the distance at which the light between the separation of the tin-foil no longer became visible. This distance of the point on the wax was above 7 feet, how much farther it might have been luminous he had no opportunity of trying, this distance being the farthest he could remove it in his room, and under the disadvantage of having the end of the artificial cloud within 33 inches of the edge of the wainscot. When a ball of $\frac{3}{10}$ of an inch was put in the place of the point, the light was visible at the distance of $4\frac{1}{2}$ feet, but with a ball of 3 inches diameter only at 2 feet.

Exp. 10. Mr. N. took another stick of sealing-wax, $1\frac{3}{10}$ inch diameter, and about 10 inches long, and pasted on it round pieces of tin-foil of half an inch in diameter, at about half an inch distance from each other. One end of this stick of wax was screwed to the receiving rod, D; and into the other end was screwed the pointed wire used in the 4th experiment. He then laid a piece of brass on this wax, so as to connect all the separations of the round pieces of tin-foil except 2; then the point of this wire on the wax was placed nearly in contact with the ball. On charging the artificial cloud the electric fire now struck to the point, and continued to strike to it all the while it was gradually removed to the distance of $1\frac{1}{10}$ inch: beyond that distance it would not strike, but the point continued luminous till it was removed to the distance of 3 feet.

Exper. 11. The apparatus remaining as in the last experiment, he only took away the piece of brass which laid on the wax to connect the pieces of tin-foil together. The charged artificial cloud did not now strike to the point till it was removed from the ball c to the distance of $4\frac{1}{2}$ inches; it then began to strike to it, and continued striking while it was gradually removing sometimes to 10 inches; but when the point was removed beyond the greatest striking distance, the point was not luminous as in the last experiment, except when the artificial cloud discharged its electric fire out into the air, in a diverging pencil from the ball c; it was then luminous, but at that instant only. Every time the artificial cloud struck to the point, the electric fire made a beautiful appearance in passing off between the separation of the pieces of tin-foil. Mr. N. then connected all the tin-foil on the wax, so as to leave no separation; then the charged artificial cloud would not strike to the point at any distance.

Exp. 12. Mr. N. placed the rod D, with the 4 inch ball at the end as in the first experiment, this he put on a glass pillar to insulate it; then from the rod he made a communication to the earth, with about 3 feet of silver wire, which was

only $\frac{1}{800}$ part of an inch diameter: on charging the artificial cloud, it struck to the ball D, as in the first experiment, viz. $17\frac{4}{10}$ inches.

Observation 1.—From the first 3 experiments it appears, that the artificial cloud strikes at distances greater as the termination of the conductor is more blunted, or as it terminates with the larger ball; and that the striking distance is less as the end of the conductor tends more to a point; and in the 4th experiment, that when the end of the conductor is pointed, the point is not struck at any distance whatever: but continues luminous to a certain distance, carrying off silently the electricity of the artificial cloud. It seems from these experiments, that pointed conductors are to be preferred before those terminating with a large ball, the pointed one depriving the cloud silently of its electric fire; whereas the ball receives the electric fire in a strong spark. And in the 5th experiment, where a point projects but $\frac{1}{10}$ of an inch from a ball of 4 inches diameter, neither the ball, nor point projecting from it, is struck at any distance. This seems to show the utility of a pointed rod, even if it projects but a small distance above the highest part of a building. The 6th experiment shows, that a point within the surface of a ball does not prevent the ball being struck. The 7th and 8th experiments likewise show, that our artificial cloud strikes to a ball of 4 inches diameter, only at the distance of $6\frac{8}{10}$ inches, when the point is drawn out 9 inches from the $3\frac{1}{2}$ inch ball, placed opposite to the side of the artificial cloud; and that when the point projects only 1 inch, that then it strikes to the 4 inch ball at $16\frac{4}{10}$ inches distance.

May we not from these last 2 mentioned experiments conclude, that the more elevated our pointed conductors are, the greater is the chance of preserving our buildings from the effects of lightning? For here the point being elevated or projecting 9 inches out of the ball, representing the highest part of a building, was found continually depriving the artificial cloud of its electric fire to such a degree that it would not strike half the distance that it did when the point was elevated only 1 inch. And from the 9th experiment we learn, that the conductor terminating in a point acts at a far greater distance than one terminating with a ball, in carrying off the electric fire, or matter of lightning from the artificial cloud. And though the point was luminous so far, yet there was no distance whatever at which the artificial cloud would strike to it.

From the 10th and 11th experiments we learn, that the metallic part of the conductor being separated or discontinued is the reason that the artificial cloud strikes to the point; and that it strikes farther to the point as the number of the separations are increased; and that if the metallic communication with the moist earth be made complete, then the charged cloud will not strike to the point. When a conductor to a building, terminating in a point, has been struck, Mr. N. thinks that there had not been a complete and sufficient metallic

communication with moist earth; and from all the accounts he had met with, this seems to have been the cause of their having been struck. From the 12th experiment we learn, that a very fine wire will conduct a strong spark.

Mr. N. contrived another moveable artificial cloud: it consisted of a hollow tube of wood, with a ball at each end, being together about 6 feet in length: from each end was suspended a light hollow wooden cylinder; these with the balls and tube were covered with tin-foil: it was placed with its axis resting on two semi-circular hollows in a piece of brass fixed on a glass pillar, by which it was insulated: it moved very easily on its axis, and was brought to a horizontal position by means of two moveable pieces.

Exp. 13. Mr. N. first put this moveable artificial cloud into a horizontal position, and placed it so that the brass on which the axis rested, was in contact with the end of the artificial cloud. Then, under each of the hollow cylinders he placed a stand, having a good metallic communication with the earth. On one of the stands was put a pointed wire, the same as was used in the 4th experiment; and on the other, a brass ball of 3 inches diameter. He then placed the point and ball each 12 inches from the middle of the bottom of its correspondent hollow cylinder: on charging the artificial cloud (which consequently charged the moveable artificial cloud in contact with it) the point was luminous, and the moveable artificial cloud still remained in a horizontal position, though there was now a point under one end and a ball under the other; and on ceasing to charge the two clouds, it was found directly after, that the point had drawn off almost all the electric fire from both.

Exp. 14. The two clouds being charged, he took away the stand with the 3 inch ball on it: the point remained luminous, and the moveable artificial cloud still continued horizontal, not being attracted to the point, though there was now only the stand with the pointed wire under one end of it, the point having carried off the electric fire as in the last experiment.

Exp. 15. The two clouds being again charged, he replaced the stand with the ball on it; and now, instead of taking away this stand, as he did in the last experiment, he took away the stand with the pointed wire on it: the consequence was, that the end of the moveable artificial cloud was now attracted down to the ball till it came to its striking distance, where it then discharged its electricity on it in a strong spark. The moveable artificial cloud then receded a little till it was charged, and then was attracted by the ball as before, till it came to its striking distance, when it again discharged its electricity at once, and so continued striking and then receding to a little distance as long as the two clouds were charged.

Exp. 16. The moveable artificial cloud continuing to strike to the ball as in the last experiment, he now replaced the stand with the pointed wire on it, then

immediately the point became luminous, and the moveable artificial cloud ceased striking to the ball, and soon returned to its horizontal position as at first.

Exp. 17. The apparatus remaining as in the last experiment, and the two clouds continuing to be charged, he took away the stand with the point; then the moveable artificial cloud was attracted down to the ball, and struck as before. He then placed the stand with the point close to the stand with the ball; on which the point became instantly luminous, and immediately the moveable artificial cloud ceased striking, soon returning from the ball and settling nearly in a horizontal position. There the point carried off the electric fire as in the 13th and 14th experiments.

Observation.—From the 13th experiment, with the point under one end of the moveable artificial cloud, and a 3 inch ball under the other end, it seems as if neither the ball nor point attracted either end; or that they both equally attracted, or repelled each end, as in either case the moveable artificial cloud would remain horizontal. And in the 14th experiment, in order to try whether the point would attract or repel the moveable artificial cloud, the ball was taken away, and only the point was left under one end, as now all the action of the point either to attract or repel would be exerted on that end which was now over the point, and consequently that end should either be attracted down to it, or repelled from it: but from the experiment it appears, that the point drew off all the electricity silently, without either attracting or repelling the end of the moveable artificial cloud which was over it, as it continued horizontal all the time it was charged.

The 15th experiment was made to see if the ball would either attract or repel the moveable artificial cloud, as in this experiment the ball only was under one end, and every thing else exactly the same as when the point only was under. But here we find the effect of the ball very different from that of the point; for instead of drawing off the electricity silently, as the point did, without attracting the end of the moveable artificial cloud; on the contrary, the moveable artificial cloud was attracted down towards the ball, till it came within its striking distance, where it discharged its electric fire all at once on the ball with a loud and strong spark. And again, in the 16th experiment, where the stand with the point is replaced at the other end, while the cloud was attracted down to the ball, it instantly prevented its striking to the ball by carrying off the electric fire as fast as the moveable artificial cloud received it from the artificial one. And from the 17th experiment we learn, that when the stand with the point was placed close to the stand with the ball, while the moveable artificial cloud was striking to it, the cloud even in this case instantly ceased to strike to the ball, returning from it and soon settling nearly in a horizontal position.

Exp. 18. Mr. N. took off the cylinders from the ends of the moveable

artificial cloud, and placed it, together with the glass pillar by which it was insulated, on another foot of such a height that when the ball at one of the ends was 3 inches above the ball c at the end of the artificial cloud, then the moveable artificial cloud was horizontal. He then placed the stand with the point at the distance of 18 inches, and directly under the ball at the other end: on charging the artificial cloud, the point was luminous; and that end of the moveable artificial cloud which was 3 inches above the ball c was attracted down to it, then receded from it about 1 inch, and then the artificial cloud kept constantly striking to it, as long as it continued to be charged. On ceasing to charge the artificial cloud, it was found immediately after, that the point had carried off almost all the electric fire.

Exp. 19. Every thing remaining as in the last experiment, and the artificial cloud being charged, Mr. N. took away the stand with the point, and placed in its stead the stand with the 3 inch ball on it, exactly at the same distance as the point: then instantly that end of the moveable artificial cloud, which had continued to be attracted down near to the artificial cloud, was repelled from it, and at the same time the other end was attracted by the 3 inch ball, till it came so near as to discharge its electricity on it in a strong spark. The end of the moveable artificial cloud then receded from the 3 inch ball, the other end being now attracted by the artificial cloud, which charged it almost instantly again; it then receded with rapidity from it, and discharged its electric fire on the ball as before, and thus continued in great motion receiving strong sparks from the artificial cloud, and discharging them on the ball, representing in miniature a storm of lightning where an electrical cloud strikes into another cloud, and that discharges itself on a building which is without a regular conductor, or one terminating with a ball.

Exp. 20. While this storm of lightning in miniature continued, Mr. N. removed the stand with the 3 inch ball, and placed in its stead the stand terminating with the point; the point was immediately luminous, and in an instant the artificial storm ceased. The end of the moveable artificial cloud, next the charged artificial one, was now attracted to it, as in the 18th experiment.

Exp. 21. The apparatus remaining as in the last experiment, Mr. N. unscrewed the pointed wire from the stand, and screwed it into one end of a stick of wax of 6 inches in length, with 11 pieces of tin-foil stuck on it at the 40th part of an inch asunder; he then screwed this wax with the point on the stand, and placed it so that the point was directly under the end of the moveable artificial cloud, and at 18 inches distance as before: on charging the artificial cloud, the moveable artificial cloud was first attracted, then repelled, and so alternately as when the stand with the 3 inch ball was under; but with this difference, that instead of striking in a strong spark, as it did to the 3 inch ball,

it now struck with a very small spark to the point, the point depriving the moveable cloud of most of its electricity as it approached it, which was very visibly passing away between the separations of the tin-foil.

Exp. 22. Every thing as in the last experiment, a chain only being hung on the pointed wire, to complete the metallic communication with the earth. As soon as the chain was hung on, the moveable artificial cloud instantly ceased striking to the point, and the other end of it was then attracted to the artificial cloud, which then kept constantly striking to it: the moveable artificial cloud did not return to the point as it did before the chain was hung on, as in the last experiment.

Observation.—In the 18th experiment, where the moveable artificial cloud was intended to represent a cloud in its natural state receiving electric fire from a charged cloud, we find, that the point deprived it of its electric fire which it received from the charged one so fast, that the artificial cloud could keep constantly striking to the other end, without repelling it from it; but that in the 19th experiment, when the ball was under the end of the moveable artificial cloud instead of the point; then, instead of the artificial cloud continuing to strike to the other end without repelling that end, it now first attracted and charged it with electricity, or the matter of lightning; then immediately repelled it, and being attracted by the ball under the other end, it moved down with an acquired velocity, till it came within its striking distance, discharging then its electricity on the ball with a loud and strong spark, and so continued alternately receiving and discharging its electric fire on the ball. It being first attracted, at which time it received an additional quantity of electricity, and then repelled till it had discharged that additional quantity, is exactly agreeable to all the known laws of electricity.

This experiment may throw some light on what we sometimes see in nature, viz. one cloud continuing to strike towards the earth a considerable time; for should a cloud in its natural state be so situated between a charged cloud and the earth, it may be first attracted and charged, and then repelled, and if it should be repelled so as to come within the attracting distance of any blunt body with a good or partial conductor, it would then continue to be attracted till it came within its striking distance, and then discharge its lightning suddenly on it; and if it was not repelled or attracted beyond the attracting distance of the charged cloud, it would again be attracted to it and charged, then repelled as before, and so may continue receiving and discharging the lightning, till the charged cloud is nearly exhausted of its electricity or matter of lightning.

But if a cloud, in its natural state, should be so situated within the striking distance of a charged cloud, and at the same time within the power of a good metallic conductor terminating in a point; then from these experiments it

appears, that the charged cloud would continue striking to the natural cloud, and that would again part with it silently, by means of the point, without striking on it till the charged cloud is nearly exhausted. When we see a cloud striking into another cloud several times together, we conclude from all the known laws of electricity, that the cloud which first received the stroke must have discharged part or the whole of what it received before it could receive another stroke.

In the 20th experiment we find, that though the moveable artificial cloud was in great motion, receiving and discharging its electric fire on the ball, that, on taking away the ball, and putting the point in its place, the artificial storm immediately ceased. In the 21st experiment, where the point was on a stick of wax, with separations in the metallic communication with the earth, we find that, even in that situation, the stroke on the point was very small to what it was on the ball with a good communication, great part of the electric fire visibly passing off as the cloud approached the point; and when the metallic communication was made complete by hanging on the chain, it then ceased striking to the point.

Exp. 23. The tube before called the moveable artificial cloud, from its moving very easily on its axis, was, by means of 2 screws now fixed, immoveable, with the ball at one of its ends above the ball c at the end of the artificial cloud, at the height of 3 inches; and underneath the ball, at the other end, was placed the stand with the point, at the distance also of 3 inches. The artificial cloud was then charged, and an electric spark struck from the ball c at the end of it to the ball of the now fixed cloud above it, and at the same instant struck from the ball at the other end to the point at 3 inches.

Exp. 24. The tube used in the last experiment (which Mr. N. now again calls the moveable artificial cloud from its being made again to move freely on its axis) was placed exactly in every respect as in the last experiment; the only difference was, that it could now move easily on its axis, whereas in the last experiment it was fixed immoveable at the distances: on charging the artificial cloud, the moveable artificial cloud, instead of receiving a spark, and discharging it on the point, as in the last experiment, was now attracted down to the artificial cloud there remaining, not striking to the point, or returning to it so long as the artificial cloud continued to be charged.

Observation.—By the 23d experiment we see, that if our cloud is fixed at a certain distance between the artificial cloud and the point, the fixed cloud, at the instant it receives the electric spark, directly discharges it again on the point. But in the 24th experiment, where there is no other alteration than making the cloud moveable on its axis, the distances being exactly the same, the end of the cloud then recedes from the point and will not strike to it. This 24th experi-

ment is much more agreeable to nature than the 23d, for clouds are not fixed but floating bodies.

In order to see the effect of rods terminating with balls of different sizes, or terminating with a point, moving swiftly under the artificial cloud, Mr. N. made use of the following apparatus, viz. A hollow tube of wood covered with tin-foil, with a heavy weight fastened to one end of this tube; and at about 3 inches above the weight was an axis, it was then suspended by this axis between two wooden pillars: in this wooden tube was a brass rod, which was moveable, so that a ball or point fixed on it could be raised to the height required.

Exp. 25. A ball of $1\frac{3}{10}$ inch diameter was fixed to the under part of the artificial cloud, and then this apparatus was placed under it with a point, the swinging rod was held down to the floor, and the point covered: then the artificial cloud was charged by a certain number of turns of the glass cylinder; the swinging rod with the point was then let go, and passed swiftly and very near to the ball under the artificial cloud. This was repeated several times, removing the point lower each time, till the greatest striking distance to the point was found, which was generally $1\frac{1}{8}$ inch.

Exp. 26. The point being removed, a ball of $\frac{3}{10}$ diameter was placed in its stead, and tried as the point in the preceding experiment; the striking distance was generally found to be $2\frac{1}{10}$ inches.

Exp. 27. The $\frac{3}{10}$ ball being removed, another of $1\frac{3}{10}$ was tried as in the last 2 experiments, and the striking distance was generally 15 inches.

Observation.—In the 25th experiment it appears, that the point is struck by means of a swift motion; and from the 26th experiment, that the ball of $\frac{3}{10}$ was struck farther than the point; and the ball of $1\frac{3}{10}$ in the 27th experiment, at a much greater distance than either, even with the swift motion. From these experiments Mr. N. is induced, first, to prefer elevated pointed conductors; next to them those that are pointed, though they project but a little distance above the highest part of the building; and after them those terminating in a ball, and placed even with the highest part of a building, though it appears from these experiments, that they are more liable to be struck, and have not the property of guarding the distant parts of a building as elevated points have; but if they have a good metallic communication with the earth, the building might not be hurt, though the lightning should strike on the conductor. Those conductors which are recommended to be within the inside of a building, and a foot or 2 below the highest part,* are certainly very dangerous, especially for all that part of the building above the conductor.

Mr. N. was a witness of the dreadful effects of a stroke of lightning on a

* Mr. Wilson's letter to the Marquis of Rockingham, Philos. Trans. vol. 54, p. 247.—Orig.

house that had an accidental partial-conductor within the inside of the upper part of the house. It happened to a house near Ratcliff Highway, on the 29th of July, 1775. In the uppermost room stood a large iron triblet, of about 3 feet in height; the lightning made its way through the roof of the house, throwing off a number of tiles, rending and tearing the laths and plaster on the inside, to get to the triblet, on which it struck from thence to a hammer, which lay on the floor near it: it then made its way, by partial conductors, down into the cellar to the leaden pipe, which conveyed water from the main, and in its way rent the house in various parts, so as to make it scarcely habitable. It left marks of fusion on different metallic utensils. If the conductor from the triblet had happened to have been made by a complete and sufficient metallic communication with the earth, all parts of the house below would have been preserved; but the parts above would have been equally rent and destroyed.

Now to make a few remarks on Mr. Wilson's paper, intitled, *New Experiments and Observations on the Nature and Use of Conductors*, Mr. N. says, in p. 2 Mr. Wilson mentions, that he had declared his dissent in the year 1772 against pointed conductors. In answer to this I can only say, that from these experiments of mine, the direct contrary appears to be the fact; that the point, instead of increasing an actual discharge, prevents a discharge where it otherwise would happen; and that the blunted conductors tend to invite the clouds charged with lightning. The first 11 experiments of Mr. Wilson are intended to show, that pointed conductors draw off the electricity from a cloud at a much greater distance than those which are blunted. My 9th experiment proves the truth of those experiments of his; the only difference is, that in mine the point acted on my artificial cloud at a much greater distance; from which it appears, to use his own words, p. 4, "that a charged body is exhausted of more of the fluid by a pointed than by a blunted conductor." In answer to his 12th experiment, and on to the 18th, where the model of the house moved swiftly, under his large artificial cloud, and where the point was struck at 5 inches, and sometimes at a quarter of an inch farther than his $\frac{3}{10}$ ball; I must observe, that I have sometimes seen his apparatus at the Pantheon, with which he made his experiments, strike as far to the $\frac{3}{10}$ ball as the point; but in my experiments I have had it strike 10 inches $\frac{3}{10}$ to a point, and 10 inches and $\frac{8}{10}$ to a $\frac{3}{10}$ ball; but to a 1 inch and $\frac{3}{10}$ ball it commonly struck to 15, and sometimes to 16 inches. In answer to the 18th and following experiments I must observe, that the substitute being fixed is unnatural: for clouds are composed of a fluid matter, moving with the utmost facility in another fluid substance; and from my 23d experiment, where the substitute was fixed, the point was struck; yet in the 24th experiment, where there was no other alteration than allowing the cloud to move freely, then the point was not struck. I imagine, if Mr. Wilson's large artificial cloud at

the Pantheon, which was 155 feet long and 16 in diameter, had been properly insulated, and there had been several cylinders properly mounted to have charged it, he would have found the striking distance, and his other experiments, very different from what he did, particularly those where his substitute was fixed about $1\frac{1}{2}$ inch from his large artificial cloud.

I must beg to intrude a little more on your time to remark on that part of Mr. Wilson's paper, where from his experiments he seems to conclude, that the lightning at Purfleet first struck on the point of the rod of the conductor, and then, by a lateral part of that stroke, struck the cramp on the coping stone. I believe, if he had examined the situation of the stone, and the place where the cramp was struck, he would have found, that if the lightning had struck on the point of the conductor, that to have produced that effect on the stone, it must after it had struck on the point, and passed down a quantity of metal, have struck from the metal up into the air, then down again on the cramp, and then again to the metal it had left, for the small dent or hollow made by the lightning was on the upper surface of the stone, and yet the metallic communication to the earth continued from the point under the stone which was struck. It appears more probable to me, from the trifling damage it did, that the charged cloud had passed over the pointed conductor, and had been exhausted of a great part of its electricity in passing; and that after it had passed, it was attracted down lower by a ridge of hills that was beyond, and that the cloud being out of the influence of the point to prevent its striking, the end of the cloud might strike at an angle in the cramp, and so to the metallic part of the conductor, which was only about 7 inches below. I shall conclude with observing, that Mr. Henly and myself had the pointed rod of the conductor at Purfleet taken down to examine the point; but we found no appearance on it that showed that it had been struck.

XXXVI. Reasons for Dissenting from the Report of the Committee appointed to consider of Mr. Wilson's Experiments; including Remarks on some Experiments exhibited by Mr. Nairne. By Dr. Musgrave, F. R. S. p. 801.

I do not find that Dr. Franklin, in any of the passages where he speaks of the efficacy of sharp-pointed conductors to prevent electrical explosions, has expressed any doubt of their being universally preferable for this purpose to those which have a blunt or spherical termination. The same observation may be made of the other gentlemen who are the advocates for his doctrine. It may therefore be assumed, that both he and they mean to assert a universal proposition, "That sharp points will, in all cases, draw off the electrical fluid silently, within the distance at which rounded ends will explode; or, at least, that the former sort will in no case receive an explosion at a greater distance than the latter." Though I dissent from this doctrine, I do not mean to assert the contrary universal pro-

position, but only to deny the universality of that asserted by Dr. Franklin, which I apprehend to be sometimes true, and sometimes also false. Here it may be of use to show, that sharp points having the most perfect communication with the earth, are not wholly exempt from receiving them. My first authority shall be Dr. Franklin himself. "Let a person," says he, p. 60, "standing on the floor, present the point of a needle at 12 or more inches from the prime conductor, and while the needle is so presented, the conductor cannot be charged, the point drawing off the fire as fast as it is thrown on by the electrical globe. Let it be charged, and then present the point at the same distance, and it will suddenly be discharged." The word suddenly means, I suppose, that it will receive an explosion; that being the most natural and obvious proof of the suddenness of the discharge. The same thing is more directly asserted by Mr. Henly, in vol. 64 of the Phil. Trans. p. 138, where he says, that in discharging 3 of his large jars, to the coating of which he had connected a wire nicely tapered to a point, the fire flew to the point, and the jars were discharged with a full and loud explosion. A 3d, and equally decisive proof, is furnished by Mr. Nairne's own experiments, though seemingly made with a contrary view. For when the double or interrupted conductor was used, and the 2d conductor fixed down by screws at about 3 inches distance from the first, the point presented to the contrary end of the 2d conductor was found to receive a strong and loud explosion, with a white light at the distance of at least 3 inches.

If we compare this experiment with another, very common one, exhibited at the same time by Mr. Nairne, the comparison will perhaps lead us to the discovery of a principle on which electrical explosions very frequently depend. Though the point, in the circumstances above described, received so strong an explosion, yet when it was presented directly to the prime conductor, it received no explosion whatever at any distance, unless a succession of weak sparks, at the distance of about a quarter of an inch, can be called so. To what must this difference be attributed? Plainly to the different quantity of electric fluid accumulated on the prime conductor in the one and the other case. Where the point is presented to the prime conductor, from the time the machine begins to work, the property attributed to them, and which, in some cases, they really possess, of stealing away the electricity silently; this property, I say, operating from the very beginning, prevents the electric fluid from being accumulated in the prime conductor, and of course the quantity of it will always be small. But when a double or interrupted conductor is used, the 2d conductor receives no electricity till the prime conductor is pretty highly charged, and, if put at the greatest striking distance, not till it is fully charged, and consequently the sharp point presented to the opposite end can carry away none of it till that time; when the whole quantity is thrown off at once. It should seem then, that the

explosion in one case, and the non-explosion in the other, depended wholly on the different quantities to be thrown off: whence it will follow that though a small quantity of electricity will pass off silently on a point, yet that this power is very limited; for that if a somewhat greater quantity be applied suddenly to a sharp point, it will not pass off silently, but create an explosion in proportion to its density.

I cannot omit the opportunity here offered, of remarking the unfairness of the insinuations that have been thrown out to the prejudice of Mr. Wilson. Had there been any juggle in making his experiments, it would certainly have been detected by the committee appointed to examine them. And in case of such a detection, it was the duty of the committee to lay open the imposture both to the society and the public. Instead of which, instead of disputing or even doubting the fairness of them, they have in a manner admitted it, by only saying in their report, that they appear to be inconclusive. This, I say, is admitting the facts to be fairly stated: unless we could suppose their regard for Mr. Wilson, and tenderness for his reputation, had induced them, after detecting the fallacy of his experiments, to pass it over in silence; of which improper partiality I do not know that they are so much as suspected. What therefore the committee, after a strict scrutiny of the matter, did not think themselves warranted to say, I take for granted they would not insinuate; and that therefore such insinuations can only arise from the levity of more obscure persons, puzzled perhaps by the seeming contradiction between Mr. Wilson's experiments and those of Mr. Nairne, and too impatient to investigate the real causes of that difference. I am persuaded however, that the known property of sharp points to carry off electricity silently, when the quantity is small, together with that other principle, which I apprehend I have here established, that they cease to do so when the quantity is large; that these two (taken together) will clear up the whole difficulty, and account for Mr. Nairne's experiments, without any impeachment to those of Mr. Wilson.

I have already had occasion, in the course of this argument, to consider 2 of those experiments, of which therefore I shall say no more; but proceed, without further digression, to examine those that remain. The first I shall mention, is that in which the prime conductor being previously charged with electricity, a sharp point is presented to it within the attracting, and without the exploding distance, and then brought slowly on towards it. In this case no explosion follows; neither is there any reason to expect it should, because the quantity of electricity is gradually diminished by the approach of the point, so that when it comes within the striking distance, there is not enough left to make an explosion.

Again, when an intermediate conductor is used, terminated at each end with

a ball, and the middle of it resting in equilibrio on a pivot, on which it has a free oscillation upwards and downwards; if in this state a point is placed under the end most distant from the prime conductor, the machine being then worked, the other end will approach so near the prime conductor, as that the stream of electricity will flow freely into it, as fast as it is produced by the action of the wheel. In this case there will be no explosion; and the reason is obvious, because the 2d conductor, when it approaches so near the first as to form an uninterrupted channel for the electric stream, becomes virtually a part of the first. Hence the point operates on both together, just as it does when presented directly to the prime conductor, that is, it steals away the electricity by little and little, leaving not enough to give an explosion. When, instead of the point, a polished ball is placed under the same end as before, this being less disposed to receive the electric fluid, conveys away none of it; so that accumulating to a certain degree on the prime conductor, it explodes on the contiguous end of the 2d, which, having a free oscillation, flies up with the stroke, and carries the opposite end towards the ball, where, being saturated, it gives a snap; the recoil of which snap throws that end up, and the contrary end back towards the conductor, and so on alternately, as long as the machine continues working.

The event however is widely different when the 2d conductor, instead of having a free oscillation, is screwed down in one place, and at such a distance from the prime conductor, as not to receive the electric fluid till considerably accumulated. For then the sharp point, previously opposed to its other end, discharges it, as before observed, not in a continued stream and silently, but at intervals, and with a strong explosion.

The last of Mr. Nairne's experiments, and the only one yet unconsidered, is that of the sharp point, which, being fixed to a kind of inverted pendulum, oscillated with great velocity under the prime conductor, without receiving any explosion. Now from this experiment I do not comprehend how any general conclusion can possibly be drawn. It has been already shown, from the acknowledgement of Dr. Franklin, and the experiments of Mr. Henly and Mr. Nairne, that electricity, accumulated to a certain degree, will explode on a point. If therefore, in any particular instance it does not explode, what can we infer from it, but that the accumulation in every such instance was not sufficiently great; which may happen either from the smallness of the apparatus, or from want of care in making the experiment.

And now if we look back upon Mr. Nairne's experiments (which, by the bye, have not all of them the merit of novelty) we shall find them to be nothing more than different exemplifications of this well-known principle, that sharp points, giving less resistance to the ingress of the electric fluid, will draw it off at a greater distance than blunt or spherical terminations, and where the quantity

is small, will draw it off silently. This then is the whole amount of his experiments; the only one of them in which the electric fluid had time to accumulate, being attended with a different event from the rest, and producing, as might reasonably be expected, a strong explosion.

It is not however this single property of sharp-pointed conductors, which must decide the question. We have already seen, that there are two properties inseparable from them, both of which must be taken into the account, before we can determine the propriety of affixing them to buildings, particularly powder magazines, as preservatives from lightning: first, their greater propensity to admit the electric fluid, in consequence of which they act on electrified bodies at a greater distance than rounded ends will; and, 2dly, their incapacity to draw away more than a certain quantity of electricity without an explosion. The first quality enables them, when electricity is accumulated gradually, or when they are brought gradually towards the electrified body, to steal away the fluid by little, till there is not enough left to give an explosion. And hence, in common experiments, the point, placed at a greater distance than the ball, will prevent the electricity from exploding, as it otherwise would do, on the latter. But if we combine this quality with the 2d, the superior propensity to admit, with the incapacity in certain circumstances of discharging silently, it will be evident, *a priori*, that the phenomena must in such cases be reversed, just as they appear to be in Mr. Wilson's experiments; that the point must strike at a greater distance, and the rounded end at a lesser.

These phenomena, to persons who have not carefully considered them, must appear so extraordinary, that unless the cause of the diversity is explained, they will perhaps be led to suspect some unfairness in making the experiment. The truth however is this; that when the two conductors are set at the greater of the two distances, the absolute quantity of electricity collected before the explosion, is exactly the same in each experiment; and therefore the distances of the ball and point from the 2d conductor being equal, and the greatest at which either of them will be struck, the explosion will go to the point, as being more susceptible, and giving less resistance than the ball. But in the 2d supposed case, when the 2d conductor is set considerably within the former distance, the quantity of electricity which explodes on the point and the ball is not the same; the point in this case exerting its known property of stealing away the electricity silently, which the ball from its greater resistance is incapable of doing. The consequence is, that the quantity accumulated to give an explosion on the ball is greater than that which explodes upon the point, and, being greater, will very naturally explode to a greater distance.

I come now to consider more particularly the practical question, whether the sharp-pointed or the blunt conductors are most proper to be affixed to buildings,

as preservatives from lightning. And here it is necessary to observe, that buildings may be exposed to a stroke of lightning in several different ways. The lightning which, to avoid prolixity, I shall only speak of as positive electricity: the lightning, I say, may accumulate directly over the building; or it may be brought towards the building by a small cloud fetching it in several successive trips from a large cloud at some distance; or a large electrified cloud may be carried rapidly towards it by the wind: a circumstance this by no means rare, there being no less than 4 instances of it upon record in the *Phil. Trans.* In the first of these supposed cases, a sharp-pointed conductor might possibly drain the cloud of its lightning as fast as it began to accumulate, and so prevent any explosion whatever. In the 2d, as the cloud, by supposition, not being driven in one direction by the wind, could not move with any remarkable velocity, it is reasonable to imagine, that in this case also there might be no explosion; and that the electricity of the larger cloud might be gradually exhausted. But if, according to the 3d supposition, a cloud of great extent, and highly electrified, should be driven with great velocity in such a direction, so as to pass directly over the sharp-pointed conductor, there can be no doubt but that such a point, from its superior readiness to admit electricity, would take the explosion at a much greater distance than a rounded end, and in proportion to the difference of that striking distance would do mischief, instead of good.

But perhaps it will be said, that every stroke of lightning falling on a sharp point is previously diminished by that point, and therefore may more easily be transmitted through the conductor, than when it falls undiminished on a rounded end. On this supposition I must observe, that it not only contradicts Mr. Wilson's experiments at the Pantheon, but also Mr. Henly's experiment already referred to in this paper, where the fire flew to a very taper point, and melted the end with a strong and loud explosion. So also the sharp-pointed conductors affixed in America to the houses of Mr. West, Mr. Raven, and Mr. Mayne, do not seem to have diminished the force of the explosion, if we may judge from the violence of its effects as related at large in Dr. Franklin's works. It should seem therefore, that the power of diminishing a stroke, like that of preventing it, is only contingent, and depends, as before said, on the degree of velocity with which the lightning moves.

The sum of the whole is, that conductors, terminated by sharp points, are sometimes advantageous, and at other times prejudicial. Now as the purpose for which conductors are fixed on buildings is, not to protect them from one particular sort of clouds only, but, if possible, from all, it cannot surely be advisable to use that kind of conductors, which, if they diminish danger on one hand, will increase it on the other. It is the duty of a pilot to keep out of the

way of rocks; but it is also incumbent on him, in avoiding the rock, not to take so large a compass as to run his ship on a quicksand. When I say that sharp-pointed conductors may in some cases diminish danger, I speak of them, perhaps, rather too favourably: for their power of stealing away the electric fluid being confined to cases where the accumulation is small, it follows, that they only operate where their operation is not wanted. The cases against which we wish principally to provide, are the explosions of extensive and highly electrified clouds; and here we have seen, that blunted ends, as acting to a much smaller distance, are entitled to the preference.

If it be admitted, that sharp-pointed conductors are attended with any, the slightest degree of danger, how much must that danger be augmented by carrying them high up into the air, by fixing them on every angle of a building, and making them project in every direction? Ought this to be advised while there is still a doubt of the possibility of their doing mischief? And can the committee, therefore, be perfectly justified for giving such a decided preference to the use of sharp conductors, in defiance of numerous experiments, not one of which they have attempted to controvert?

Lastly, I beg leave to correct an expression I have used with respect to pointed conductors, that they only operate where their operation is not wanted. Now this is not accurately true: for if by operating on a quantity of electricity too small in itself to do mischief, they prevent its increasing to a great and dangerous quantity, this would, as far as it goes, be a very considerable advantage. I ought therefore to have said only, that pointed conductors afford no protection where the danger is great and imminent, and only obviate that which is distant and problematical; and that these last are not the cases against which we principally wish to provide.

XXXVIII. On the Use of an Amalgam of Zinc, for the Purpose of Electrical Excitation, &c. By Bryant Higgins, M.D. p. 861.

By divers experiments lately made by myself, and repeated by others, I find that, agreeable to the suggestion made in my last course of chemistry, the amalgam of zinc, which contains 4 times more quicksilver than zinc, is much better for electrical excitation than the tin amalgam of the ingenious Mr. Canton, when used in the same circumstances. I also find, that electrical cylinders are easily and effectually cleansed by applying to them a piece of the dry skin of the dog-fish while the cylinders are turned round; and that in this method of cleansing the glass cylinders, we avoid the inconvenience of removing the cushion, and the danger of scratching the glass, to both which we are exposed in the use of whiting and other cleansing powders.

XXXIX. *Chemical Experiments and Observations on Lead Ore.* By Richard Watson, D. D., F. R. S. p. 863.

Reprinted in Bishop Watson's Chemical Essays..

XL. *Description of a most Effectual Method of securing Buildings against Fire.* Invented by Charles Lord Viscount Mahon,* F. R. S. p. 834.

§ 1. This new and simple method may be divided into 3 parts; namely, under-flooring, extra-lathing, and inter-securing, which particular methods may be applied, in part or in whole, to different buildings, according to the various circumstances attending their construction, and according to the degree of accumulated fire, to which each of these buildings may be exposed, from the different uses to which they are meant to be appropriated.

§ 2. The method of under-flooring may be divided into 2 parts, viz. into single and double under-flooring. The method of single under-flooring is as follows. A common strong lath, of about $\frac{1}{4}$ of an inch thick, either of oak or fir, should be nailed against each side of every joist, and of every main timber, which supports the floor intended to be secured. Other similar laths ought then to be nailed the whole length of the joists, with their ends butting against each other: these Lord M. called the fillets. The top of each fillet ought to be at $1\frac{1}{2}$ inch below the top of the joists or timbers against which they are nailed. These fillets will then form, as it were, a sort of small ledge on each side of all the joists.

§ 3. When the fillets are about to be nailed on, some of the rough plaster hereafter mentioned must be spread with a trowel all along that side of each of the fillets, which is to lie next to the joists, in order that these fillets may be well bedded in it when they are nailed on, so that there should not be any interval between the fillets and the joists.

§ 4. A great number of any common laths, either of oak or fir, must be cut nearly to the length of the width of the intervals between the joists. Some of the rough plaster referred to above (§ 3) ought to be spread, with a trowel, successively on the top of all the fillets, and along the sides of that part of the joists which is between the top of the fillets and the upper edge of the joists. The short pieces of common laths just mentioned ought, in order to fill up the intervals between the joists that support the floor, to be laid in the contrary direction to the joists, and close together in a row, so as to touch each other, as much as the want of straightness in the laths will possibly allow, without the laths lapping over each other; their ends must rest on the fillets spoken of above (§ 2) and they ought to be well bedded in the rough plaster. It is not proper to

* Now (1807) Earl Stanhope.

use any nails to fasten down either these short pieces of laths, or those short pieces hereafter mentioned (§ 7).

§ 5. These short pieces of laths ought then to be covered with one thick coat of the rough plaster spoken of hereafter (§ 9), which should be spread all over them, and which should be brought, with a trowel, to be about level with the tops of the joists, but not above them. This rough plaster in a day or two should be trowelled all over, close home to the sides of the joists; but the tops of the joists ought not to be anywise covered with it.

§ 6. The method of double under-flooring is, in the first part of it, exactly the same as the method just described. The fillets and the short pieces of laths are applied in the same manner; but the coat of rough plaster ought to be little more than half as thick as the coat of rough plaster applied in the method of single under-flooring.

§ 7. In the method of double under-flooring, as fast as this coat of rough plaster is laid on, some more of the short pieces of laths, cut as above directed (§ 4), must be laid in the intervals between the joists on the first coat of rough plaster; and each of these short laths must be, one after the other, bedded deep and quite sound into this rough plaster while it is soft. These short pieces of laths should be laid also as close as possible to each other, and in the same direction as the first layer of short laths.

§ 8. A coat of the same kind of rough plaster should then be spread over this 2d layer of short laths, as there was on the first layer above described. This coat of rough plaster should (as above directed § 5 for the method of single under-flooring) be trowelled level with the tops of the joists, but it ought not to rise above them. The sooner this 2d coat of rough plaster is spread on the 2d layer of short laths just mentioned (§ 7) the better. What follows, as far as § 13, is common to the method of single as well as to that of double under-flooring.

§ 9. Common coarse lime and hair, such as generally serves for the pricking-up-coat in plastering, may be used for all the purposes before or hereafter mentioned; but it is considerably cheaper, and even much better, in all these cases, to make use of hay instead of hair, to prevent the plaster-work from cracking. The hay ought to be chopped to about 3 inches in length, but no shorter. One measure of common rough sand, 2 measures of slacked lime, and 3 measures, but not less, of chopped hay, will prove, in general, a very good proportion, when sufficiently beaten up together in the manner of common mortar. The hay must be well dragged in this kind of rough plaster, and well intermixed with it; but the hay ought never to be put in, till the 2 other ingredients are well beaten up together with water. This rough plaster ought never to be made thin for any of the work mentioned in this paper. The stiffer it is the better, pro-

vided it be not too dry to be spread properly on the laths. If the flooring boards are required to be laid very soon, a 4th or a 5th part of quick* lime in powder, very well mixed with this rough plaster just before it is used, will cause it to dry very fast.

§ 10. When the rough plaster-work between the joists has got thoroughly dry, it ought to be observed whether there be any small cracks in it, particularly next to the joists. If there are any, they ought to be washed over with a brush, wet with mortar-wash, which will effectually close them; but there will never be any cracks at all, if the chopped hay and the quick lime be properly used.

§ 11. The mortar-wash made use of is merely this. About 2 measures of quick lime, and 1 measure of common sand, should be put into a pail, and should be well stirred up with water, till the water grows very thick, so as to be almost of the consistency of a thin jelly. This wash, when used, will get dry in a few minutes.

§ 12. Before the flooring boards are laid, a small quantity of very dry common sand should be strewed over the rough plaster-work, but not over the tops of the joists. The sand should be struck smooth with a hollow rule, which ought to be about the length of the distance from joist to joist, and of about $\frac{1}{4}$ of an inch curvature; which rule, passing over the sand, in the same direction with the joists, will cause the sand to lie rather rounding in the middle of the interval between each pair of joists. The flooring boards may then be laid and fastened down in the usual manner; but very particular attention must be paid to the rough plaster-work and to the sand being most perfectly dry before the boards are laid, for fear of the dry-rot; of which however there is no kind of danger, when this precaution is observed.

§ 13. The method of under-flooring Lord M. has also applied, with the utmost success, to a wooden stair-case. It is made to follow the shape of the steps, but no sand is laid on the rough plaster-work in this case.

§ 14. The method of extra-lathing may be applied to cieling joists, to sloping roofs, and to wooden partitions. It is simply this: as the laths are about to be nailed on, some of the above mentioned rough plaster ought to be spread between these laths and the joists, or other timbers, against which the laths are to be nailed. The laths ought to be nailed very close to each other. When either of the ends of any of the laths laps over other laths, it ought to be attended to that these ends be bedded sound in some of the same kind of rough plaster.

* Lord M. practised this method in an extensive work with great advantage. In 3 weeks this rough plaster grows perfectly dry. The rough plaster, so made, may be applied at all times of the year with the greatest success. The easiest method, by much, of reducing the quick lime to powder is, by dropping a small quantity of water on the lime-stone, a little while before the powder is intended to be used; the lime will still retain a very sufficient degree of heat.—Orig.

This attention is equally necessary for the 2d layer of laths hereafter mentioned (§ 15).

§ 15. This first layer of laths ought to be covered with a pretty thick coat of the same rough plaster spoken of above (§ 9). A 2d layer of laths ought then to be nailed on, each lath being, as it is put on, well squeezed and bedded sound into the soft rough plaster. For this reason, no more of this first coat of rough plaster ought to be laid on at a time than what can be immediately followed with the 2d layer of laths. The laths of this 2d layer ought to be laid as close to each other as they can be, to allow of a proper clench for the rough plaster. The laths of the 2d layer may then be plastered over with a coat of the same kind of rough plaster, or it may be plastered over in the usual manner.

§ 16. The 3d method, which is that of inter-securing, is very similar, in most respects, to that of under-flooring; but no sand is afterwards to be laid on it. Inter-securing is applicable to the same parts of a building as the method of extra-lathing just described; but it is not often necessary to be used.

§ 17. His lordship made a great number of experiments on every part of these different methods. He caused a wooden building to be constructed at Chevening, in Kent, in order to perform them in the most natural manner. The methods of extra-lathing and double under-flooring were the only ways used in that building.

On the 26th of September 1777, Lord M. repeated some of his experiments before the president and some of the fellows of the R. S., the Lord Mayor and Aldermen of the city of London, the committee of city lands, several of the foreign ministers, and a great number of other persons.

§ 18. The first experiment was to fill the lower room of the building, which room was about 26 feet long by 16 wide, full of shavings and faggots, mixed with combustibles, and to set them all on fire. The heat was so intense, that the glass of the windows was melted like so much common sealing-wax, and ran down in drops; yet the flooring boards of that very room were not burnt through, nor was one of the side timbers, floor-joists, or cieling-joists, damaged in the smallest degree; and the persons who went into the room immediately over the room filled with fire, did not perceive any ill effects from it whatever, even the floor of that room being perfectly cool during that enormous conflagration immediately underneath.

§ 19. He then caused a kind of wooden building, of full 50 feet in length, and of 3 stories high in the middle, to be erected, quite close to one end of the secured wooden house. He filled and covered this building with above 1100 large kiln faggots, and several loads of dry shavings, and he set this pile on fire. The height of the flame was no less than 87 feet perpendicular from the ground, and the grass on a bank, at 150 feet from the fire, was all scorched; yet the

secured wooden building, quite contiguous to this vast heap of fire, was not at all damaged, except some parts of the outer coat of plaster-work. This experiment was intended to represent a wooden town on fire, and to show how effectually even a wooden building, if secured according to his new method, would stop the progress of the flames on that side, without any assistance from fire-engines, &c.

§ 20. The last experiment made that day, was the attempting to burn a wooden stair-case, secured according to the simple method of under-flooring. The under side of the stair-case was extra-lathed. Several very large kiln faggots were laid, and kindled, under the stair-case, round the stairs and on the steps; this wooden stair-case notwithstanding resisted, as if it had been of fire-stone, all the attempts that were made to consume it. His lordship afterwards made 5 other still stronger fires on this same stair-case, without having repaired it, having also filled the small place in which this stair-case is entirely with shavings and large faggots; yet the stair-case is still standing, being but little damaged.

§ 21. In most houses it is necessary only to secure the floors; and that according to the method of single under-flooring described above, in § 2, 3, 4, and 5. The extra-expence of it, all materials included, is only about 9 pence per square yard, unless there should be particular difficulties attending the execution, in which case it will vary a little. When quick lime is used, the expence is a trifle more. The extra-expence of the method of extra lathing, is no more than 6 pence per square yard for the timber side-walls and partitions; but for the cieling about 9 pence per square yard. No extra-lathing is necessary in the generality of houses.

XLI. A Method of finding, by the Help of Sir Isaac Newton's Binomial Theorem, a Near Value of the very Slowly Converging Infinite Series $x + \frac{1}{2}x^2 + \frac{1}{3}x^3 + \frac{1}{4}x^4 + \frac{1}{5}x^5 + \text{Ec.}$ when x is very nearly equal to 1. By Francis Maseres, Esq., F. R. S., &c. p. 895.

If the capital letters A, B, C, D, E, &c. be put for the numeral co-efficients of the powers of x in the said series, so that A shall be $= 1$, $B = \frac{1}{2}$, $C = \frac{1}{3}$, $D = \frac{1}{4}$, $E = \frac{1}{5}$, and so on, we shall have $B = \frac{1}{2}A$, $C = \frac{2}{3}B$, $D = \frac{3}{4}C$, $E = \frac{4}{5}D$, &c., and the proposed series $x + \frac{1}{2}x^2 + \frac{1}{3}x^3 + \frac{1}{4}x^4 + \frac{1}{5}x^5 + \text{Ec.}$ will be $= x + \frac{1}{2}Ax^2 + \frac{2}{3}Bx^3 + \frac{3}{4}Cx^4 + \frac{4}{5}Dx^5 + \frac{5}{6}Ex^6 + \frac{6}{7}Fx^7 + \frac{7}{8}Gx^8 + \text{Ec.}$; in which series the fractions $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$, $\frac{5}{6}$, $\frac{6}{7}$, $\frac{7}{8}$, &c. which generate the co-efficients of the powers of x in the several terms after the first term x , are derived from each other by the continual addition of 1 to both their numerators and their denominators.

This observation suggests a method of finding a near value of the sum of this series by the help of Sir Isaac Newton's binomial theorem, which may be explained as follows. If m and n represent any two whole numbers, the reciprocal

of the $\frac{m}{n}$ th power of the binomial quantity $1 - x$, or, according to Sir Isaac

Newton's notation of powers, the quantity $(1 - x)^{\frac{-m}{n}}$, will, according to that celebrated theorem, be equal to the infinite series

$1 + \frac{m}{n} Ax + \frac{m+n}{2n} Bx^2 + \frac{m+2n}{3n} Cx^3 + \frac{m+3n}{4n} Dx^4 + \frac{m+4n}{5n} Ex^5$ &c.; in which series the capital letters A, B, C, D, E, F, G, H, &c. stand for 1 and the co-efficients of $x, x^2, x^3, x^4, x^5, x^6, x^7, x^8$, &c. Now it is evident, that the generating fractions $\frac{m+n}{2n}, \frac{m+2n}{3n}, \frac{m+3n}{4n}, \frac{m+4n}{5n}, \frac{m+5n}{6n}, \frac{m+6n}{7n}, \frac{m+7n}{8n}$, &c. are derived from $\frac{m}{n}$, and from each other, by the continual addition of n to both their numerators and denominators. Therefore, though they are greater than they would be if m was subtracted from the numerator of each of them, that is, than the

fractions $\frac{n}{2n}, \frac{2n}{3n}, \frac{3n}{4n}, \frac{4n}{5n}, \frac{5n}{6n}, \frac{6n}{7n}, \frac{7n}{8n}$, &c. and consequently, than the equal corresponding fractions $\frac{1}{2}, \frac{2}{3}, \frac{3}{4}, \frac{4}{5}, \frac{5}{6}, \frac{6}{7}, \frac{7}{8}$, &c.; yet the farther we go in the series,

the less is the proportion in which they exceed the latter fractions; insomuch that, if we go far enough in the series, we may find terms in it whose proportion to the corresponding terms in the series $\frac{1}{2}, \frac{2}{3}, \frac{3}{4}, \frac{4}{5}, \frac{5}{6}, \frac{6}{7}, \frac{7}{8}$, &c. shall approach as near as we please to a proportion of equality. And, by taking n of a very great magnitude in comparison of m , we may even make the first terms of the series

$\frac{m+n}{2n}, \frac{m+2n}{3n}, \frac{m+3n}{4n}, \frac{m+4n}{5n}, \frac{m+5n}{6n}, \frac{m+6n}{7n}, \frac{m+7n}{8n}$, &c. approach very nearly to an equality with the corresponding terms of the series $\frac{1}{2}, \frac{2}{3}, \frac{3}{4}, \frac{4}{5}, \frac{5}{6}, \frac{6}{7}, \frac{7}{8}$, &c.

which are the generating fractions of the proposed series $x + \frac{1}{2}x^2 + \frac{1}{3}x^3 + \frac{1}{4}x^4 + \frac{1}{5}x^5 + \frac{1}{6}x^6 + \frac{1}{7}x^7 + \frac{1}{8}x^8 +$ &c. In order to this, let m be taken $= 1$, and $n = 1,000,000,000,000$, that is, $=$ a billion, or the square of a million, which,

to avoid the frequent repetition of so many cyphers, we will call b . Then will $(1 - x)^{\frac{-1}{1,000,000,000,000}}$, or $(1 - x)^{\frac{-1}{b}}$, be $= 1 + \frac{1}{b} Ax + \frac{1+b}{2b} Bx^2 + \frac{1+2b}{3b} Cx^3$

$+ \frac{1+3b}{4b} Dx^4 + \frac{1+4b}{5b} Ex^5$ &c. which, on account of the great magnitude of b , $2b, 3b, 4b, 5b, 6b, 7b$, &c. in comparison of 1, will be nearly equal to (though somewhat greater than) $1 + \frac{1}{b} Ax + \frac{b}{2b} Bx^2 + \frac{2b}{3b} Cx^3 + \frac{3b}{4b} Dx^4 + \frac{4b}{5b} Ex^5$ &c.

or $1 + \frac{x}{b} + \frac{x^2}{2b} + \frac{x^3}{3b} + \frac{x^4}{4b} + \frac{x^5}{5b} +$ &c. Therefore, multiplying both sides by b ,

we shall have $b \times (1 - x)^{\frac{-1}{b}}$ nearly $= b + x + \frac{1}{2}x^2 + \frac{1}{3}x^3 + \frac{1}{4}x^4 + \frac{1}{5}x^5 +$ &c.;

and, subtracting b from both sides, the proposed series $x + \frac{1}{2}x^2 + \frac{1}{3}x^3 + \frac{1}{4}x^4 + \frac{1}{5}x^5 +$ &c. will be nearly equal to $b \left(\frac{1}{1-x} \right)^{\frac{-1}{b}} - b$. We must therefore first subtract x from 1, and then divide 1 by the remainder, which will give us a quotient equal to $\frac{1}{1-x}$. And, having found this quotient, we must extract its b th,

or 1,000,000,000,000th, root, and multiply the said root by b , or 1,000,000,000,000; and lastly, from the product we must subtract b , or 1,000,000,000,000: and the remainder thus obtained will be nearly equal to the proposed infinite series $x + \frac{1}{2}xx + \frac{1}{3}x^3 + \frac{1}{4}x^4 + \frac{1}{5}x^5 + \&c.$ Q. E. I.

As an example of this method of finding the value of the series $x + \frac{1}{2}x^2 + \frac{1}{3}x^3 + \frac{1}{4}x^4 + \frac{1}{5}x^5 + \&c.$ Mr. M. supposes x to be equal to $\frac{9}{10}$; whence $1 - x = \frac{1}{10}$, and $\frac{1}{1-x} = 10$. In this application he finds the value of the said series to be nearly equal to 2.302585093, the hyperbolic logarithm of the number 10.

XLII. A Method of Extending Cardan's Rule for resolving one Case of a Cubic Equation of this Form, $x^3 - qx = r$, to the other Case of the same Equation, which it is not naturally fitted to solve, and which is therefore often called the Irreducible Case. By Francis Maseres, Esq., F. R. S., &c. p. 902.

It is well known that Cardan's rule, for resolving the cubic equation $x^3 - qx = r$, is only fitted to resolve it when $\frac{1}{4}r^2$ is equal to, or greater than, $\frac{1}{27}q^3$; and that it is of no use in the resolution of the other case of this equation; for in this case $\frac{1}{4}r^2 - \frac{1}{27}q^3$ becomes a negative quantity, and consequently its square root becomes impossible, and the expression given by Cardan's rule for the value of x involves in it the impossible quantity $\sqrt{(\frac{1}{4}r^2 - \frac{1}{27}q^3)}$, and therefore is unintelligible and useless.

Yet it is possible, by the help of Sir Isaac Newton's binomial theorem, to extend this rule to this latter case, in which $\frac{1}{4}r^2$ is less than $\frac{1}{27}q^3$, and which it is not of itself fitted to resolve; or, to speak with more accuracy, it is possible to derive from the expression of the value of x given by Cardan's rule for the resolution of the equation $x^3 - qx = r$ in the first case, in which $\frac{1}{4}r^2$ is greater than $\frac{1}{27}q^3$, another expression somewhat different from the former, that shall exhibit the true value of x in the 2d case, in which $\frac{1}{4}r^2$ is less than $\frac{1}{27}q^3$, provided it be not less than $\frac{1}{54}q^3$; and this without any mention of either impossible, or negative, quantities. To show how this may be effected, is the design of this paper.

Mr. M. then sets forth, at great length, the whole process of finding out Cardan's rule; which, being given in most books on algebra, may well be omitted in this place. This process brings out, for one form of Cardan's rule, $x = \sqrt[3]{\frac{1}{2}r + \sqrt{(\frac{1}{4}r^2 - \frac{1}{27}q^3)}} + \sqrt[3]{\frac{1}{2}r - \sqrt{(\frac{1}{4}r^2 - \frac{1}{27}q^3)}}$, or $x = \sqrt[3]{a + \sqrt{(a^2 - b^3)}} + \sqrt[3]{a - \sqrt{(a^2 - b^3)}}$, where $a = \frac{1}{2}r$, and $b = \frac{1}{3}q$. Or, if we put $\sqrt{(a^2 - b^3)} = c$, then that rule becomes $x = \sqrt[3]{(a + c)} + \sqrt[3]{(a - c)}$. Now by expanding the two quantities $\sqrt[3]{(a + c)}$ and $\sqrt[3]{(a - c)}$, or the equal forms $(a + c)^{\frac{1}{3}}$ and $(a - c)^{\frac{1}{3}}$, into infinite series, by Newton's binomial theorem, and adding the two series together, the value of x will be expressed in one series only. And because the two quantities $a + c$ and $a - c$ have the same terms a and c , but differing only in the sign of the latter, the terms of both the two series will

be the same, of which those of all the former will be alternately positive and negative after the first term, but those of the latter all negative after the first term; by which means it happens that all the even terms of the former, being positive, will be cancelled by all the equal or corresponding even terms of the latter, which are negative; and hence the sum of the two series, or the value of x , will be denoted by double the sum of the odd terms of either series only. Thus, by the binomial theorem,

$$(a + c)^{\frac{1}{3}} = a^{\frac{1}{3}} \times \left(1 + \frac{c}{3b} - \frac{c^2}{9b^2} + \frac{5c^3}{81b^3} - \frac{10c^4}{243b^4} \&c.\right)$$

$$(a - c)^{\frac{1}{3}} = a^{\frac{1}{3}} \times \left(1 - \frac{c}{3b} - \frac{c^2}{9b^2} - \frac{5c^3}{81b^3} - \frac{10c^4}{243b^4} \&c.\right)$$

then the sum of these two give, for the value of x , the series $2\sqrt[3]{a} \times \left(1 - \frac{c^2}{9b^2} - \frac{10c^4}{243b^4} - \frac{154c^6}{6561b^6} - \&c.\right)$

Now this series is a general expression for the value of x , or the root of the equation $x^3 - qx = r$, in all cases whatever, when $a = \frac{1}{2}r$, and $b = \frac{1}{3}q$, or $c^2 = a^2 - b^3 = \frac{1}{4}r^2 - \frac{1}{27}q^3$. When $\frac{1}{4}r^2$ is greater than $\frac{1}{27}q^3$, then c^2 or $\frac{1}{4}r^2 - \frac{1}{27}q^3$ will be positive, and the signs of all the terms of the above series, after the first term, will be negative, or to be subtracted from the first term. But when $\frac{1}{4}r^2$ is less than $\frac{1}{27}q^3$, then c^2 or $\frac{1}{4}r^2 - \frac{1}{27}q^3$ will be negative, and all the even terms of the series, will have their signs changed, from minus to plus; by which means the series will have its signs alternately positive and negative, after the first term, leaving only the 3d, 5th, 7th, &c. terms negative.

XLIII. Account of the Advantages of a newly-invented Machine much varied in its Effects, and very Useful for determining the Perfect Proportion between different Moveables acting by Levers and Wheel and Pinion. By Mr. Le Cerf, Watch-maker at Geneva. p. 950.

This machine may be called a compass of geometrical and mechanical proportion; its property is to resolve a great number of problems analogous to the theory and practice of watch-making, in a manner which is at the same time very evident and much more convenient than the mode of arithmetical calculation. It not only shows the proportion of the diameters between the wheels and the pinions, but serves to determine every species of proportion of the calibers, of the size of pivots, width of pallets, magnitude of cylinders, and in general of whatever is an object of dimension. In every case it affords a product, either in unity or fractions, as perfect as the application of it to the mechanism we are considering is easy.

Nothing therefore can more conduce to the perfection of watch-making than an instrument which immediately determines all the dimensions and proportions required; dimensions and proportions which could not heretofore be obtained but by means so long, so laborious, and so imperfect, that they have probably

given rise to a general relaxation with regard to the true principles, and to the adoption of rules and measures arbitrary, vague, and which produce very considerable errors: such, for instance, is the error of taking a little more than 3 points of a tooth, in order to determine the size of the pinions of 6, without regard to the revolutions which the different numbers of the teeth of the wheels produce. The consequence is, that an equal measure being taken on a wheel of 18, and on one of 72 teeth, produces a variation of 1 entire revolution, as will be shown hereafter. For instance, the pinion is the divisor of the wheel as well as that of the circle, each tooth of which is to raise its required proportion of degrees. A pinion of 6 is to raise 60° per tooth, because $6 \times 60 = 360$; one of 7 is to raise $51^\circ 25\frac{1}{2}$ and some seconds; one of 8 is to raise 45° ; one of 10 is to raise 36° : finally, one of 12 must incontestibly raise 30° per tooth, since $12 \times 30 = 360$.

Among various methods that have been used for the solution of this important problem, I shall mention only the most simple one, easily understood. I set in motion two wheels, each of 12 teeth. By making one wheel turn the other, it is evident that their two diameters must be perfectly equal, allowing for the necessary shake between the teeth.* It will hence follow that these two movers, which we will suppose wheel and pinion, must reciprocally raise the 30° required; but if we increase one of these so as to make a revolution more than the other, which is fixed at 12 teeth, and at 1 line diameter, this last will indeed have 24 teeth; but instead of 2 lines of diameter, it will have only 23 and $\frac{1}{12}$ relatively to that of the pinion; or if we give it exactly the double diameter, it will have 25 teeth instead of 24; consequently, on 3 revolutions, or 36 teeth, we must subtract from the wheel 2 and $\frac{1}{12}$ of a line; and so on as far as 12, or rather 11, effective revolutions; for the first, being supposed to be in equilibrio with its pinion, ought not to be reckoned. The wheel will then indeed have 144 teeth, or 12×12 ; but it will only have 11 times as much in diameter, that is, 11 lines instead of 12, in order that the angles of these two movers may always be in the same perfect proportion to each other, of which the precise raising of 30° per tooth is one of the most convincing proofs.

To make this more intelligible, as the primitive radii ought to be equal between these movers, a pinion of 6 wings, being a line in diameter, requires the taking off of $\frac{1}{6}$ to take from it what is useless in its catch; this is what forms the apparent diameter. This deduction will reduce the primitive radius of this pinion to $\frac{5}{6}$ of a line; for as its revolutions about a wheel of 12 teeth are as 2 to

* This shake is very inconsiderable, especially when the pinion and the teeth of the wheel are properly opened; for, according to my experiments, the deduction to be made on this account is reduced to the 96th part of the circumference of a pinion of 12, which produces the sum of the 8th part of a tooth on the wheel, whatever its number may be.—Orig.

1, what is to make the radius of the wheel for the first revolution will be equal to this in $\frac{1}{2}$, or 1 line; but for 2, 3, and 4 revolutions, and so on, we must add $\frac{1}{2}$ of a line to the radius already laid down, for as many revolutions as the pinion must make more than the wheel: a wheel of 12 teeth will consequently have $\frac{3}{2}$ of a line radius or diameter; one of 18 teeth, $\frac{5}{2}$; one of 24 teeth $\frac{7}{2}$, and so on. The same thing holds true in all the other pinions, whatever the number of them may be, where the same rule is to be observed. Thus on 10 revolutions of a pinion of 10, we must deduct from the diameter of the wheel 1 diameter of the pinion; as many on 8 revolutions of a pinion of 8; on 7 of a pinion of 7, as many; on 6 of a pinion of 6, as many; and so on for all the movers acting by revolutions and teeth.

The diameter of the wheel must of necessity increase in a ratio of the actual revolutions of the pinion, and not in that of its apparent diameter; since we are considering the working parts of wheels and pinions, where the angles, relatively to the change of the curves and the circumference, must be reciprocally in the same proportion, to operate constantly the degrees of raising which are required. We may easily conceive, that as the teeth of a wheel become constantly more parallel to each other, and approach to the straight line, as their number increases, the depth in that case need not be so great, and the curve being shorter is more favourable to the uniformity of the frictions than on wheels that are few in number: this is what most commonly happens to pinions of 6, the numbers of whose wheels hardly ever exceed 60 or 72 teeth. For if, in the usual method of using a pinion-gage, we were to take, on a wheel of 12 or 18 teeth, a little more than the 3 points (which points from the nature of their angles would hitch into the 3 teeth at one and the same time) supposed to be in due proportion with a pinion of 6, and that afterwards we were to make use of the same measure on a wheel of 42 teeth, the natural consequence would be, that this pinion would be, by $\frac{3}{6}$, or half its diameter, too large. A pinion of 7, supposed to be of a proper size, with a wheel of 21 teeth, would, according to the same measure of 3 full teeth taken on a wheel of 70, be too large by its whole diameter, insomuch that the wheel, instead of 70 teeth, ought to have but 63, &c. Though the superior angles, that is, the circumference of the wheels from 12 to 120, and above ad infinitum, relatively to the size of a pinion given according to our rule, are and ought to be invariably the same; the lower angles, that is, the thickness of the teeth, are yet extremely different in every wheel. For instance, all the wheels from 12 to 120 being sloped, and rounded with the same file, the teeth of the wheel of 48 are half as thick again as those of 12, &c. In proportion as the parallelism of the angles increases, the teeth of course become larger and fuller, and each tooth consequently always exceeds the intermediate space in the same proportion, and gradually as far as the right line. This determines the

proper space between the teeth of each wheel, whence follows the curve, equally constant and advantageous in every respect, both with regard to the execution and to the uniformity in its lead on a pinion such as this, the thickness of the teeth of which is a 6th part of its diameter, just as the excess of the primitive radius or true diameter is the 6th part of its apparent diameter.

According to this exposition, founded on experiments, it is easy to see what errors must have crept into clock-making by the old method of taking, for the pinion of 6, 3 or a little more than 3 points of teeth; for a pinion of 7, the 3 full teeth when finished; for a pinion of 8, the 3 full teeth and the void space as far as the 4th; for a pinion of 10, 4 full teeth, so as the wheel comes out of the engine, (the making of which has hitherto been entrusted to women, children, and servants); finally, for a pinion of 12, 5 points of rather strong teeth, with the same arbitrary formality. These methods then, are only fit to perpetuate misunderstanding and division on the manner of fixing and ascertaining the first principles of watch-making. Nothing therefore ought to make us more sensible of the want there is of our compasses of proportion, than the consideration of measures as absurd in themselves as they are impossible to fix; there is no proportion which can be fixed (except it be by mere chance) between these movers, not even within so much as 8 or 10 degrees of rise per tooth; whereas by our machines, whose utility and the manner of making use of them is comprehended at first sight, we may come within a quarter or even an 8th of a rise per tooth, or nearly so. It is not so easy to adapt these principles to our machines for all sorts of pinions, numbers of teeth, and sizes of wheels, and generally to whatever acts by lever, or wheel and pinion, or whatever is susceptible of relation, dimension, proportion, and compensation, from the first to the last movers of a watch or clock; for the least fault, either in the systems or geometrical principles, or the mechanical execution, is as fatal as would be the setting down a cypher too much or too little in the solution of a problem; and the more fatal as the machine is more complicated.

As to the true point of the opening of the teeth, this is my rule. I take rather a soft file, in thickness 6 and a quarter of its diameter for a pinion of 6, 7 and a quarter for one of 7, 8 and a quarter for one of 8, 10 and a quarter for one of 10, 12 and a quarter for one of 12, and so on; and I believe that this rule may be the easiest for the preservation both of the best figure of the curves and the necessary force of their teeth. Just as the thickness of the teeth of a pinion of 6 must be the 6th part of its diameter; that of the pinion of 7, a 7th; one of 8, an 8th; and so on, for any number of pinions whatever. Just in the same manner as the length of the teeth of the wheel, which shall be exactly the aperture of their angles, or (which comes to the same) half the

diameter of its pinion of 6, $\frac{3}{7}$ for a pinion of 7, $\frac{3}{8}$ for one of 8, $\frac{3}{10}$ for one of 10, $\frac{3}{12}$, or a quarter, for a pinion of 12, and so on.

XLIV. New Experiments on the Leyden Phial, respecting the Termination of Conductors. By Benj. Wilson, Esq., F. R. S. p. 999.

In the 64th volume of the Philos. Trans., there is a paper of Mr. Henly's on the subject of conductors, containing several experiments, intended to show that pointed terminations are preferable to spherical ones for securing buildings, &c. from accidents by lightning. On those experiments Mr. W. made some observations, and particularly on the 5th, where a point and ball were placed at the same distance from a sphere of copper, so as to make part of the circuit in the Leyden experiment. In the description of that experiment Mr. W. objected to the 2 chains employed, because the metallic communication was, by that method, considerably interrupted, on account of a want of contact between the several links composing the chains. He did not then repeat the experiment, because the particular circumstances attending the Leyden phial appeared very unlike what happen in nature; and therefore he contented himself with pointing out the several circumstances in which they differed; and in observing that, according to Mr. Henly's account, the point did not protect the rounded end from being struck, which it ought to have done, if Dr. Franklin's philosophy was well founded.

Since that time an occasion has offered which made it necessary to try this particular experiment. The occasion alluded to arose from a late investigation of Mr. Nairne's experiments by Dr. Musgrave, who was desirous of having that experiment repeated; because, as it stood in Mr. Henly's account, it seemed to contradict a considerable part of the doctor's reasoning. Not being furnished with an apparatus to make the experiment, he requested Mr. Cavallo to assist him with his; and though it was not so complete for the purpose as could be wished, yet it answered sufficiently well to show, that an attention to the circumstance of a perfect communication in this experiment was very material to discover the truth; and that the want of it had probably occasioned the ball to be struck in preference to the point, as related by Mr. Henly: for on employing a wire instead of the chains, the point was struck at more than 3 times the distance of the ball.

Seeing so great a difference between the two experiments, Mr. W. procured such an apparatus as he thought would be the least exceptionable for determining the fact on which these different appearances seemed to depend; namely, a perfect and an imperfect circuit of communication with the Leyden phial. The circuit of communication was divided into two parts. A bent rod of brass, with a ball of the same metal, $\frac{3}{4}$ of an inch in diameter, screwed on to its upper

extremity, and a copper ball, 5 inches in diameter, screwed on to the lower end, formed one of the parts. This part was supported by a stand of wood having a cap of brass at the top, into which the brass rod was occasionally screwed. The other part of the circuit consisted of a brass rod also, one end of which branched out in the form of a fork with two prongs, that pointed towards the centre of the copper ball; and those prongs were so constructed, that either of them could be made longer or shorter, just as the experiment required. On the end of one of the prongs was fixed a ball of brass, $\frac{3}{4}$ of an inch in diameter, and on the other a sharp steel point or needle. The shoulder of this fork screwed into a small plate of iron fixed on the inside of a wooden vessel, containing the greatest part of a cylindrical glass jar 12 inches and $\frac{3}{4}$ high, and about 4 inches in diameter. The coating of this glass (which was tin-foil) measured nearly 144 square inches on each surface. Besides this coating, part of the inside of the wooden vessel was coated also with tin-foil, for the purpose of making a secure communication between the iron plate and the outer coating of the jar. Within the jar itself was fitted a cylinder of wood, covered with tin-foil also, to make a communication between the inside coating of the glass and a brass rod fixed upright in the centre of the wooden cylinder. This upright rod having a ball of brass at the end, $\frac{3}{4}$ of an inch in diameter, was bent towards the first part of the circuit: so that the two balls being on a level, looked towards each other, but were placed from time to time at different distances, as occasion required, and thus answered the purpose of an electrometer.

The results of the several experiments, made by Dr. Lind, Mr. Cavallo, and Mr. Wilson, show, that in 23 experiments there was not any one instance where the ball was struck at a greater distance than the point, nor even at the same distance. It is remarkable, that in 2 or 3 experiments where the point was farther off than the ball, both the point and the ball were struck at the same time; which shows, that the influence of the point, though placed at a greater distance, was equal to the influence of the spherical termination placed considerably nearer.

On an application to Dr. Higgins, he favoured them with the use of his machine; the cylinder of which, when excited with the assistance of his amalgama, acted so powerfully, that it charged the jar, accompanying the new instrument, very readily. They began the experiments, where the electrometer was struck at the greatest distance, and then adjusted the distances of the ball and point from the copper ball accordingly; so that if the point was struck (when they were adjusted) the moving of the ball $\frac{1}{8}$ part of an inch would occasion the ball to be struck in preference to the point, and vice versâ. Afterwards they lessened the striking distance of the electrometer in every experiment till they attained the least distance. On reversing part of the apparatus, all those

xperiments were repeated again; the copper ball being put nearest to the glass instead of the forked part, and the forked part in the place of the copper ball. This set of experiments being completed, they made others, where the ball only was opposed; and after them, where the point only was opposed to the copper ball.

Having gone through all these experiments, they then repeated the experiment with the chain, after Mr. Henly's manner. The chain employed on this occasion was of iron, and very rusty, no other being then at hand. To avoid every objection, it was resolved on, that all the experiments they had made at Dr. Higgins's should be repeated, but with the two chains instead of the forked apparatus. On the 23d of June they went through the whole of the experiments thus circumstanced. The chains employed were brass, and a glass stand supported the ball and point. The cylinder measured about 13 inches in diameter: this glass, with the assistance of Dr. Higgins's amalgama, acted powerfully. All these experiments show that the point was struck at a greater distance than the ball.

XLV. Observations on the Solar Eclipse which happened June 24, 1778. By Mr. W. Wales, F. R. S., and Master of the Mathematical School in Christ's Hospital. p. 1013.

The following observations of the solar eclipse, which happened on the 24th instant, were made at the Royal Mathematical School in Christ's Hospital, where the latitude is $51^{\circ} 30' 55''$ N. and the longitude not quite half a second in time west of the cupola of St. Paul's. The time was taken by a most excellent watch made by Mr. Larcum Kendall, which goes while it is winding up, and has a provision for counteracting the effects of heat and cold. It was regulated by double altitudes of the sun's lower limb, taken from a basin of quicksilver with a Hadley's quadrant of Mr. Ramsden's making; and the quicksilver was shaded from the wind by a roof, formed by 2 glasses whose planes had been ground perfectly parallel by the same ingenious artist, so that the time may be depended on within a second, or 2 seconds at the most.

The telescope, which is of the Gregorian form, was made by the late Mr. Short; the focal length of the great speculum being 18 inches, and the aperture $4\frac{1}{8}$ inches. He used a magnifying power of about 75 times for the beginning and end, and of about 50 or 55 times with the micrometer, in measuring the sun's diameter, and the distances between the 2 cusps of the luminaries. The micrometer, which is an exceeding good one, was made also by Mr. Short. The divided glass is not achromatic, but only a single lens, whose focal length is about 28 feet $5\frac{1}{2}$ inches; but as he had not an opportunity of examining this point himself by adjusting the telescope to parallel rays

without the micrometer, and then putting it on, and measuring the distance at which objects are seen distinctly, he assumed the sun's apogeal diameter to be $31' 28''$, as given by Mr. Short; and, on that hypothesis, the reductions of the parts of the micrometer are made. Its error was determined immediately before the beginning of the eclipse, by measuring the angle subtended by a small ball which is on the top of the spire of St. Bride's church, in Fleet-street, alternately before and after O, or the beginning of the divisions of the scale: these measurements were as annexed:

Off the scale, or before o.		On the scale, or after o.		
Inch.	Ver.	Inch.	Ver.	
0.05	4	0.05	11	
0.05	4	0.05	11	
0.05	$3\frac{1}{2}$	0.05	12	
0.05	$4\frac{1}{2}$	0.05	$12\frac{1}{2}$	
0.05	5	0.05	15	
0.05	4.3	0.05	12.3	Mean on the scale.
		0.05	4.3	Mean off the scale.
		0.00	8.0	Difference.

Half the above difference, or 4 divisions of the vernier, $= 4''.83$, is the error of the micrometer, to be subtracted from the measured distances of the cusps, and also from the diameters of the sun, taken near the middle of the eclipse; in the same direction with the chords which were measured about the same time: and, this direction being nearly vertical, these measurements will, in some degree, be affected by refraction; but they may readily be corrected if the altitudes of the sun be computed to the times when they were taken, and thence the effect of the refractions.

The beginning of this eclipse was at $3^h 39^m 47^s$, apparent time, very exact; and the end at $5^h 25^m 1\frac{1}{2}^s$. The same mean diameter, at the time of the eclipse, was $31' 27''.8$.

XLVI. An Eclipse of the Sun, June 24, 1778; observed at Leicester. By the Rev. Mr. Ludlam, Vicar of Norton, near Leicester. p. 1019.

The beginning was observed at $3^h 35^m 27^s$; the end at $5^h 19^m 30^s$ or 34^s , according to the time shown by the clock, the sun being a little hazy at the end of the eclipse. But the clock being $1^m 22^s$ faster than the sun, the beginning will be $3^h 24^m 5^s$, and the end at $5^h 18^m 8^s$ or 12^s , solar time.

The difference between the meridians of Greenwich and Leicester, from observations in the Philos. Trans., computed by Mr. Wales, as below:

From solar eclipse June 3, 1769.....	}	Beginning	$4^m 24.5$
		End	$4 \quad 38.5$
ζ Tauri, April 28, 1770.....		Immersion	$4 \quad 27.8$
Aldebaran, Nov. 18, 1774.....		Emersion	$4 \quad 50.5$
Solar-eclipse, June 24, 1778.....	}	Beginning	$4 \quad 23.2$
		End	$4 \quad 41.3$

M. du Sejours, in the Memoirs of the Academy of Sciences for 1771, makes the difference of the meridians of Paris and Leicester, from the end of the solar eclipse of 1769, to be $13^m 59^s$; and the difference of the meridians of

Paris and Greenwich, both from the beginning and end of that eclipse, $9^m\ 20^s$. Whence the difference of the meridians of Greenwich and Leicester $4^m\ 39^s$. If we take the mean of all these computations, we shall have the difference between the meridian of the observatory at Greenwich and St. Martin's church in Leicester, $4^m\ 35^s$ of time very nearly.

XLVII. A ready way of Lighting a Candle, by a very moderate Electrical Spark. By John Ingenhousz, M. D., F. R. S. p. 1022.

It has been long known that an electrical spark will kindle spirit of wine, especially when previously warmed; and that vitriolic æther will be kindled by a very moderate spark, even when cold. However, I never saw an electrician who made a common use of this experiment to light his candle when he had occasion for it. The reason is, because though it may be done without much danger of failing in the attempt, yet it requires some trouble to prepare every thing necessary for making the experiment answer with certainty. Besides, æther is very precious, and is easily lost by evaporation before the electric power is excited, or before every thing is quite ready for performing the experiment.

I used to light my candle a good while ago by the explosion of a small jar (by small I understand one which has 8 or 10 inches of metallic coating, or even less) in the following manner. As I often amuse myself with electrical experiments, I have always an electrical machine, ready for action, fixed on a table in my room. When I have occasion to light a candle, I charge a small coated phial, whose knob is bent outwards, so as to hang a little over the body of the phial; I then wrap some loose cotton over the extremity of a long brass pin or a wire, so as to stick moderately fast to its substance. I next roll this extremity of the pin, wrapped up with cotton, in some fine powder of resin (which I always keep in readiness on the table for this purpose, either in a wide-mouthed phial or in a loose paper): this done, I apply the extremity of the pin or wire to the external coating of the charged phial, and bring as quickly as possible, the other extremity, wrapped round with cotton, to the knob: the powder of resin takes fire, and communicates its flame to the cotton, and both together burn long enough to light a candle. As I do not want more than half a minute to light my candle in this way, I find it a readier method than kindling it by flint and steel, or calling a servant.

I have found, that powder of white or yellow resin lights easier than that of brown. The farina lycopodii may be used for the same purpose; but it is not so good as the powder of resin, because it does not take fire quite so readily, requiring a stronger spark not to miss; besides, it is soon burnt away. By dipping the cotton in oil of turpentine, the same effect may be as readily obtained, if you take a jar somewhat greater in size. This oil will inflame so

much the readier if you strew a few fine particles of brass on it. The pin dust is the best for this purpose; but as this oil is scattered about by the explosion, and, when kindled, fills the room with much more smoke than the powder of resin, I prefer the last.

This experiment may be made use of for lighting a candle in the night as well as in the day. But for this purpose a charged phial should always be kept in readiness, and placed where it may be easily found in the dark. The jar for this purpose should be furnished in the manner invented by Mr. Cavallo, with a glass tube at the inside, reaching from the mouth of the phial to the bottom, through which tube the wire which establishes the communication with the inner coating passes, which, as soon as the phial is charged, is to be taken away, by holding it by the piece of sealing-wax, or glass rod covered with sealing-wax, fastened to the knob of the wire, which wire is only to be put into the glass tube again when the phial is to be discharged. Mr. Cavallo finds, that this jar will keep its charge a month, if the glass tube, and likewise the jar where it is not coated, are carefully lined with sealing-wax both within and without. A jar, containing 6 or 8 pints, fitted up in this manner, may be kept as a magazine of electrical fire, and a little coated phial, just large enough to light a candle, may be occasionally applied to it on purpose to light the powder of resin. As soon as this little phial is charged, which is done in an instant, the wire must be taken out of the large jar or magazine, to keep the remainder of the charge, which may serve afterwards for several charges of the little phial.

I have often carried in my pocket such a little jar a whole day, on purpose to fire a kind of pistol loaded with inflammable air in the manner described by Mr. Volta, of Como. A phial of about 2 ounces contained electrical fire enough to kindle such a pistol 20 times. In order to take out only as much of the electrical charge as was wanted for this purpose, I plunged into the glass tube of the charged phial a small glass tube, 4 inches long, adapted as a Leyden phial, by sticking in it at the bottom, which is hermetically sealed, a bit of tin-foil, an inch long, coiled up, and pasting a similar bit at the outside: a thin wire passed through the tube from the inside tin-foil to the opening, which was shut by a smooth brass ball stuck to it, and in contact with the said wire. The outside part of this tube, which was not coated with tin-foil, was lined or varnished with sealing-wax.

XLVIII. Electrical Experiments, to explain how far the Phenomena of the Electrophorus may be accounted for by Dr. Franklin's Theory of Positive and Negative Electricity; being the Annual Lecture instituted by the Will of Henry Baker, Esq., F. R. S. By John Ingenhousz, M. D., F. R. S. p. 1027.

Having had the honour of being appointed, by the President and Council of

the R. S., to read the annual dissertation on some philosophical subject, instituted by our worthy member the late Mr. Baker, I have endeavoured to pursue some electrical experiments, to explain how far the Electrophorus perpetuus may be accounted for on the almost generally received theory of Dr. Franklin, of positive and negative electricity.

This electrical instrument consists of 2 different pieces; viz. 1. a metallic body, in the form of a plate, or any other convenient figure, furnished with an insulating handle, to be used for lifting it up; and 2. a flat non-conducting substance, such as glass, resin, or some other non-conducting matter, on which the metal plate is placed. This machine, invented by Mr. Volta, a learned gentleman of Como, is certainly a valuable acquisition to the electrical apparatus. Once excited, it is for a long while ready to afford electricity enough for all experiments which do not require a very great force; and it has the advantage of not being so much affected by damp weather, as the common machines with glass globes, cylinders, disks, &c. It is very easily put in action by a slight friction with a dry hand, a piece of leather, a rough skin of a hare, a cat, or some other animal. It is as easy to excite, with this machine, a negative as a positive electricity. It has the advantage of being capable at almost all times of affording at pleasure such a force of electricity as is wanted, even to such a degree, that the metal plate is no longer able to contain all the electric fluid communicated to it; but throws it every way, either on the metal on which the resinous cake is usually fixed, or into the air: and this increase of electrical power is obtained by the easiest means; for instance, by charging with the electrophore a coated phial, and placing it afterwards on the resinous cake itself, or on the metal plate placed on the resinous cake (provided the metal plate be less in circumference than the resinous cake, and no metallic communication exist between the metal plate and that metal on which the cake is fastened.) If the knob of the phial, thus placed, be touched by the finger, and then taken away, holding it by the knob, the force of the electrophore is found to be remarkably increased.

But a more pleasing way of increasing the electrical force of this instrument is by transferring alternately the metal plate from one resinous cake to another, and touching it after it is placed on the cakes. By this method both cakes acquire continually more and more electricity; so that in a short time, by this alternate translation, the metal plate returns from either cake quite overcharged; and thus Leyden phials may be charged by it very strongly, and even so as to break them. It is very remarkable, that by this method the metal plate returns from one cake in a positive, and from the other in a negative state. This manner of increasing the two electricities was found out by my learned friend Dr. Klinkoch, professor in the university of Prague, soon after I had given him

a description of this new instrument, which I had received from his royal highness the archduke Ferdinand, a very little while after Mr. Volta had invented it.

It is true, that Father Beccaria had a long while ago excited an electricity, almost perpetual, by 2 panes of glass, one placed on the other, each having but one metal coating, and joined together so that no metal was placed between the 2 glasses. These 2 glasses being applied to a prime conductor of an electrical machine, so as to receive a charge in the same way as one glass coated on both sides is to be charged, afford numberless sparks from both coatings, after the 2 glasses have been discharged in the common way, by making a communication between the 2 coatings.

In order to produce these sparks, the 2 glasses must be separated from each other, so as to avoid touching the coatings in the moment of separation. Each of the coatings will give a spark, which may be repeated after having replaced the 2 glasses one on the other. After the 2 glasses have been thus joined again, and touched after their conjunction, another spark is obtained from both after their separation; and these sparks may be thus repeated almost ad infinitum; so that these 2 glasses, once excited, seem to be an unexhausted source of electrical sparks. Father Beccaria calls this experiment *electricitas vindex*. Whether this denomination be a proper one to convey some idea of what is understood by it, I will not now endeavour to determine. The same father Beccaria had also found, that the coating of a glass, after being discharged, was able to give new signs of electricity, when taken off by means of silk strings. Some other experiments were made, many years ago, by Mr. Cigna of Turin, and by some other electricians, which have a great deal of similarity with the electrophore.

But as the inventors of these experiments did not adapt them as an electrical machine, they do not diminish at all, in my opinion, the honour which Mr. Volta deserves, for having enriched the electrical apparatus with a very simple and handy machine, continually ready to excite as strong an electricity as is requisite for the more ordinary purposes. The novelty and simplicity of the machine could not but strike every electrician; I cannot express how much I was pleased with the first sight of it, and with what eagerness I set about endeavouring to understand the nature of it. I analyzed it in various ways: I compared it with Father Beccaria's *electricitas vindex*, with an ordinary coated glass, and coated resinous electrics.

Some electricians, puzzled with the strange phenomena which it affords, thought it over-turned entirely the almost universally received theory of Dr. Franklin, and that it could not be understood but by establishing new principles.

After considering the matter maturely, I thought that these phenomena,

though at first sight extraordinary, could be explained by the same principles which were already received by almost every philosopher.

But before proceeding to my intended explanation of the more obvious phenomena of the electrophorus, I must beg leave to set down some constant laws, which nature observes in the various motions of the electric fluid, and to which electricians do not seem to give a sufficient attention. 1. The electric fluid exists in all substances, in a certain quantity, which is natural to them. 2. The electric fluid is repulsive of itself, that is, each particle of electric fluid tends to recede as far from another particle of the same fluid as it can. 3. The state of electricity of a body, is that in which it has acquired more electrical fluid than the neighbouring bodies, or in which it has less of this fluid than the surrounding bodies. 4. In the first case, the electrical fluid tends to expand itself through all bodies near it, which can by their nature receive it. In the 2d case, the electrical fluid of all the surrounding bodies, finding less resistance towards a body negatively electrified, or having lost a part of its natural share of electricity, rushes towards that body, and tends to diffuse itself through it, and thus to dispose itself in an equilibrium.

5. The reason why the electric fluid, existing every where, seems to remain inactive in the common state of nature, is, because all other bodies having their ordinary share of this fluid, an equal pressure exists on all sides. Thus, if all the bodies on the earth were to acquire more or less of electric fluid at the same time, in equal proportions, no electrical phenomena would be the consequence of such a state; because the pressure being every where equal, the repulsive force of the electrical particles would be every where balanced. Thus two bodies, both negatively or both positively electrified, will not give a spark to each other: they only recede more from each other, because the other surrounding bodies are not in the same situation with them. This assertion seems to be illustrated by Father Beccaria's electrical well (*puteus electricus*;) which is nothing but a metal vessel electrified, in which two cork balls are suspended by silk threads; the balls do not show signs of electricity within the cavity of the vessel, because the electric fluid presses equally on every side.

6. All non-conducting bodies may acquire, on each part of their substance, more or less of the electric fluid, as well as conducting bodies, at least to a certain proportion; but they do not allow it to pass freely through their substance or over their surfaces.

7. All bodies whatever are susceptible of electricity, positive and negative indifferently, either by exciting them by friction or any other way, or by bringing them within the sphere of action of a body already electrical; so that even metals, the best conductors, may be as easily excited by friction, if insulated, as

glass or sealing wax. The only material difference between the conducting and non conducting substances seems to be, that the electricity does not spread itself so easily and so rapidly through or on those bodies which are non-conductors, as on those which are conductors. An electrical spark thrown on the surface of a piece of metal insulated, of whatever length it be, diffuses itself equally through the whole mass, if this metal be left to itself out of the sphere of action of another body charged with electricity. The whole electricity communicated by this spark is discharged at once by touching any part of the same metal. On the contrary, electricity seems rather to stick to that part of a non-conducting body to which it is applied, spreading but slowly and unequally over its surface, from which it may be taken by degrees, by touching those parts to which it was applied. There are some bodies which seem to be of a middle state between these two, viz. through which the electric fluid propagates as through good conductors, but slowly, such as common wood, moist air, and many other bodies. Electricity seems to diffuse itself through these bodies almost as sugar and salt diffuse themselves through water, spreading farther and farther through the liquid.

8. All those bodies which are non-conductors seem to acquire a state of electricity with some reluctance; and, after they have acquired it, to hold it more tenaciously, or to part with it more reluctantly than conductors. One touch takes away all the electricity of a metallic body, but does not absolutely convey away all the electricity of a piece of glass or another electric body, such as sealing wax, amber, &c. The metal plate of an electrophore takes hardly any electricity at all from the resinous cake, if it be lifted up without having been touched when it was on the cake.

9. All resinous bodies, silk and many others, retain more tenaciously their state of electricity than glass, however dry. Thus a piece of glass excited is almost quite deprived of its electricity by a conducting substance being applied to it; but a resinous body, though touched, retains still a great share of its electricity. 10. A conducting body insulated, being placed within the sphere of action of an excited non-conducting body, or even in contact with it, acquires at the same time 2 contrary electricities; viz. the part in contact, or very near the non-conducting electrified body, acquires a contrary electricity to that of the non-conducting body, at the same time that the opposite or farthest extremity is possessed of the same electricity with the non-conducting body. 11. A conducting body insulated, being in contact with another conducting body excited with either electricity, acquires the same electricity throughout its whole extension, or divides with this body its electricity equally. 12. But an insulated conducting body, being only in the sphere of action of another electrified conducting body, acquires, as in the first mentioned case, two different electricities,

at the same time; viz. towards the electrified body it acquires a contrary electricity, and at the opposite extremity it acquires the same kind of electricity with the electrified body.

It seems therefore to be a law of nature, that the electric fluid which is accumulated on a body, and finds an obstruction in its free passage to another neighbouring body by the interposition of a non-conducting body (such as dry air, glass, &c.) forces, by its repulsive power, the electric fluid, naturally contained in all bodies, to the farthest extremity of the neighbouring body, so as to excite in its nearest extremity a kind of defect of the electric fluid, or a kind of vacuum, till at last the accumulation of the electric fluid becomes so great on the electrified body, that it overpowers the resistance of the intermediate non-conducting substance, forces its way through it, and rushes in the form of a spark on the neighbouring body.

If the electric fluid be thrown on the surface of a pane of glass, coated on both sides with a metallic substance, such as tin foil; the fluid, finding an obstruction to its passage through the body, is crowded on that surface of the glass which has received it; forces the electric fluid out of the other surface, if some conducting body is near it, or in contact with it, and can convey it away; till this fluid becomes so much crowded on that surface as to overpower the resistance of the glass, and to force its way through the substance of the glass, in order to diffuse itself on the other surface, on which was produced a kind of vacuum. The glass, being thus rent, is no longer able to be what is called charged; but after the crowded electrical fluid of a prime conductor has in the same manner rent the plate of air (which obstructed to a certain degree its free passage between the prime conductor and the neighbouring body) by giving it a spark, the same spark may be repeated at pleasure, because the opening formed by the spark through the plate of air is immediately shut up again according to the nature of all fluids.

If an insulated conducting body be situated in the manner described, so as to possess at its different extremities a contrary electricity, it may impart to any other body brought in contact with it, or within its striking distance, a share of that electricity which it has acquired at its farther extremity. The former body, so touched, has effectually lost that part of electrical fluid which was in a certain manner crowded on that extremity; and therefore, being taken out of the sphere of action of the excited body, as, for instance, a prime conductor, after having thus lost a part of the electric fluid crowded on its extremity, is found to possess a negative electricity if the excited body had a positive, and a positive if the excited body had a negative one.

Thus we see how far we must believe what is commonly affirmed as a fact, that a body, plunged in the atmosphere of an electrified body, acquires a state of

electricity contrary to that of the electrified body. If the body plunged in this atmosphere be of a small extent, it will be found so to all appearance, because the two extremities of a small body cannot be separately examined; whereas a body of a certain extent exhibits in a very perceptible manner the two distinct electricities. The reason of this wonderful phenomenon is to be understood from the principles adopted, and may without much attention be understood if we suppose the excited body to be in a positive state of electricity; for in this case the atmosphere of electric fluid, surrounding the excited body, forces by its repulsive quality the electric fluid of the neighbouring body towards its farther extremity, and thus accumulates or crowds it on that extremity, from whence it is therefore ready to fly off on any other body, which is of a nature to receive it, being brought near enough.

If the excited body be in a state of negative electricity, the explanation is not so obvious as in the positive case: it requires some more attention to conceive what passes. The excited body, having lost a part of its natural share of electric fluid, a kind of vacuum, if I may call it so, takes place on this body. The electric fluid of any other body being in its natural state, and therefore in a kind of inactivity, confined as it were within its limits by the electric fluid of all the surrounding bodies, is set at liberty, exerts its natural repulsive quality towards that body, on which it does not find a similar quantity of electric fluid resisting its spring, or its elastic and repulsive quality; it therefore rushes towards that kind of vacuum which exists on a body negatively electrified; and thus the electric fluid of this body, losing its natural state of equilibrium, and accumulating itself towards the vacuum, produces there a real positive electricity, at the same time that the opposite extremity has a negative one.

I must speak one word more of that particular quality of conducting bodies, by which they receive, with a kind of reluctance, either state of electricity; and, after having received it, part with the same with as much seeming difficulty. This quality, not unknown to attentive electricians, who must have observed it, has commonly appeared somewhat extraordinary and difficult to be believed by many electricians, to whom I have happened to explain my theory of the electrophore. As this quality is the foundation of this theory, I conceive it will not be amiss to demonstrate it by facts. The first part of this inherent quality of non-conducting bodies, receiving a state of electricity with more difficulty than conducting bodies, is easily shown by the following simple experiment: a piece of dry glass, held near a prime conductor, will receive no electricity, or almost none, at the same distance as that at which a piece of metal or another conducting substance will have received a considerable degree of electricity, or even a full spark. The 2d part of this inherent quality may be thus demonstrated: a piece of metal insulated; as, for instance, the metal plate of an electrophore,

placed on the cake of resin excited with a considerable degree of electricity, will not receive any electricity at all, or only a faint one, when it is separated from the cake without having been touched when it was in contact with the cake, or in the sphere of action of the cake, though it was really in a state of actual electricity all the time it was on the plate. Now, if the cake of resin did part as easily with its state of electricity as the metal plate, it would leave a considerable degree of electricity on the metal plate; the more as it is well known that the metal does not at all resist the receiving of it.

Though it would be perhaps in vain to attempt a further explanation of this inherent quality of non-conducting bodies, yet it will be easy to illustrate this law of nature by an example of another inherent quality in all matter, which Sir Isaac Newton calls the *vis inertiae*; and is a *vis insita*, by which matter resists being put in motion, and when it is once put in motion requires as much force to stop its motion as it required to be brought from a state of rest to that of motion.

Let us now consider attentively the state of a body situated, as I have before described, in the sphere of action of an excited electric; as, for instance, a cake of resin, a flat glass, or any other non-conducting substance; or, in other words, let us consider the state of the metal plate placed on the resinous cake of an electrophore, supposing this cake to be excited with a positive electricity; which electricity it acquires easily by sliding the knob of a Leyden phial, charged in the common way, over its surface, and by various other ways. The superabundant electric fluid of the cake repels the electric fluid of the metal plate to its farther extremity, and excites there an accumulation of that fluid; or, in other words, produces there a positive electricity, while it produces a negative electricity at the surface in contact with the cake. If in this condition a conducting body be brought in contact with the metal plate, or within its striking distance, it receives a spark from it; which spark is the electric fluid of the metal plate crowded on the extremity of the metal by the repulsive force of the superabundant electric fluid of the cake. If the metal plate be touched at that side where it is really in a negative state, it will, notwithstanding, part with its accumulated positive electricity; because the repulsive power of the atmosphere of the cake will force this crowded electric fluid out of whatever part of the metal is touched, the electric fluid passing through metals very freely.

The metal plate, thus deprived of the electric fluid crowded upon it, becomes in a negative state; but the repellent power of the electric fluid of the cake continuing to act on the metal plate, forces what remains in it towards the farther extremity, so as to produce much the same state as it had before it was put on the cake; so that the negative state, in which it is in reality, cannot appear but when this metal is taken out of the pressing action of the atmosphere of the

cake; and therefore the metal plate being removed, by the insulating handle, from the cake, gives evident signs of having lost a part of its natural share of electric fluid; or, in other words, of being electrified negatively, the resinous cake being more tenacious of the state of electricity, which it had acquired, than the metal plate. If the resinous cake be in a state of negative electricity, (which it acquires by a friction either with a dry hand, a piece of leather, or a rough skin; or by sliding the negative part of a charged phial on it, or by many other ways) the contrary must happen, viz. the electric fluid of the metal plate, finding a kind of vacuum on the resin cake, rushes upon it, and thus leaves its opposite extremity in a negative case.

A conducting body, having its natural quantity of electric fluid, being brought near this metal plate, gives it a spark, which spark the metal plate retains as an additional quantity. If the metal plate be afterwards separated from the cake, it must retain this additional quantity which it has received from the approaching body; because the resinous cake being, from its nature, more tenacious of the state of electricity acquired than the metal, remains thereabout in the same condition as it was before the metal plate was placed on it; but the metal plate, having acquired an additional quantity in the time it was placed on the cake, carries with it this quantity, and must therefore return from the cake in a positive state. This confirms what was before said, that in the first case the cake of resin does not quit readily the electric fluid which it had acquired; and, in the 2d case, does not steal from the metal plate the electric fluid which it had lost. What happens to the metal plate placed on the resinous cake, happens also to the metal on which the resinous cake is commonly fixed; but the reverse must take place, that is, when the upper plate is taken off the cake in a positive state, the metal under the cake must be found in a negative state, if the electrophore be placed on an electrical stand.

It may be asked, what difference there is between an electrophore and a coated phial, or a flat glass coated on both sides and charged? I answer, that there is none at all, if both or only one of the metallic coatings can be taken off by silk strings, or a piece of sealing wax, or any other insulating substance. The very same day I received the electrophore sent to me by his royal highness the archduke Ferdinand from Milan (which electrophore was a thin resinous cake stuck on a flat piece of metal, to which was adapted a metal plate furnished with a glass handle to lift it up) I produced the same appearance by a common pane of glass and the metal plate of the electrophore; but soon finding that glass, however dry, quickly loses its electricity (probably from its easily attracting moisture from the air) I tried to cover it with a resinous substance, or to varnish it over with a hard copal varnish; by which means it was easily excited by fric-

tion, and retained a long while the electric power, though not so long as the resinous cake.

I will now explain the nature of an electrophore in a manner more familiar to electricians, who understand the received theory, by taking, instead of an electrophore, only a common pane of glass, adapted as a magic picture, with this difference only, that both coatings may be taken off by silk strings fastened to them, or by pieces of sealing wax. Having established a free communication between the common stock and the under coating, apply the upper coating to the prime conductor of an ordinary electrical machine, the pane of glass is charged in the common way. The prime conductor has forced a superabundant quantity of electric fluid on the surface next to it, by means of the coating, and as much is forced out of the opposite surface, and driven into the common stock. Open a metallic communication between the two coatings, and instantly the glass will be what is called discharged: and indeed it is so to all appearance; but if we examine more accurately what has happened, we shall find that the upper metallic coating has parted, by the discharge, with all the electric fluid which the prime conductor had forced on it, and even with that part of its own electric fluid which the repellent power of the superabundant electric fluid, communicated to that surface of the glass by the force of charging, had driven into it; and that the under coating has recovered as much as the glass had forced through it into the common stock, and has, above that, acquired or absorbed that additional quantity which that surface of the glass, being brought into a negative state, had drawn from the metal itself. And thus it will appear that the glass had, in the discharging, by no means parted with that state of electricity which it had acquired by the force of charging.

Now glass, and all electric substances, receiving with more difficulty a state of electricity, and parting with it with more reluctance, the consequence must be, that when the two coatings are separated from the glass, so as not to be in the way of absorbing or losing the electric fluid by other conducting bodies being near them, the upper coating, which was positive when the glass was charged, and nearly in its natural state, when after the discharge it remained in conjunction with the glass, must now give signs of a negative electricity, having lost by the discharge a share of even its natural electric fluid in the manner mentioned. The under coating, which was in a negative state when the glass was charged, and (like the other coating) in a natural state when after the discharge it remained in conjunction with the glass, must now, being separated from the glass, be in a positive state, because it had absorbed a quantity of electric fluid in the manner explained; which superabundant quantity it must take with it in the moment of separation from the glass, because the glass, being unwilling

easily to change its acquired state of electricity, leaves the metal without depriving it of that quantity of fluid which it had acquired.

If these two coatings, separated from the glass, are brought near each other, they attract each other; a spark ensues, because the coating, which has acquired a superabundant quantity of electric fluid, imparts it to the other, which had lost as much; and thus a perfect equilibrium is restored between them. If both these coatings be applied as before on the same glass, a positive spark may be obtained from the upper coating, and a negative one from the other. If they be separated again from the glass, as in the first case, the upper coating will afford a negative spark, and the under a positive; and these alternate sparks may be continued a very long time.

This explanation or theory agrees perfectly with the experiments exhibited by our deceased member, the late Mr. Canton, with elder pith balls hanging by linen threads from a wooden box, which balls are excited either negatively or positively by a piece of excited glass.

XLIX. Observations and Experiments tending to confirm Dr. Ingenhousz's Theory of the Electrophorus; and to shew the Impermeability of Glass to Electric Fluid. By William Henly, F. R. S. p. 1049.

Dr. Franklin has observed, "That there is a great quantity of the electrical fire in glass; that what it has it holds; and that it has as much as it can hold: that what is already in it, refuses or strongly repels any additional quantity: that when an additional quantity is applied to one surface of a phial (for instance by the atmosphere of an excited tube) a quantity is repelled or driven out of the inner surface of that side into the vessel, returning again into its pores, when the excited tube with its atmosphere is withdrawn; and that the particles of that atmosphere do not themselves pass through the glass." The following experiments remarkably illustrate this, by showing that bodies are very differently affected by a fluid acting immediately on them through glass; or by acting on them immediately by the glass, as abovementioned.

Exper.—A circular box, 3 or 4 inches in diameter, and $\frac{1}{4}$ of an inch deep, is furnished with a thin glass for a top. In this box scatter some very small steel filings, or sift them into it through a piece of writing paper, which has a number of holes pricked through it with a pin. Then apply one of the ends of a magnetic bar to the upper surface of the glass; the filings will be instantly attracted to the glass, and remain there as long as the magnet is thus suspended over them; but the moment it is removed, the filings fall to the bottom of the box, and there remain at rest. The glass then being made perfectly clean and warm, let a fine piece of amber, or sealing wax, &c. be strongly excited and applied to it, as the magnet was in the former experiment; the filings will be instantly in

motion, and will continue so for some seconds. When their motion ceases, withdraw the amber, &c. and the motion of the filings will be renewed, and continue as at first; this shows, that in both cases, they really act as conductors of the electric fluid between the lower surface of the glass and the bottom of the box, in order to restore an equilibrium, as on Dr. Faanklin's principles they ought to do; and that the electric fluid does not, like the magnetic, absolutely permeate the glass.

Exper.—Take a clean, dry, thin phial, about 4 inches long, and 1 inch in diameter. In the cork of this phial fix a small loop of very fine iron wire. In the loop suspend another wire, about $2\frac{1}{2}$ inches in length, by a similar loop; and on the lower end hang a light round ball of the pith of elder or cork, and be careful to give the wire as free a motion as possible. Let one of the ends of a small magnetic bar be brought near the side of the phial, then the little ball will instantly come to the glass, and there remain as long as the magnet is held within the distance of its influence. Remove the magnet, and the ball instantly retires to, and remains in the centre of the phial: then dry and warm the glass, and let an electric strongly excited be applied to the side of the phial, as the magnet was in the former experiment; the ball instantly comes to the side of the glass, and there remains some seconds, and then returns to the centre of the phial. Withdraw now the excited electric, and the ball instantly returns to the glass on the principle before-mentioned, which is more completely shown by the filings in the little box.

Exper.—Let a piece of thin glass be placed as a cover to a circular box, about 6 inches in diameter, and $\frac{3}{4}$ of an inch deep: put into the box 20 or 30 light balls of cork, or of the pith of elder; then, having made the glass very dry and warm, expose its surface to the electric matter issuing from the prime conductor to a good electrical machine; the balls will be instantly in motion, and will so continue for some time, the box being moved in such manner that every part of the glass may be affected. Then remove the box, and the balls being at rest, turn the glass, placing the upper surface downwards; the balls will then instantly renew their motion. When this 2d motion ceases, touch the surface of the glass near the centre with a finger, or, which is better, with a round, smooth piece of wood or metal, the balls will instantly fly to either of these, and will frequently pile themselves up between the glass and the bottom of the box, 8 or 10 in a pile, and will remove themselves, following the wood, &c. to different parts of the glass, till the charge is exhausted. Apply the glass again to the conductor as before, and when the motion of the balls nearly ceases, remove the glass, and place on each surface a circular coating of metal, reaching within an inch of the edge of the glass all round. Make a communication between these coatings, and the glass will then show that it has been charged, and will

give a very strong shock : this proves, that the electric matter did not absolutely pass through the glass, but only acted on the electricity inherent in it, in the manner explained by Dr. Franklin.

The direction of the electric matter, in the discharge of the Leyden bottle, has been shown in a variety of methods (see Philos. Trans. vol. 64 and 67 ;) but I shall here mention one which is a very curious addition to the number. Mr. Lullin, of Geneva, placed 2 wires, the one upon, the other under, a card, being about an inch from each other. This apparatus being made a part of the circuit, a charged bottle of a proper size was discharged through it : when the charge passed along the surface of the card, from the end of that wire into which it was discharged, till it came to the end of the other wire, and there pierced a hole through the card, passing by that wire to the negative side of the bottle ; and this happened whether the bottle was charged positively or negatively. A learned and ingenious correspondent of mine, the hon. Frederic Christian Mahling, counsellor of state at Copenhagen, has improved this experiment, by first painting the card in a line about half an inch broad on each surface with vermillion. The charge passing in this line (the card being previously well dried) shows its passage by a black mark on the vermillion, the mark being on one side of the card when the bottle is charged positively, and on the other side of it when the bottle is charged negatively. To which I would add, that a line of light is seen on one surface of the card through the whole space between the ends of the wires in one case, for instance, when the bottle is charged positively. But no light is seen in the other case, that is, when the bottle is charged negatively, till the electricity bursts a hole through the card to get at the wire which is in contact with the negative side of the bottle, as in this case the charge passes along the under surface of the card. If the card be placed vertically between 2 insulated wires, as in the universal discharger, described in Mr. Cavallo's Treatise on Electricity, the experiment may be made with great facility and certainty. The card may be fixed on a bit of sealing wax, or set in a piece of wood, sawn to a proper depth with a fine tenon saw.

L. Track of His Majesty's armed Brig Lion, from England to Davis's Streights and Labrador, with Observations for determining the Longitude by Sun and Moon and Error of Common Reckoning ; also the Variation of the Compass and Dip of the Needle, as observed during the said Voyage in 1776. By Lieut. Richard Pickersgill, late Commander of the said Vessel. p. 1057.

This is a copy of the commander's journal of the voyage, comprising, in different columns, the day and hour, the latitude, the longitude by the observation and the ship's reckoning, the temperature by the thermometer, the observed alti-

tude of the sun and moon, their observed distance, the azimuth, the variation, the winds, and miscellaneous remarks.

END OF THE SIXTY-EIGHTH VOLUME OF THE ORIGINAL.

I. On a Cure of the St. Vitus's Dance by Electricity. By A. Fothergill, M. D., F. R. S. p. 1. Vol. 69, Anno 1779.

Ann Agutter, a girl of 10 years of age, of a pale, emaciated habit, was admitted an out-patient at the Northampton Hospital on the 6th of June, 1778. From her father's account it appeared (for she was speechless and with difficulty supported from falling by 2 assistants) that she had for 6 weeks laboured under violent convulsive motions, which affected the whole frame, from which she had very short intermissions, except during sleep; that the disease had not only impaired her intellectual faculties, but of late had deprived her of the use of speech. Volatile and fetid medicines were recommended, and the warm bath every other night; but with no better success, except that the nights which had been restless became somewhat more composed. Blisters and antispasmodics were directed, and particularly the flowers of zinc, which were continued till the beginning of July, but without the least abatement of the symptoms; when her father being impatient at fruitless attendance at the hospital, Dr. F. recommended a trial of electricity, under the management of the Rev. Mr. Underwood. After this Dr. F. heard no more of her till the 1st of August, when her father came to inform him that his daughter was well, and desired she might have her discharge. To which, after expressing his doubts of the cure, he consented; but should not have been perfectly convinced of it, had he not received afterwards a full confirmation of it from Mr. Underwood, dated September 16, in the following extract.

“ July 5. On the glass-footed stool for 30 minutes: sparks were drawn from the arms, neck, and head, which caused a considerable perspiration, and a rash appeared in her forehead. She then received shocks through her hands, arms, breasts, and back; and from this time the symptoms abated, her arms beginning to recover their uses. July 13. On the glass-footed stool 45 minutes: received strong shocks through her legs and feet, which from that time began to recover their wonted uses; also 4 strong shocks through the jaws, soon after which her speech returned. July 23. On the glass-footed stool for the space of 1 hour: sparks were drawn from her arms, legs, head, and breast, which for the first time she very sensibly felt; also 2 shocks through the spine. She could now walk alone; her countenance became more florid, and all her faculties seemed

wonderfully strengthened, and from this time she continued mending to a state of perfect health. Every time she was electrified positively, her pulse quickened to a great degree, and an eruption, much like the itch, appeared in all her joints."

Thus far Mr. Underwood. To complete the history of this singular case, Dr. F. Oct. 28, 1778, rode several miles, on his return from the country, to visit her; and had the satisfaction to find her in good health, and the above account verified in every particular, with this addition, that at the beginning of the disease she had but slight twitchings, attended with running, staggering, and a variety of involuntary gesticulations which distinguish the St. Vitus's dance, and that these symptoms were afterwards succeeded by convulsions, which rendered it difficult for 2 assistants to keep her in bed, and which soon deprived her of speech and the use of her limbs. The eruptions which appeared on the parts electrified soon receded, without producing any return of the symptoms, and therefore could not be called critical, but merely the effect of the electrical stimulus. Having given her parents some general directions as to her regimen, &c. he took his leave, with a strong injunction to make him acquainted in case she should happen to relapse. Dr. F. adds that some time before, he was fortunate enough to cure a boy who had long had the St. Vitus's dance (though in a much less degree) by electricity. A violent convulsive disease, somewhat similar to the above, though he thinks not attended with aphonia, was successfully treated in the same way by Dr. Watson, and is recorded in the Phil. Trans. May we not then conclude, that these facts alone, and more might perhaps be produced, are sufficient to entitle electricity to a distinguished place in the class of antispasmodics?

II. A Case in which the Head of the Os Humeri was sawn off, and yet the Motion of the Limb preserved. By Mr. Daniel Orred, of Chester, Surgeon. p. 6.

This case (as Dr. Perceval, by whom it was communicated, remarks) affords a confirmation of an important fact, inserted in Phil. Trans. vol. 59, and shews that the chirurgical improvements there proposed by Mr. Charles White, may be extended to operations on other parts of the human body. But as this account, in all the leading circumstances of the case, as well as in the mode of operation, coincides with that of Mr. White, which may be perused in that author's Cases in Surgery, 8vo. 1770, it appeared unnecessary to print it here.

III. Experiments on some Mineral Substances. By Peter Woulfe, F. R. S. p. 11.

Omitted for the reason assigned at p. 120, vol. 14, of these Abridgments.

IV. Of a Petrefaction found on the Coast of East Lothian. By Edward King, Esq., F. R. S. p. 35.

In the year 1745 the Fox man of war was unfortunately stranded on the coast of East Lothian in Scotland, where she went to pieces; and the wreck remained about 33 years under water; but this last year a violent storm from the north-east laid a part of her bare, and several masses, consisting of iron, ropes, and balls, were found on the sands near the place, covered over with a very hard ochry substance, of the colour of iron, which adhered so strongly, that it required great force to detach it from the fragments of the wreck. And, on examination, this substance appeared to be sand, concreted and hardened into a kind of stone. The specimen now laid before the society had been taken out of the sea, from the same spot, some time before, and is a consolidated mass that had undergone the same change. It contains a piece of rope that was adjoining to some iron ring, and probably had been tied to it. The substance of the rope is very little altered; but the sand is so concreted round it, as to be as hard as rock, and it retains very perfectly impressions of parts of the ring, just in the same manner as impressions of extraneous fossil bodies are often found in various kinds of strata.

Now, considering these circumstances, we may fairly conclude, in the first place, that there is, on the coasts of this island, a continual progressive induration of masses of sand and other matter at the bottom of the ocean, somewhat in the same manner as there is at the bottom of the Adriatic sea, according to the account given by Dr. Donati, in the Phil. Trans. vol. 49, p. 588, or Abridg. vol. 10, p. 705. And, in the next place, it seems that iron, and the solutions of iron, contribute very much to hasten and promote the progress of the concretion and induration of stone, whenever they meet and are united with those cementing crystalline particles, which there is reason to believe are the more immediate cause of the consolidation of all stones and marbles whatever, and which very much abound in sea water.

Hence it may be inferred, that wherever there is any induration and petrefaction of matter, from any causes whatever, it is greatly hastened in its progress, and the consolidation is rendered much more complete and firm, by being near any mass of iron, and still more so by the admixture of any solution of that metal. This appears, in some degree, from the present specimen; where, near to the ring, and in the portion of the fragment that has the largest impression of it, the concreted sand-stone is of a firmer texture, and there is a larger cohering mass formed about that part of the rope, than about those parts that are farther removed from the ring. The same conclusion also may be drawn from a very remarkable piece of antiquity, which was discovered about 3 years since on the coast of Kent. Some fishermen, sweeping for anchors in the Gull

stream (a part of the sea near the Downs,) drew up a very curious old swivel gun, near 8 feet in length. The barrel of the gun, which was about 5 feet long, was of brass; but the handle, by which it was to be turned or traversed, which was about 3 feet in length, and also the swivel and pivot on which it turned, were of iron, and all round these latter, and especially about the swivel and pivot, were formed exceeding hard incrustations of sand, converted into a kind of stone, of an exceeding strong texture and firmness; whereas round the barrel of the gun, except where it was near adjoining to the iron, there were no such incrustations at all, the greater part of it being clean, and in good condition, just as if it had still continued in use.

The incrustation round the iron part of this gun was also the more deserving of attention, as it inclosed within it, and held fastly adhering to it on the outside, a number of shells and corallines, just in the same manner as they are often found in a fossil state. There were plainly to be distinguished, on the outside of this mass of incrustation, pectens, cockles, limpets, muscles, vermiculi marini and balani; and besides these, one buccinum and one oyster; and they were all so thoroughly and strongly fixed to it, and were also converted into such a hard substance, that it required as much force to separate or break them, as to break a fragment off any hard rock.

Dr. Fothergill also, who had communicated some very original conjectures on this subject to the society many years ago, mentioned some further curious facts. On passing through the streets of London in his walks, before the sign-irons were taken down, he perceived, that on the broad stone pavements, whenever he came just under any sign-irons, his cane gave a different sound, and occasioned a different kind of resistance to the hand, from what it did elsewhere; and attending more particularly to this circumstance, he found, that every where, under the drip of those irons, the stones had acquired a greater degree of solidity, and a wonderful hardness, so as to resist any ordinary tool, and gave, when struck on, a metallic sound: and this fact, by repeated observations, he was at length most thoroughly convinced of. Taking the hint therefore from hence, he made several experiments; and, among the rest, placed 2 pieces of Portland stone in the same aspect and situation in every respect, but washed the one frequently with water impregnated with rusty iron, and left the other untouched: and in a very few years he found the former had acquired a very sensible degree of that hardness before described, and on being struck gave the metallic sound; while the other remained in its original state, and subject to the decays occasioned by the changes of the weather, which we find in many instances make a most rapid progress. He also mentioned a very curious circumstance of his having found on the sea-coast near Scarborough, many years ago, part of a horse-shoe incrustated with sea sand, which was so concreted as to have

acquired the hardness of common grit stone, and retained the colour of the sand, with very little tincture of the iron ochre : and by the part which was left free from the incrustation it appeared most probable, that the horse-shoe had not been buried there many years, but had very recently acquired this incrustation on that part only that was most exposed to the washing of the sea water.

Now, all these facts put together scarce leave any doubt but that iron, and solutions of iron, greatly promote and hasten the progress of all kinds of petrefaction ; and therefore we may fairly infer, that whereas iron is of such manifest use in the progress of vegetation, that plants are indebted for their green colour, and for many of their valuable qualities, to its being intimately mixed in their substance ; and as it is obviously useful also in the animal system, and may be extracted by the magnet from the ashes of animal substances ; so it is no less useful in the consolidation of stones and marble in the fossil world. Mr. Pryce, in a very useful and curious treatise of mineralogy, has also lately shown it to be equally useful in the mineral world, by forming a proper nidus for the assemblage of the most valuable metals, and thus attracting and uniting them. This metal, therefore, seems to be almost universally one of the greatest bands that unites the several parts of matter, and one of the most useful and important of substances in the world.

If iron and the solutions of iron do thus contribute to the induration of bodies, such solutions must probably have that tendency in every stage of those bodies' existence ; and therefore it seems likely that the fine ornamental carvings in Portland, or other stone, might be much hardened, and preserved for a much longer time than has been usual, from the injuries of the weather, by being washed and brushed over by water, in which is infused a solution of iron. And perhaps even the softer kinds of stones might have been preserved by this means ; and the venerable remains of that fine pile of building, Henry the 7th's chapel, might have been saved from the destruction with which we now see it ready to be overwhelmed. It is very probable also, that common sea sand, with a very small admixture of a solution of iron, may at length, without any great expence, be converted into a useful species of stone, and be applied to the purpose of covering the fronts of houses even more durably, and in as beautiful a manner as some of the late invented stuccos ; and even those stuccos may be improved by means of the same mixture.

V. Account of Dr. Knight's Method of making Artificial Loadstones. By Mr. Benjamin Wilson, F. R. S. p. 51.

The method was this : having provided himself with a large quantity of clean filings of iron, Dr. K. put them into a large tub that was more than $\frac{1}{3}$ filled with clean water : he then, with great labour, worked the tub to and fro for many

hours together, that the friction between the grains of iron by this treatment might break off such smaller parts as would remain suspended in the water for some time: the obtaining of which very small particles in sufficient quantity seemed to him to be one of the principal desiderata in the experiment. The water being by this treatment rendered very muddy, he poured it into a clean earthen vessel, leaving the filings behind; and when the water had stood long enough to become clear, he poured it out carefully, without disturbing such of the iron sediment as still remained, which now appeared reduced almost to impalpable powder. This powder was afterwards removed into another vessel, in order to dry it; but as he had not obtained a proper quantity of it by this first step, he was obliged to repeat the process many times.

Having at last procured enough of this very fine powder, the next thing to be done was to make a paste of it, and that with some vehicle which would contain a considerable quantity of the phlogistic principle; for this purpose he had recourse to linseed oil in preference to all other fluids. With these 2 ingredients only he made a stiff paste, taking particular care to knead it well before he moulded it into convenient shapes. Sometimes, while the paste continued in its soft state, he would put the impression of a seal on the several pieces; one of which is in the British Museum. This paste was then put upon wood, and sometimes on tiles, in order to bake or dry it before a moderate fire, at about a foot distance. The doctor found, that a moderate fire was most proper, because a greater degree of heat made the composition frequently crack in many places.

The time required for the baking or drying of this paste was generally 5 or 6 hours, before it attained a sufficient degree of hardness. When that was done, and the several baked pieces were become cold, he gave them their magnetic virtue in any direction he pleased, by placing them between the extreme ends of his large magazine of artificial magnets for a few seconds or more, as he saw occasion. By this method the virtue they acquired was such, that when any one of those pieces was held between 2 of his best ten guinea bars, with its poles purposely inverted, it immediately of itself turned about to recover its natural direction, which the force of those very powerful bars was not sufficient to counteract.

VI. Account of an Extraordinary Dropsical Case. By Mr. John Latham. p. 54.

Miss A. was of a florid, lively constitution, but from a child was subject to a violent eruption, which came generally on a sudden, covering the whole neck, breast, and often great part of the face; and after remaining a week or 2 abated in violence, and went off by degrees. The intervals were uncertain, but for the most part in spring and in autumn she was more apt to have it, though frequently

3 or 4 times in the year. Various methods were tried to eradicate this complaint without effect; nor did the appearance of the menses, as there was reason to hope, in the least turn out in her favour. It will be needless to relate here the various medicines which had been given her with little or no success, except that the most relief she found was from the use of salt water, which was thought to make the intervals the longer in 2 or 3 instances, as well as the appearance of the eruption milder. Things continued thus till the autumn 1773, when the menses became obstructed, continuing so for some months, but appeared once more very plentifully; after which they never returned, neither did the eruption, except in the most trifling manner. About Christmas 1773 she complained of a weight in the abdomen, and fulness of the stomach; which symptoms were relieved by some gentle opening medicines. She then went on a visit to some friends at a distance, after which Mr. L. saw her no more for 2 months. He learned, that during that time the complaints had returned more violent, for which she consulted a physician on the spot, but without the relief she found at first; for the abdomen began to increase in size every day, and became painful, the urine high coloured, and in small quantity, with thirst, and every other symptom of an approaching dropsy.

In a narrative of this kind it might be expected, that a detail of the medicines she took during her illness might be noted; but Mr. L. chiefly acted in his surgical capacity, and as she was after this time, till the first operation, for the most part in London, under the care of physicians of the first eminence, it was out of his power to give such an account; suffice it then to say, that she was obliged to submit to the operation of the paracentesis the 27th of June, 1774. The quantity he then drew off was only 12 pints, somewhat fetid, but not very dark coloured, nor was it ever after the least offensive. The operation was repeated in 6 weeks, when 29 pints were taken off; after that time once in 4 weeks to the end of the year. During the whole of the year 1775, Mr. L. tapped her once in a fortnight more or less; and in the year 1776 she for the most part underwent the operation every 8 or 9 days, the intervals gradually shortening, till by the end of the year she could go no longer than a week between, which continued to the day of her death, which happened May 14, 1778, being then not quite 23 years of age. About a week before that time, she was troubled with incessant vomitings, which nothing would relieve; but was better a few hours before her death, and went off pretty easy.

Mr. L. had good reason to suppose the complaint originated from a disease of the left ovary, for after the first tapping he felt a substance of the size of a cricket ball; and, as the operations went on, this became more and more manifest, increasing so much as at last to occupy the whole space of the abdomen forward, being of a very irregular form, and he was clear of many pounds weight, for she

appeared, even after the water had been drawn off, as large as a woman in the last month of pregnancy. It would have added greatly to his satisfaction to have been able to clear up this point in every particular, by opening her after death; but he had the extreme mortification of being denied this necessary circumstance, notwithstanding his most earnest solicitations. He was therefore obliged to content himself with giving this bare recital of facts, which will serve to record to futurity a case, which he believes has not its equal in regard to the number of operations. It is remarkable that this young lady had a good appetite for the most part, and was very chearful; and, except a day before and after each operation, used to visit her friends at several miles distance as she would have done in health, and till within the last 2 or 3 months could walk a mile or 2 with tolerable ease.

As to the quantity of water drawn off, Mr. L. found it to amount to about 24 pints at each operation; for though the first time produced only 12 pints, and in several of the latter operations the quantity fell short of 24 pints, yet he might venture to state it at least at 24 pints or 3 gallons on an average, as in many of the operations he took off from 28 to 30 pints. The number of times he tapped her was in all 155, which brings out in the whole 3720 pints, being 465 gallons, not far short of $7\frac{1}{2}$ hogsheads.

VII. Problems concerning Interpolations. By Edw. Waring, M.D., F.R.S., and of the Institute of Bononia, Lucasian Prof. of Mathematics. p. 59.

Mr. Briggs was the first person it seems that invented a method of differences for interpolating logarithms at small intervals from each other: his principles were followed by Reginald and Mouton in France. Sir Isaac Newton, from the same principles, discovered a general and elegant solution of the above-mentioned problem: perhaps a still more elegant one on some accounts has been since discovered by Messrs. Nicole and Stirling. In the following theorems the same problem is resolved and rendered somewhat more general, without having any recourse to finding the successive differences.

Theorem 1. Assume an equation $a + bx + cx^2 + dx^3 \dots x^{n-1} = y$, in which the coefficients $a, b, c, d, e, \&c.$ are invariable; let $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ denote n values of the unknown quantity x , whose correspondent values of y let be represented by $s^\alpha, s^\beta, s^\gamma, s^\delta, s^\epsilon, \&c.$ Then will the equation $a + bx + cx^2 + dx^3 + ex^4 \dots x^{n-1} = y =$

$$\frac{x - \beta \cdot x - \gamma \cdot x - \delta \cdot x - \epsilon \cdot \&c.}{\alpha - \beta \cdot \alpha - \gamma \cdot \alpha - \delta \cdot \alpha - \epsilon \cdot \&c.} \times s^\alpha + \frac{x - \alpha \cdot x - \gamma \cdot x - \delta \cdot x - \epsilon \cdot \&c.}{\beta - \alpha \cdot \beta - \gamma \cdot \beta - \delta \cdot \beta - \epsilon \cdot \&c.} \times s^\beta \\ + \frac{x - \alpha \cdot x - \beta \cdot x - \delta \cdot x - \epsilon \cdot \&c.}{\gamma - \alpha \cdot \gamma - \beta \cdot \gamma - \delta \cdot \gamma - \epsilon \cdot \&c.} \times s^\gamma + \frac{x - \alpha \cdot x - \beta \cdot x - \gamma \cdot x - \epsilon \cdot \&c.}{\delta - \alpha \cdot \delta - \beta \cdot \delta - \gamma \cdot \delta - \epsilon \cdot \&c.} \times s^\delta + \&c.$$

Demonstration. Write α for x in the equation $y =$

$\frac{x - \beta . x - \gamma . x - \delta . x - \epsilon . \&c.}{\alpha - \beta . \alpha - \gamma . \alpha - \delta . \alpha - \epsilon . \&c.} \times s^{\alpha} + \frac{x - \alpha . x - \gamma . x - \delta . x - \epsilon . \&c.}{\beta - \alpha . \beta - \gamma . \beta - \delta . \beta - \epsilon . \&c.} \times s^{\beta} + \&c.;$ and all the terms except the first in the resulting equation will vanish, for each of them contains in its numerator a factor $x - \alpha = \alpha - \alpha = 0$; and the equation will become $y = \frac{\alpha - \beta . \alpha - \gamma . \alpha - \delta . \alpha - \epsilon . \&c.}{\alpha - \beta . \alpha - \gamma . \alpha - \delta . \alpha - \epsilon . \&c.} \times s^{\alpha} = s^{\alpha}$. In the same manner by writing $\beta, \gamma, \delta, \epsilon, \&c.$ successively for x , in the given equation, it may be proved, that when x is equal to $\beta, \gamma, \delta, \epsilon, \&c.$ then will y become respectively $s^{\beta}, s^{\gamma}, s^{\delta}, s^{\epsilon}$, which was to be demonstrated.

2. Assume $y = ax^r + bx^{r+s} + cx^{r+2s} + dx^{r+3s} \dots x^{r+(n-1)s}$; and when x becomes $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ let y become respectively $s^{\alpha}, s^{\beta}, s^{\gamma}, s^{\delta}, s^{\epsilon}, \&c.$; then will $y = \frac{x^r . x^s - \beta^s . x^s - \gamma^s . x^s - \delta^s . x^s - \epsilon^s . \&c.}{\alpha^r . \alpha^s - \beta^s . \alpha^s - \gamma^s . \alpha^s - \delta^s . \alpha^s - \epsilon^s . \&c.} \times s^{\alpha} + \frac{x^r . x^s - \alpha^s . x^s - \gamma^s . x^s - \delta^s . x^s - \epsilon^s . \&c.}{\beta^r . \beta^s - \alpha^s . \beta^s - \gamma^s . \beta^s - \delta^s . \beta^s - \epsilon^s . \&c.} \times s^{\beta} + \&c.$

This may be demonstrated in the same manner as the preceding theorem, by writing $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ successively for x .

Problem. Let there be n values, $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ of the quantity x , to which the n values $s^{\alpha}, s^{\beta}, s^{\gamma}, s^{\delta}, s^{\epsilon}, \&c.$ of the quantity y correspond; suppose these quantities to be found by any function x of the quantity x ; let $\pi, \rho, \sigma, \tau, \&c.$ be values of the quantities x , to which $s^{\pi}, s^{\rho}, s^{\sigma}, s^{\tau}, \&c.$ values of the quantity y correspond: for x substitute its above-mentioned values $\pi, \rho, \sigma, \tau, \&c.$ in the function x , and let the quantities resulting be $s^{\pi}, s^{\rho}, s^{\sigma}, s^{\tau}, \&c.$ not equal to the preceding $s^{\alpha}, s^{\beta}, s^{\gamma}, s^{\delta}, s^{\epsilon}, \&c.$ respectively: to find a quantity which, added to the function x , shall not only give the true values of the quantity y corresponding to the values $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ of the quantity x , but also corresponding to the values $\pi, \rho, \sigma, \tau, \&c.$ of the above-mentioned quantity x .

Assume $s^{\pi} - s^{\alpha} = T^{\pi}, s^{\rho} - s^{\beta} = T^{\rho}, s^{\sigma} - s^{\gamma} = T^{\sigma}, s^{\tau} - s^{\delta} = T^{\tau}, \&c.$; then the errors of the function x will be respectively $T^{\pi}, T^{\rho}, T^{\sigma}, T^{\tau}, \&c.$; and the correcting quantity sought may be

$$\frac{x - \alpha . x - \beta . x - \gamma . x - \delta . x - \epsilon . \&c.}{\pi - \alpha . \pi - \beta . \pi - \gamma . \pi - \delta . \pi - \epsilon . \&c.} \times \frac{x - \rho . x - \sigma . x - \tau . \&c.}{\pi - \rho . \pi - \sigma . \pi - \tau . \&c.} \times T^{\pi} \\ + \frac{x - \alpha . x - \beta . x - \gamma . x - \delta . x - \epsilon . \&c.}{\rho - \alpha . \rho - \beta . \rho - \gamma . \rho - \delta . \rho - \epsilon . \&c.} \times \frac{x - \pi . x - \sigma . x - \tau . \&c.}{\rho - \pi . \rho - \sigma . \rho - \tau . \&c.} \times T^{\rho} + \&c.$$

Alit. Let $x - \alpha . x - \beta . x - \gamma . x - \delta . x - \epsilon . \&c. . x - \pi . x - \rho . x - \sigma . x - \tau . \&c. = N$;
 $\pi - \alpha . \pi - \beta . \pi - \gamma . \pi - \delta . \pi - \epsilon . \&c. . \pi - \rho . \pi - \sigma . \pi - \tau . \&c. = \Pi$;
 $\rho - \alpha . \rho - \beta . \rho - \gamma . \rho - \delta . \rho - \epsilon . \&c. . \rho - \pi . \rho - \sigma . \rho - \tau . \&c. = P$;
 $\sigma - \alpha . \sigma - \beta . \sigma - \gamma . \sigma - \delta . \sigma - \epsilon . \&c. . \sigma - \pi . \sigma - \rho . \sigma - \tau . \&c. = \Sigma$;
 $\tau - \alpha . \tau - \beta . \tau - \gamma . \tau - \delta . \tau - \epsilon . \&c. . \tau - \pi . \tau - \rho . \tau - \sigma . \&c. = T, \&c.$;
then may the correcting quantity sought be

$$N \left(\frac{T^{\pi}}{\Pi(x - \pi)} + \frac{T^{\rho}}{P(x - \rho)} + \frac{T^{\sigma}}{\Sigma(x - \sigma)} + \frac{T^{\tau}}{T(x - \tau)} + \&c. \right)$$

This problem may be demonstrated in the same manner as the preceding

theorems, by writing for x , in the correcting quantity, successively its values π , ρ , σ , τ , &c.

2. For the correcting quantity sought may be assumed the quantity

$$\frac{x^s - \alpha^s \cdot x^s - \beta^s \cdot x^s - \gamma^s \cdot x^s - \delta^s \cdot \&c. \cdot x^s - \rho^s \cdot x^s - \sigma^s \cdot x^s - \tau^s \cdot \&c.}{\pi^s - \alpha^s \cdot \pi^s - \beta^s \cdot \pi^s - \gamma^s \cdot \pi^s - \delta^s \cdot \&c. \cdot \pi^s - \rho^s \cdot \pi^s - \sigma^s \cdot \pi^s - \tau^s \cdot \&c.} \times T^\pi$$

$$+ \frac{x^s - \alpha^s \cdot x^s - \beta^s \cdot x^s - \gamma^s \cdot x^s - \delta^s \cdot \&c. \cdot x^s - \pi^s \cdot x^s - \sigma^s \cdot x^s - \tau^s \cdot \&c.}{\rho^s - \alpha^s \cdot \rho^s - \beta^s \cdot \rho^s - \gamma^s \cdot \rho^s - \delta^s \cdot \&c. \cdot \rho^s - \pi^s \cdot \rho^s - \sigma^s \cdot \rho^s - \tau^s \cdot \&c.} \times T^\rho + \&c.$$

3. In general, let z be any quantity which is $= 0$, when x becomes either α , β , γ , δ , ϵ , &c.: let z become successively A , B , C , D , &c. when x becomes π , ρ , σ , τ , &c. respectively. When x is either $= \rho$, σ , τ , &c. let $\Pi = 0$; but if $x = \pi$, let $\Pi = p$: in the same manner when x is either $= \pi$, σ , τ , &c. let $P = 0$; but when $x = \rho$ let $P = r$: and similarly, let $\Sigma = 0$ when x is either π , ρ , τ , &c.; but when $x = \sigma$ let $\Sigma = s$: and likewise, when x is either π , ρ , σ , &c. let $T = 0$; but when $x = \tau$ let $T = t$: &c. then for the correcting quantity sought may be assumed $\frac{z}{A} \cdot \frac{\Pi}{p} \cdot T^\pi + \frac{z}{B} \cdot \frac{P}{r} \cdot T^\rho + \frac{z}{C} \cdot \frac{\Sigma}{s} \cdot T^\sigma + \frac{z}{D} \cdot \frac{T}{t} \cdot T^\tau + \&c.$

Theorem. Assume n quantities α , β , γ , δ , ϵ , &c. then will the sum of all the n quantities of the following kind

$$\frac{\alpha^m}{\alpha - \beta \cdot \alpha - \gamma \cdot \alpha - \delta \cdot \alpha - \epsilon \cdot \&c.} + \frac{\beta^m}{\beta - \alpha \cdot \beta - \gamma \cdot \beta - \delta \cdot \beta - \epsilon \cdot \&c.} + \frac{\gamma^m}{\gamma - \alpha \cdot \gamma - \beta \cdot \gamma - \delta \cdot \gamma - \epsilon \cdot \&c.}$$

$$+ \&c. = 0, \text{ if } m \text{ be any whole number less than } n - 1; \text{ but if } m = n - 1,$$

$$\text{then will the above-mentioned sum} = 1. \text{ In general, the sum of the } n \text{ terms}$$

$$\frac{\alpha^m (\beta\gamma\delta\&c. + \beta\gamma\epsilon\&c. + \beta\delta\epsilon + \gamma\delta\epsilon\&c. + \&c.)}{\alpha - \beta \cdot \alpha - \gamma \cdot \alpha - \delta \cdot \alpha - \epsilon \cdot \&c.} + \frac{\beta^m (\alpha\gamma\delta\&c. + \alpha\gamma\epsilon\&c. + \alpha\delta\epsilon\&c. + \gamma\delta\epsilon\&c. + \&c.)}{\beta - \alpha \cdot \beta - \gamma \cdot \beta - \delta \cdot \beta - \epsilon \cdot \&c.}$$

$$+ \&c. = 0, \text{ if } m \text{ be less than } n, \text{ and } m + r \text{ not equal to } n - 1; \text{ where } r \text{ is equal}$$

$$\text{to the number of letters contained in each of the contents above-mentioned,}$$

$$\beta\gamma\delta\&c. \beta\gamma\epsilon\&c. \beta\delta\epsilon\&c. \gamma\delta\epsilon\&c. \&c. \&c. \text{ respectively: but if } m + r = n - 1,$$

$$\text{then will the above-mentioned sum} = \pm 1; \text{ it will be } + 1 \text{ if } r \text{ be an even}$$

$$\text{number, otherwise } - 1.$$

VIII. On the Periodic Time of the Comet of the Year 1770. By J. A. Lexell, of the Petersburg Academy of Sciences. p. 68. From the Latin.

Mr. Lexell having, from the observations of this comet, computed its elements, and particularly its periodic time, which he states at $5\frac{1}{2}$ years, or nearer 5 years and 7 months; he proposes here, reversely, to show that these elements agree very well with the best observations that have been made of it; but the contrary if those elements be much varied. These elements, as Mr. L. has deduced them, are the following: viz. 1. The longitude of the ascending node $4^s 12^o 0'$; 2, the inclination of its orbit to the ecliptic $1^o 33' 40''$; 3, the elongation of the descending node from the perihelion $44^o 17' 4''$; and therefore the longitude of the perihelion $11^s 26^o 16' 26''$; 4, time of passing the perihelion in the year 1770, Aug. $13^d 15^h 5^m$ nearly, or Aug. 13.5450; 5, the comet's dis-

tance from the perihelion 0.6743815, its log. 9.8289057; 6, the semi-axis of the orbit described by the comet 3.1478606, its log. 0.4980155. Hence the log. of the semi-parameter is 0.0807300, and the log. of the excentricity 9.8952927, and therefore the periodic time 5.585 years.

From these elements, Mr. L. computes a great number of the comet's places by theory, and compares them with the actual observations that had been made; and he finds them commonly to agree within less than 1 minute, both in longitude and latitude. He next makes a small variation in the elements, and thence computes some of the comet's places by theory; thus, supposing the periodic time to be 5.6 years, the log. of the orbit's semi-parameter 0.0808000, the log. of the perihelion distance 9.8288794, time of the perihelion 13.5400 of August, longitude of the node $4^s\ 12^o\ 9'$, inclination of the orbit $1^o\ 33'\ 40''$, elongation from the node $44^o\ 7'\ 59''$; then several of the comet's places being calculated from these, they are found to agree nearly as well with the observations as the former.

Mr. L. next tries, by supposing the periodic time to be much greater, and thence deducing the other elements from 2 of the observations, whether by computation the other places will agree with observation. And first, supposing the periodic time to be 6 years, and the log. of the semi-parameter 0.0817000; then the other elements will be, viz. log. of the perihelion distance 9.8273218, time in the perihelion 13.2850 of August, longitude of the node $4^s\ 12^o\ 6'$, inclination of the orbit $1^o\ 34'\ 30''$, elongation of the perihelion from the node $44^o\ 9'\ 56''$, and hence he computes 5 of the comet's places, to compare them with the observations, which differ but little. Next assuming the log. of the semi-parameter 0.0818500, with the same periodic time 6 years, and thence computing the rest of the elements from 2 of the observations, and the other places from these; then the results from these 2 suppositions differ more than the former from the actual observations.

Again, making other suppositions with 7, &c. years for the periodic times, &c., the results are found, on trial, to come out still more irregular, and different from the observed places of the comet. In this way, trying several other numbers, some greater and some less than the former, and calculating from them, he finds still other deviations, more or less, from the actual observations. Hence he concludes that the list of elements first mentioned, are the most proper ones, as best agreeing with observation. After which the astronomer royal makes the following remark.

In a pamphlet of 18 pages in 4to., published at Upsal in 1776, Mr. Eric Prosperin, astronomer to the king of Sweden, has shown by his calculations, that the observations of near 4 months made on this comet by M. Messier could not be represented by a parabolic orbit; and thence founds a strong conjecture, and on the circumstances of the different parabolas which he found necessary to

represent the motion of the comet at different periods of time during its appearance, that its orbit may be sensibly elliptical, which it seems M. Pingre, who first calculated the orbit in a parabola, had also some suspicion of, and concludes with recommending the investigation of the true elements of its orbit in an ellipsis. The laborious calculation thus recommended has, we see, been since successfully and satisfactorily performed in this paper by Mr. Lexell.

N. MASKELYNE.

IX. On the General Resolution of Algebraic Equations. By Edw. Waring, M. D., F. R. S., &c. p. 86.

In the year 1757 I sent some papers to the R. S., which papers were printed in the year 1759, and copies of them delivered to several persons; these papers somewhat corrected, with the addition of a 2d part, on the properties of curve lines, were published in the year 1762. In the years 1767, 1768, and 1769, I printed, and published in the beginning of the year 1770, the same papers with additions and emendations, under the title of *Meditationes Algebraicæ*. In these papers were contained, with many other inventions, the most general resolution of algebraic equations known, as it contains the resolution of every algebraic equation, of which the general resolution has been given, viz. the resolution of quadratic, cubic and biquadratic, the resolution of Mr. de Moivre's and Mr. Bezout's, since published, equations; it discovers the resolution of an equation of n dimensions, of which the n roots are given, and also deduces innumerable equations of n dimensions, which contain $n - 1$ independent coefficients. Whence it seems probable, that this new method of mine may contain the most general resolution of algebraic equations that ever has, or perhaps ever will be invented.

The general resolution is $x = a \sqrt[n]{p} + b \sqrt[n]{p^2} + c \sqrt[n]{p^3} + d \sqrt[n]{p^4} \dots + r \sqrt[n]{p^{n-3}} + s \sqrt[n]{p^{n-2}} + t \sqrt[n]{p^{n-1}} + \frac{A}{n}$, if the equation be $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3} + Dx^{n-4} - \&c. = 0$.

I shall add the resolution of some particular equations from this method, and then subjoin the equation to which $x = a \sqrt[n]{p} + b \sqrt[n]{p^2} + c \sqrt[n]{p^3} + \&c.$ is the general resolution.

1. Let the resolution be $x = a \sqrt[3]{p} + b \sqrt[3]{p^2}$: then the correspondent equation free from radicals will be found $x^3 - 3abpx - a^3p - b^3p^2 = 0$. Let $x^3 - px - a = 0$ be a cubic equation whose resolution is required, which suppose the same as the equation found above, and consequently their correspondent terms equal, i. e. $p = 3abp$, and $a = a^3p + b^3p^2$; whence $p = \frac{p}{3ab}$, which value being substituted for p in the 2d equation, there results $a = \frac{pa^2}{3b} + \frac{bp^2}{9a^2}$. In this equation for a or b may be assumed unity, or any other quantity whatever, and there will

result an equation of the formula of a quadratic, from which the other b or a may be found; whence from the equation ($p = \frac{P}{3ab}$) may be deduced, and consequently the resolution of the cubic required.

In the same manner, for p may be assumed any quantity whatever, and in the equation $a = a^3p + b^3p^2$ for b substitute its value $\frac{P}{3ap}$, or for a its value $\frac{P}{3bp}$, and there result the equations $a = a^3p + \frac{P^3}{27a^3p}$, and $a = \frac{P^3}{27b^3p^2} - b^3p^2$, which have the formula of a quadratic, from which may be deduced the resolution of the cubic required.

2. Let the resolution assumed be $x = a\sqrt[4]{p} + b\sqrt[4]{p^2} + c\sqrt[4]{p^3}$; exterminate the irrational quantities, and there results the equation $x^4 - (2b^2 + 4ac)px^2 - 4(a^2bp + bc^2p^2)x - a^4p + b^4p^2 - c^4p^3 + 2a^2c^2p^2 - 4ab^2cp^2 = 0$; suppose $p = 1$, and the given equation $x^4 + qx^2 - rx + s = 0$; then let the correspondent terms of the given and resulting equations be respectively made equal to each other, and there result the 3 equations $2b^2 + 4ac = -q$, and $4b(a^2 + c^2) = r$, and $a^4 - b^4 + c^4 - 2a^2c^2 + 4ab^2c = -s$; reduce these equations into one, so that the unknown quantities a and c may be exterminated, and there results the equation $4b^6 + 2qb^4 + (\frac{q^2}{4} - s)b^2 - \frac{r^2}{16} = 0$ of the formula of a cubic, from which the unknown quantity b may be found, which being substituted for its value (b) in the preceding equations, from the equations thence ensuing may be found the unknown quantities a and c , and consequently the resolution of the given biquadratic $x^4 + qx^2 - rx + s = 0$.

From the same principles can be deduced different resolutions of the above-mentioned biquadratic $x^4 + qx^2 - rx + s = 0$.

3. (1st) Let $x = a\sqrt[2n]{p} + b\sqrt[2n]{p^2}$; then will the equation free from radicals be $x^{2n} - 2b^np^{n-1}x^{n-1} - \frac{n}{1.2} \cdot 2nb^{n-1}a^2p^{n-1}x^{n-2} - \frac{n \times (n^2 - 1)}{1.2.3.4} \times 2nb^{n-2}a^4p^{n-2}x^{n-3} - \frac{n \times (n^2 - 1) \times (n^2 - 4)}{1.2.3.4.5.6} \times 2nb^{n-3}a^6p^{n-3}x^{n-4} - \frac{n \cdot (n^2 - 1) \cdot (n^2 - 4) \cdot (n^2 - 9)}{1.2.3.4.5.6.7.8} \times 2nb^{n-4}a^8p^{n-4}x^{n-5} - \dots - \frac{n \cdot (n^2 - 1) \cdot (n^2 - 4) \cdot (n^2 - 9) \cdot (n^2 - 16) \cdot [n^2 - (n-2)^2]}{1.2.3.4.5.6.7 \dots (2n-2)} \times 2na^{2n-2}bpx = a^np - b^np^2$.

This equation may be deduced from the following principles. Let $\alpha, \beta, \gamma, \delta, \epsilon$, &c. be the $2n$ roots of the equation $x^{2n} - 1 = 0$, then, by prop. 23 of my Meditat. Algebraicæ, the equation free from radicals will be the product of the following quantities $(x - a\alpha\sqrt[2n]{p} - b\alpha^{2n}\sqrt[2n]{p^2}) \cdot (x - a\beta\sqrt[2n]{p} - b\beta^{2n}\sqrt[2n]{p^2}) \cdot (x - a\gamma\sqrt[2n]{p} - b\gamma^{2n}\sqrt[2n]{p^2}) \cdot (x - a\delta\sqrt[2n]{p} - b\delta^{2n}\sqrt[2n]{p^2}) \cdot (x - a\epsilon\sqrt[2n]{p} - b\epsilon^{2n}\sqrt[2n]{p^2})$ &c. $= 0$: multiply these quantities into each other, and from the resulting product, by prob. 3 of the Meditat. Algebr. easily can be deduced the equation free from radicals which was to be found.

3. (2d). Let $x = a\sqrt[2n+1]{p} + b\sqrt[2n+1]{p^2}$; then will the correspondent equa-

tion free from radicals be $x^{2n+1} - (2n+1)b^na^np^x - \frac{n(n+1)}{1.2.3} \times (2n+1)b^{n-1}a^3px^{n-1} - \frac{n \times (n^2-1) \times (n+2)}{1.2.3.4.5} \times (2n+1)b^{n-2}a^5px^{n-2} - \frac{n \times (n^2-1) \times (n^2-4) \times (n+3)}{1.2.3.4.5.6.7} \times (2n+1)b^{n-3}a^7px^{n-3} - \frac{n(n^2-1) \cdot (n^2-4) \cdot (n^2-9) \cdot (n+4)}{1.2.3.4.5.6.7.8.9} \times (2n+1)b^{n-4}a^9px^{n-4} - \dots - \frac{n(n^2-1) \cdot (n^2-4) \cdot (n^2-9) \cdot (n^2-16) \cdot [n^2 - (n-2)^2] \times (2n-1)}{1.2.3.4.5.6.7.8.9 \dots (2n-1)} \times (2n+1)ba^{2n-1}px = a^{2n+1}p + b^{2n+1}p^2.$

This may be derived from the same principles as the preceding.

3. (3d) In general, let the equation be $x = a^m\sqrt[p]{p} + b^m\sqrt[p]{p^2}$, then will the equation free from radicals become $x^m - ma^{m-2}bpx - m \cdot \frac{m-3}{2} a^{m-4}b^2px^2 - m \cdot \frac{m-4}{2} \cdot \frac{m-5}{3} a^{m-6}b^3px^3 - m \cdot \frac{m-5}{2} \cdot \frac{m-6}{3} \cdot \frac{m-7}{4} a^{m-8}b^4px^4 - m \cdot \frac{m-6}{2} \cdot \frac{m-7}{3} \cdot \frac{m-8}{4} \cdot \frac{m-9}{5} a^{m-10}b^5px^5 - \&c. = a^mp \pm b^mp^2$; if m denotes an even number, it will be $-b^mp^2$, but if an odd number, it will be $+b^mp^2$.

4. (1st) Let n denote an odd number, and $x = a^n\sqrt[p]{p} + b^n\sqrt[p]{p^3}$; then will $x^n - p(na^{n-3}bx^2 + n \cdot \frac{n-5}{2} a^{n-6}b^2x^4 + n \cdot \frac{n-7}{2} \cdot \frac{n-8}{3} a^{n-9}b^3x^5 + n \cdot \frac{n-9}{2} \cdot \frac{n-10}{3} \cdot \frac{n-11}{4} a^{n-12}b^4x^8 + n \cdot \frac{n-11}{2} \cdot \frac{n-12}{3} \cdot \frac{n-13}{4} \cdot \frac{n-14}{5} a^{n-15}b^5x^{10} + n \cdot \frac{n-13}{2} \cdot \frac{n-14}{3} \cdot \frac{n-15}{4} \cdot \frac{n-16}{5} \cdot \frac{n-17}{6} a^{n-18}b^6x^{12} + n \cdot \frac{n-15}{2} \cdot \frac{n-16}{3} \cdot \frac{n-17}{4} \cdot \frac{n-18}{5} \cdot \frac{n-19}{6} \cdot \frac{n-20}{7} a^{n-21}b^7x^{14} + \&c.) \pm p^2(na^{\frac{n-3}{2}}b^{\frac{n+1}{2}}x - \frac{1}{2^2} \times n \cdot \frac{n-5}{2} \cdot \frac{n-7}{3} a^{\frac{n-9}{2}}b^{\frac{n+3}{2}}x^3 + \frac{1}{2^4} \times n \cdot \frac{n-7}{2} \cdot \frac{n-9}{3} \cdot \frac{n-11}{4} \cdot \frac{n-13}{5} a^{\frac{n-15}{2}}b^{\frac{n+5}{2}}x^5 - \frac{1}{2^6} \times n \cdot \frac{n-9}{2} \cdot \frac{n-11}{3} \cdot \frac{n-13}{4} \cdot \frac{n-15}{5} \cdot \frac{n-17}{6} \cdot \frac{n-19}{7} a^{\frac{n-21}{2}}b^{\frac{n+7}{2}}x^7 + \frac{1}{2^8} \times n \cdot \frac{n-11}{2} \cdot \frac{n-13}{3} \cdot \frac{n-15}{4} \cdot \frac{n-17}{5} \cdot \frac{n-19}{6} \cdot \frac{n-21}{7} \cdot \frac{n-23}{8} \cdot \frac{n-25}{9} a^{\frac{n-27}{2}}b^{\frac{n+9}{2}}x^9 - \&c.) = a^np + b^np^3.$

The quantity $\pm p^2$ denotes $+p^2$ if $\frac{n-3}{4}$ is a whole number, otherwise $-p^2$.

4. (2d) Let n denote an even number, and x as before $= a^n\sqrt[p]{p} + b^n\sqrt[p]{p^3}$; then will $x^n - p(na^{n-3}bx^2 + n \cdot \frac{n-5}{2} a^{n-6}b^2x^4 + n \cdot \frac{n-7}{2} \cdot \frac{n-8}{3} a^{n-9}b^3x^5 + n \cdot \frac{n-9}{2} \cdot \frac{n-10}{3} \cdot \frac{n-11}{4} a^{n-12}b^4x^8 + n \cdot \frac{n-11}{2} \cdot \frac{n-12}{3} \cdot \frac{n-13}{4} \cdot \frac{n-14}{5} a^{n-15}b^5x^{10} + \&c.) \pm p^2(\frac{1}{2}n \cdot \frac{n-4}{2} a^{\frac{n-6}{2}}b^{\frac{n+2}{2}}x^2 - \frac{1}{2^3} \times n \cdot \frac{n-6}{2} \cdot \frac{n-8}{3} \cdot \frac{n-10}{4} a^{\frac{n-12}{2}}b^{\frac{n+4}{2}}x^4 + \frac{1}{2^5} \times n \cdot \frac{n-8}{2} \cdot \frac{n-10}{3} \cdot \frac{n-12}{4} \cdot \frac{n-14}{5} \cdot \frac{n-16}{6} a^{\frac{n-18}{2}}b^{\frac{n+6}{2}}x^6 - \frac{1}{2^7} \times n \cdot \frac{n-10}{2} \cdot \frac{n-12}{3} \cdot \frac{n-14}{4} \cdot \frac{n-16}{5} \cdot \frac{n-18}{6} \cdot \frac{n-20}{7} \cdot \frac{n-22}{8} a^{\frac{n-24}{2}}b^{\frac{n+8}{2}}x^8 + \&c.) = a^np + b^np^3 \pm 2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2.$

The quantities $\pm p^2$ and $\pm 2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$ denote $+p^2$, and $+2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$, if $\frac{n-2}{4}$ is a whole number, otherwise they denote $-p^2$ and $-2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$ respectively.

5. (1st) Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-1}}$, and n an odd number; then will $x^n - nabpx^{n-2} + n \cdot \frac{n-3}{2} a^2b^2p^2x^{n-4} - n \cdot \frac{n-4}{2} \cdot \frac{n-5}{3} a^3b^3p^3x^{n-6} + n \cdot \frac{n-5}{2} \cdot \frac{n-6}{3} \cdot \frac{n-7}{4} a^4b^4p^4x^{n-8} - \&c. = a^np + b^np^{n-1}$.

5. (2d) Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-1}}$, and n an even number; then will $x^n - nabpx^{n-2} + n \cdot \frac{n-3}{2} a^2b^2p^2x^{n-4} - n \cdot \frac{n-4}{2} \cdot \frac{n-5}{3} a^3b^3p^3x^{n-6} + n \cdot \frac{n-5}{2} \cdot \frac{n-6}{3} \cdot \frac{n-7}{4} a^4b^4p^4x^{n-8} - \&c. = a^np \mp 2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2 + b^np^{n-1}$. It will be $+2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$ if $n = 4r + 2$; but $-2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$ if $n = 4r$.

6. (1st) Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-2}}$, and n an odd number, which has not the number 3 for a divisor; then will $x^n - na^2bpx^{n-3} + n \cdot \frac{n-5}{2} a^4b^2p^2x^{n-6} - n \cdot \frac{n-7}{2} \cdot \frac{n-8}{3} a^6b^3p^3x^{n-9} + n \cdot \frac{n-9}{2} \cdot \frac{n-10}{3} \cdot \frac{n-11}{4} a^8b^4p^4x^{n-12} - n \cdot \frac{n-11}{2} \cdot \frac{n-12}{3} \cdot \frac{n-13}{4} \cdot \frac{n-14}{5} a^{10}b^5p^5x^{n-15} + n \cdot \frac{n-13}{2} \cdot \frac{n-14}{3} \cdot \frac{n-15}{4} \cdot \frac{n-16}{5} \cdot \frac{n-17}{6} a^{12}b^6p^6x^{n-18} - \&c.$ (to m terms, where m is the number either equal to, or the least greater than $\frac{n}{3}$) $- ab^2p^2 (nx^2 + \frac{1}{4}n \cdot \frac{n-5}{2} \cdot \frac{n-7}{3} a^2bpx^2 + \frac{1}{2^4}n \cdot \frac{n-7}{2} \cdot \frac{n-9}{3} \cdot \frac{n-11}{4} \cdot \frac{n-13}{5} a^4b^2p^2x^2 + \frac{1}{2^6} \times n \cdot \frac{n-9}{2} \cdot \frac{n-11}{3} \cdot \frac{n-13}{4} \cdot \frac{n-15}{5} \cdot \frac{n-17}{6} \cdot \frac{n-19}{7} a^6b^3p^3x^2 + \frac{1}{2^8} \times n \cdot \frac{n-11}{2} \cdot \frac{n-13}{3} \cdot \frac{n-15}{4} \cdot \frac{n-17}{5} \cdot \frac{n-19}{6} \cdot \frac{n-21}{7} \cdot \frac{n-23}{8} \cdot \frac{n-25}{9} a^8b^4p^4x^2 + \&c.) = A = a^np + b^np^{n-2}$.

Let n be an odd number divisible by 3, then will the above-mentioned quantity $= A = a^np + b^np^{n-2} + 3a^{\frac{n}{3}}b^{\frac{2n}{3}}p^{\frac{n}{3}-1} + 3a^{\frac{2n}{3}}b^{\frac{n}{3}}p^{\frac{n}{3}}$.

6. (2d) Let n be an even number, not divisible by 3; then will $x^n - na^2bpx^{n-3} + n \cdot \frac{n-5}{2} a^4b^2p^2x^{n-6} - n \cdot \frac{n-7}{2} \cdot \frac{n-8}{3} a^6b^3p^3x^{n-9} + n \cdot \frac{n-9}{2} \cdot \frac{n-10}{3} \cdot \frac{n-11}{4} a^8b^4p^4x^{n-12} - n \cdot \frac{n-11}{2} \cdot \frac{n-12}{3} \cdot \frac{n-13}{4} \cdot \frac{n-14}{5} a^{10}b^5p^5x^{n-15} + n \cdot \frac{n-13}{2} \cdot \frac{n-14}{3} \cdot \frac{n-15}{4} \cdot \frac{n-16}{5} \cdot \frac{n-17}{6} a^{12}b^6p^6x^{n-18} - \&c.$ to m terms as before $- b^2p^2 (2x^2 + \frac{1}{2}n \cdot \frac{n-4}{2} a^2bpx^2 + \frac{1}{2^3} \times n \cdot \frac{n-6}{2} \cdot \frac{n-8}{3} \cdot \frac{n-10}{4} a^4b^2p^2x^2 + \frac{1}{2^5} \times n \cdot \frac{n-8}{2} \cdot \frac{n-10}{3} \cdot \frac{n-12}{4} \cdot \frac{n-14}{5} \cdot \frac{n-16}{6} a^6b^3p^3x^2 + \frac{1}{2^7} \times n \cdot \frac{n-10}{2} \cdot \frac{n-12}{3} \cdot \frac{n-14}{4} \cdot \frac{n-16}{5} \cdot \frac{n-18}{6} \cdot \frac{n-20}{7} \cdot \frac{n-22}{8} a^8b^4p^4x^2 + \&c.) = A = a^np + b^np^{n-2}$.

Let n be an even number divisible by 3, then will the above-mentioned quantity $A = a^n p - b^n p^{n-2} - 3a^{\frac{2n}{3}} b^{\frac{n}{3}} p^{\frac{n}{3}} + 3a^{\frac{n}{3}} b^{\frac{2n}{3}} p^{\frac{n}{3}-1}$.

In all the preceding cases n , m and r denote whole affirmative numbers.

These equations may be deduced in the same manner as is before given in case 3 (1st); or can be demonstrated by writing, in the equation free from radicals, for the different powers of x , their values deduced from the given equation $x = a^{\frac{m}{n}} p + b^{\frac{m}{n}} p^n$. To render the solution general, it may not be improper to subjoin the subsequent.

Lemma. 1. Let $\alpha, \beta, \gamma, \delta, \epsilon, \zeta$, &c. be the respective roots of the equation $z^n - 1 = 0$; then will $\alpha^m + \beta^m + \gamma^m + \delta^m + \epsilon^m + \zeta^m + \&c. = 0$, unless $n = m$, or n is a divisor of m , in which case $\alpha^m + \beta^m + \gamma^m + \delta^m + \epsilon^m + \zeta^m + \&c. = n$.

2. The sum of all quantities of the following kind $\alpha^m \beta^r + \alpha^r \beta^m + \alpha^m \gamma^r + \alpha^r \gamma^m + \beta^m \gamma^r + \beta^r \gamma^m + \alpha^m \delta^r + \&c.$ will be $= 0$; unless n be either equal to, or a divisor of $m + r$, in which case the sum above-mentioned will be $= -n$; except n be either equal to m or r , or a divisor of them, in which case the sum will be $n^2 - n$; but if $m = r$, then in the former case will the above-mentioned sum $= -\frac{n}{2}$, and in the latter $= \frac{n^2 - n}{2}$.

3. The sum of all quantities of this kind $\alpha^m \beta^r \gamma^s \delta^t + \alpha^r \beta^m \gamma^s \delta^t + \alpha^m \beta^r \gamma^t \delta^s + \alpha^r \beta^m \gamma^t \delta^s + \&c.$ will be $= 0$, unless n be either equal to $r + m + s + t + \&c.$ or a divisor of it.

Let π be the number of indices m, r, s, t , &c. and n be either equal to $m + r + s + t + \&c.$ or a divisor of it, but n be neither equal to, nor a divisor of the sum of any two, three, or four, $\dots \pi - 3, \pi - 2$, or $\pi - 1$ of the above-mentioned quantities; then will the sum above-mentioned $= \mp 1.2.3.4 \dots (\pi - 2) \cdot (\pi - 1) \times n$; where it will be $+$, if π be an odd number, otherwise $-$. In this case, if a indices be m , b indices be r , c indices be s , d indices be t , &c. then will the above-mentioned sum $= \mp \frac{1.2.3.4 \dots (\pi - 2) \times (\pi - 1)}{1.2.3 \dots a \times 1.2.3 \dots b \times 1.2.3 \dots c \times 1.2.3 \dots d \times \&c.} \times n$.

Let n be either equal to, or a divisor of the sum of any number ρ (less than π) of the above-mentioned quantities m, r, s, t , &c. and consequently either equal to, or a divisor of the sum of the $(\pi - \rho)$ remaining quantities: find the sum of all possible quantities of this kind $1.2.3 \dots (\rho - 2) \times (\rho - 1) \times 1.2.3 \dots (\pi - \rho - 2) \times (\pi - \rho - 1) \times n^2$, which sum call A .

Let n be either equal to, or a divisor of the sum of any number (σ) of the above-mentioned quantities m, r, s, t , &c.; and also equal to, or a divisor of the sum of any number (ρ') of the remaining quantities, and consequently it will be either equal to, or a divisor of, the sum of the $(\pi - \rho' - \sigma)$ remaining quantities; then find the sum of all possible quantities of this sort $1.2.3 \dots (\sigma - 2) \times (\sigma - 1) \times 1.2.3 \dots (\rho' - 2) \times (\rho' - 1) \times 1.2.3 \dots (\pi - \rho' - \sigma - 2) \times (\pi - \rho' - \sigma - 1) \times n^3$, which sum call B .

In the same manner let n be either equal to, or a divisor of the sum of any number (τ) of the above-mentioned quantities $m, r, s, t, \&c.$; and similarly let n be either equal to, or a divisor of the sum of any number (σ') of the remaining quantities; and also let n be either equal to, or a divisor of the sum of any number (ρ'') of the remaining quantities; then will n be either equal to, or a divisor of the sum of the $(\pi - \tau - \sigma' - \rho'')$ remaining quantities: find the sum of all quantities of this sort $1.2.3...(\tau - 2) \times (\tau - 1) \times 1.2.3...(\sigma' - 2) \times (\sigma' - 1) \times 1.2.3...(\rho'' - 2) \times (\rho'' - 1) \times 1.2.3...(\pi - \tau - \sigma' - \rho'' - 2) \times (\pi - \tau - \sigma' - \rho'' - 1) \times n^4$, which sum call c , and so on; then will the above-mentioned sum $\alpha^m \beta^r \gamma^s \delta^t \&c. + \alpha^r \beta^m \gamma^s \delta^t \&c. + \alpha^m \beta^r \gamma^t \delta^s \&c. + \alpha^r \beta^t \gamma^m \delta^s \&c. + \&c. = \mp (1.2.3...(\pi - 2) \times (\pi - 1) \times n - A + B - C + D - \&c.)$ where it will be $+$ if π be an odd number, otherwise $-$. In this case, if a indices be m , b indices be r , c indices be s , d indices be t , $\&c.$ then will the above-mentioned sum $= \mp \frac{1.2.3...(\pi - 2) \times (\pi - 1) \times n - A + B - C + D - \&c.}{1.2.3...a \times 1.2.3...b \times 1.2.3...c \times 1.2.3...d \times \&c.}$

7. Let $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ be the roots of the equation $z^n - 1 = 0$, and the resolution be $x = a \sqrt[n]{p} + b \sqrt[n]{p^2} + c \sqrt[n]{p^3} + d \sqrt[n]{p^4} \dots + h \sqrt[n]{p^\lambda} \dots + k \sqrt[n]{p^\mu} \dots + l \sqrt[n]{p^\nu} \dots + q \sqrt[n]{p^\xi} \dots + r \sqrt[n]{p^\phi} \dots + s \sqrt[n]{p^{n-4}} + t \sqrt[n]{p^{n-3}} + v \sqrt[n]{p^{n-2}} + u \sqrt[n]{p^{n-1}}$; then will the different values of x be respectively $a \sqrt[n]{p} \times \alpha + b \sqrt[n]{p^2} \times \alpha^2 + c \sqrt[n]{p^3} \times \alpha^3 + d \sqrt[n]{p^4} \times \alpha^4 \dots + h \sqrt[n]{p^\lambda} \times \alpha^\lambda \dots + k \sqrt[n]{p^\mu} \times \alpha^\mu \dots + s \sqrt[n]{p^{n-4}} \times \alpha^{n-4} + t \sqrt[n]{p^{n-3}} \times \alpha^{n-3} + v \sqrt[n]{p^{n-2}} \times \alpha^{n-2} + u \sqrt[n]{p^{n-1}} \times \alpha^{n-1}$;

$a \sqrt[n]{p} \times \beta + b \sqrt[n]{p^2} \times \beta^2 + c \sqrt[n]{p^3} \times \beta^3 + d \sqrt[n]{p^4} \times \beta^4 \dots + h \sqrt[n]{p^\lambda} \times \beta^\lambda \dots + k \sqrt[n]{p^\mu} \times \beta^\mu \dots + s \sqrt[n]{p^{n-4}} \times \beta^{n-4} + t \sqrt[n]{p^{n-3}} \times \beta^{n-3} + v \sqrt[n]{p^{n-2}} \times \beta^{n-2} + u \sqrt[n]{p^{n-1}} \times \beta^{n-1}$;

$a \sqrt[n]{p} \times \gamma + b \sqrt[n]{p^2} \times \gamma^2 + c \sqrt[n]{p^3} \times \gamma^3 + d \sqrt[n]{p^4} \times \gamma^4 \dots + h \sqrt[n]{p^\lambda} \times \gamma^\lambda \dots + k \sqrt[n]{p^\mu} \times \gamma^\mu \dots + s \sqrt[n]{p^{n-4}} \times \gamma^{n-4} + t \sqrt[n]{p^{n-3}} \times \gamma^{n-3} + v \sqrt[n]{p^{n-2}} \times \gamma^{n-2} + u \sqrt[n]{p^{n-1}} \times \gamma^{n-1}$;

$a \sqrt[n]{p} \times \delta + b \sqrt[n]{p^2} \times \delta^2 + c \sqrt[n]{p^3} \times \delta^3 + d \sqrt[n]{p^4} \times \delta^4 \dots + h \sqrt[n]{p^\lambda} \times \delta^\lambda \dots + k \sqrt[n]{p^\mu} \times \delta^\mu \dots + s \sqrt[n]{p^{n-4}} \times \delta^{n-4} + t \sqrt[n]{p^{n-3}} \times \delta^{n-3} + v \sqrt[n]{p^{n-2}} \times \delta^{n-2} + u \sqrt[n]{p^{n-1}} \times \delta^{n-1}$;

&c.

&c.

&c.

&c.

and consequently the sum of the values or roots, which is the coefficient of the 2d term of the equation sought, will be $a \sqrt[n]{p} \times (\alpha + \beta + \gamma + \delta + \&c. (o)) + b \sqrt[n]{p^2} \times (\alpha^2 + \beta^2 + \gamma^2 + \delta^2 + \&c. (o)) + c \sqrt[n]{p^3} \times (\alpha^3 + \beta^3 + \gamma^3 + \delta^3 + \&c. (o)) + \dots + v \sqrt[n]{p^{n-2}} \times (\alpha^{n-2} + \beta^{n-2} + \gamma^{n-2} + \delta^{n-2} + \&c. (o)) + u \sqrt[n]{p^{n-1}} \times (\alpha^{n-1} + \beta^{n-1} + \gamma^{n-1} + \delta^{n-1} + \&c. (o)) = 0$.

The sum of the products of every 2 of the values or roots, which is the coefficient of the 3d term of the equation sought, will be $a^2 \sqrt[n]{p^2} \times (\alpha\beta + \alpha\gamma + \beta\gamma + \alpha\delta + \beta\delta + \gamma\delta + \&c. (o)) + ab \sqrt[n]{p^3} \times (\alpha\beta^2 + \beta\alpha^2 + \alpha\gamma^2 + \gamma\alpha^2 + \beta\gamma^2 + \gamma\beta^2 + \alpha\delta^2 + \delta\alpha^2 + \&c. (o))$; and in general all the terms will be 0, unless $a \times u$

$\times p \times [\alpha\beta^{n-1} + \beta\alpha^{n-1} + \alpha\gamma^{n-1} + \gamma\alpha^{n-1} + \beta\gamma^{n-1} + \gamma\beta^{n-1} + \alpha\delta^{n-1} + \delta\alpha^{n-1} + \beta\delta^{n-1} + \delta\beta^{n-1} + \gamma\delta^{n-1} + \delta\gamma^{n-1} + \&c. (-n)] + b \times v \times p \times [\alpha^2\beta^{n-2} + \beta^2\alpha^{n-2} + \alpha^2\gamma^{n-2} + \gamma^2\alpha^{n-2} + \beta^2\gamma^{n-2} + \gamma^2\beta^{n-2} + \alpha^2\delta^{n-2} + \delta^2\alpha^{n-2} + \beta^2\delta^{n-2} + \delta^2\beta^{n-2} + \&c. (-n)] + ct p \times [\alpha^3\beta^{n-3} + \beta^3\alpha^{n-3} + \alpha^3\gamma^{n-3} + \gamma^3\alpha^{n-3} + \beta^3\gamma^{n-3} + \gamma^3\beta^{n-3} + \alpha^3\delta^{n-3} + \delta^3\alpha^{n-3} + \beta^3\delta^{n-3} + \delta^3\beta^{n-3} + \&c. (-n)] + dsp \times [\alpha^4\beta^{n-4} + \beta^4\alpha^{n-4} + \alpha^4\gamma^{n-4} + \gamma^4\alpha^{n-4} + \beta^4\gamma^{n-4} + \gamma^4\beta^{n-4} + \&c. (-n)] + \&c. = -np (au + bv + ct + ds + \&c.)$

If $n = 2\lambda$, then will the coefficient of h^2p be $\frac{1}{2}n$, i. e. the above-mentioned coefficient will be $-np (au + bv + ct + ds + \dots + \frac{1}{2}h^2)$.

The sum of the contents of every 3 of the above-mentioned values or roots, which is the coefficient of the 4th term of the equation required, will be $a^3 \sqrt[n]{p^3} \times [\alpha\beta\gamma + \alpha\beta\delta + \alpha\gamma\delta + \beta\gamma\delta + \&c. (o)] + a^2b \sqrt[n]{p^4} \times [\alpha\beta\gamma^2 + \alpha\gamma\beta^2 + \beta\gamma\alpha^2 + \alpha\beta\delta^2 + \&c. (o)] + \&c. + a^2v \sqrt[n]{p^n} \times [\alpha\beta\gamma^{n-2} + \alpha\gamma\beta^{n-2} + \beta\gamma\alpha^{n-2} + \alpha\beta\delta^{n-2} + \alpha\delta\beta^{n-2} + \&c. (\frac{1.2.n}{1.2})] + abt \sqrt[n]{p^n} \times [\alpha\beta^2\gamma^{n-3} + \alpha\gamma^2\beta^{n-3} + \beta\alpha^2\gamma^{n-3} + \beta\gamma^2\alpha^{n-3} + \gamma\alpha^2\beta^{n-3} + \gamma\beta^2\alpha^{n-3} + \alpha\beta^2\delta^{n-3} + \&c. (1.2.n)] + \&c.$

And in general, all the terms (unless the quantity $\sqrt[n]{p^q}$ contained in the term have this formula $\sqrt[n]{p^n} = p$, or $\sqrt[n]{p^{2n}} = p^2$) will be $= 0$; let the general term be denoted by $hkh \sqrt[n]{p^{\lambda+\mu+\nu}} \times (\alpha^\lambda\beta^\mu\gamma^\nu + \alpha^\lambda\beta^\nu\gamma^\mu + \alpha^\mu\beta^\lambda\gamma^\nu + \alpha^\mu\beta^\nu\gamma^\lambda + \alpha^\nu\beta^\lambda\gamma^\mu + \alpha^\nu\beta^\mu\gamma^\lambda + \alpha^\lambda\beta^\mu\delta^\nu + \&c.)$; first let $\lambda + \mu + \nu$ neither be equal to n nor $2n$, then will the term above-mentioned $= 0$; if it be equal to n or $2n$, then will the term be $1.2 \times n \times hklp$ or $1.2nhklp^2$. If 2 of the 3 indexes λ, μ, ν be equal to each other, then divide the above-mentioned term by 1.2; if the 3 indexes be equal, i. e. $\lambda = \mu = \nu$, divide it by 1.2.3: find all quantities of this kind where $\lambda + \mu + \nu$ either is equal to n or $2n$, and add all the terms thence derived, and call the sum of them A.

The sum of the contents of every 4 of the values or roots above-mentioned, which is the coefficient of the 4th term of the equation required, will be $a^4 \sqrt[n]{p^4} \times [\alpha\beta\gamma\delta + \alpha\beta\gamma\delta + \&c. (o)] + a^3b \sqrt[n]{p^5} \times [\alpha\beta\gamma\delta^2 + \alpha\beta\gamma^2\delta + \alpha\beta^2\gamma\delta + \alpha^2\beta\gamma\delta + \&c. (o)] + \&c.$: let $hklq \sqrt[n]{p^{\lambda+\mu+\nu+\xi}} \times \alpha^\lambda\beta^\mu\gamma^\nu\delta^\xi + \alpha^\lambda\beta^\mu\gamma^\xi\delta^\nu + \alpha^\lambda\beta^\nu\gamma^\mu\delta^\xi + \alpha^\lambda\beta^\nu\gamma^\xi\delta^\mu + \alpha^\lambda\beta^\xi\gamma^\nu\delta^\mu + \alpha^\lambda\beta^\xi\gamma^\mu\delta^\nu + \alpha^\mu\beta^\lambda\gamma^\nu\delta^\xi + \&c.)$ denote a general term; this term will be $= 0$, unless $\lambda + \mu + \nu + \xi$ either $= n$ or $2n$ or $3n$; in which case the term will be either $-1.2.3nhklqp$ or $-1.2.3nhklqp^2$ or $-1.2.3nhklqp^3$; unless $\lambda + \mu = \nu + \xi = n$, when the above-mentioned term will be $-(1.2.3n - n^2)hklqp^2$; in this case if $\lambda = \nu$, and consequently $\mu = \xi$, then it will be $-(1.2.3n - 1.2n^2)hklqp^2$; but if $\lambda = \mu = \nu = \xi = \frac{1}{2}n$, then will the term be $-(1.2.3n - 3n^2)hklqp^2$. In all these cases, if 2 of the indexes λ, μ, ν, ξ be equal, then must the term given above be divided by 1.2; if 3, by 1.2.3; if 4, by 1.2.3.4; and lastly if 2 are equal to each other, and the 2 remaining indexes equal to each other, but not to the former 2, then must the term aforesaid be divided by 1.2.1.2. Find the sum of all the possible terms of this kind, which call B.

In the same manner from the preceding lemma may be found the aggregates of the contents of every 5, 6, 7, &c. roots or values multiplied into each other,

which call respectively $c, d, e, \&c.$: then will the equation required be $x^n * - np (au + bv + ct + ds + \&c.) x^{n-2} - Ax^{n-3} + Bx^{n-4} - Cx^{n-5} + Dx^{n-6} - \&c. = 0$.

From the same principles may be deduced the most general reduction yet known of equations to others of inferior dimensions, *e. g.* Let $(x) x^n + (A + a\sqrt[m]{p} + b\sqrt[m]{p^2} + c\sqrt[m]{p^3} + \dots + s\sqrt[m]{p^{m-2}} + t\sqrt[m]{p^{m-1}}) x^{n-1} + (B + a'\sqrt[m]{p} + b'\sqrt[m]{p^2} + \dots + s'\sqrt[m]{p^{m-2}} + t'\sqrt[m]{p^{m-1}}) x^{n-2} + (C + a''\sqrt[m]{p} + b''\sqrt[m]{p^2} + \&c.) x^{n-3} + \&c. = 0$; let $\alpha, \beta, \gamma, \delta, \&c.$ be the respective roots of the equation $z^m - 1 = 0$; then, from the principles before given, may be formed the different values of the equation x , which being multiplied into each other, from the propositions before-mentioned of the *Meditationes Algebraicæ*, may be deduced an equation of mn dimensions free from radicals, whose root is x , and which contains mn unknown quantities $A, a, b, c, \&c. B, a', b', c', \&c. C, a'', b'', c'', \&c. p$: for one, two or more of these unknown quantities may be assumed any quantities whatever, and thence may be deduced equations of mn dimensions, which may be reduced to equations $x^n + (A + a\sqrt[m]{p} + b\sqrt[m]{p^2} + c\sqrt[m]{p^3} + \&c.) x^{n-1} + \&c. = 0$ of n dimensions.

In the same manner may be assumed equations, which involve $\sqrt[m]{p}, \sqrt[m]{p^2}, \dots, \sqrt[m]{p^{m-1}}; \sqrt[m]{q}, \sqrt[m]{q^2}, \sqrt[m]{q^3}, \dots, \sqrt[m]{q^{m-1}}; \sqrt[m]{r}, \sqrt[m]{r^2}, \sqrt[m]{r^3}, \dots, \sqrt[m]{r^{m-1}}, \&c.$; and from so reducing them as to exterminate the irrational quantities, may often be derived equations whose resolutions or reductions are known.

The method of transforming algebraical equations into others, whose roots bear any assignable algebraical (but not exponential) relation to the roots of a given algebraical equation, first published by me in the papers sent to the R. S., and afterwards in the year 1760; and 3dly in my *Miscellanea Analytica*; and lastly in the *Meditationes Algebraicæ*, and since published by Mr. Le Grange in the Berlin Acts, is perhaps, as Mr. Le Grange observes, more general than Mr. Hudde's, or any transformation yet invented; it is very useful in the resolution of numerous problems; and further has this peculiar advantage over all other transformations yet invented, that it often easily discovers some of the first terms of the equation required, from which many elegant theorems may be derived.

In the works above-mentioned, viz. *Miscell. Analyt. Medit. Algeb. &c.* are given some problems serving to this transformation; the first of which is a series which, from the coefficients of a given algebraical equation $(x^n - px^{n-1} + qx^{n-2} - \&c. = 0)$ finds the sum of any power of the roots (viz. $\alpha^m + \beta^m + \gamma^m + \delta^m + \&c.$ where $\alpha, \beta, \gamma, \delta, \&c.$ denote the roots of the given equation), the law of which series was published by me many years before it was given by Mr. Euler. The 3d problem, often mentioned in this paper, is an elegant and useful series for finding the sum of quantities of the following kind, viz. $\alpha^m \beta^r \gamma^s \delta^t \&c. + \alpha^r \beta^m \gamma^s \delta^t \&c. + \alpha^s \beta^m \gamma^r \delta^t \&c. + \alpha^m \beta^s \gamma^r \delta^t \&c. + \&c.$

Mr. Euler gave the following resolution, $x = \sqrt[n]{\pi} + \sqrt[n]{\rho} + \sqrt[n]{\sigma} + \sqrt[n]{\tau} + \&c.$ where $\pi, \rho, \sigma, \tau, \&c.$ denote the roots of an equation of $n - 1$ dimensions $v^{n-1} - pv^{n-2} + qv^{n-3} - \&c. = 0$. It is evident, that in this case the equation whose root is x will have n^{n-1} dimensions; for let the roots of the equation $z^n - 1 = 0$ be denoted by $\alpha, \beta, \gamma, \delta, \&c.$ then will the quantity $\sqrt[n]{\pi}$ have the n following values $\alpha \sqrt[n]{\pi}, \beta \sqrt[n]{\pi}, \gamma \sqrt[n]{\pi}, \&c.$ and the same may be affirmed of the quantities $\sqrt[n]{\rho}, \sqrt[n]{\sigma}, \sqrt[n]{\tau}, \&c.$ and consequently the quantity $\sqrt[n]{\pi} + \sqrt[n]{\rho}$ will have $n \times n$ different values; and in the same manner the root $x = \sqrt[n]{\pi} + \sqrt[n]{\rho} + \sqrt[n]{\sigma} + \sqrt[n]{\tau} + \&c.$ may be proved to contain $n \times n \times n \times n \times \&c. = n^{n-1}$ roots; and consequently in this resolution, in equations of superior dimensions, the number of independent coefficients ($n - 1$) will be very few in proportion to the number of dimensions n^{n-1} , or (if we respect its formula) n^{n-2} of the resulting equation.

Let $n = 3$; then the equation resulting will rise to an equation of 9 dimensions, which has the formula of a cubic; for let $x = \sqrt[3]{\pi} + \sqrt[3]{\rho} = a$ one root, then will $\frac{-1 + \sqrt{-3}}{2} a$ & $\frac{-1 - \sqrt{-3}}{2} a$ be 2 other of the 9 roots, and consequently the roots will be $x^3 - a^3 \times x^3 - b^3 \times x^3 - c^3 = 0$, which has the formula of a cubic; and in general the above-mentioned equation of n^{n-1} dimensions will, for the same reason, have the formula of an equation of n^{n-2} dimensions.

Let the resolution be $x = \sqrt[n]{\pi} + \sqrt[n]{\rho} + \sqrt[n]{\sigma} + \sqrt[n]{\tau} + \&c.$ where $\pi, \rho, \sigma, \tau, \&c.$ denote the roots of an equation $x^{n-1} - px^{n-2} + qx^{n-3} - \&c. = 0$ of $(n-1)$ dimensions; then will the resulting equation free from radicals, whose root is x , rise to 2^{n-1} dimensions; but as every affirmative has a negative root equal to it, it will have the formula of an equation of 2^{n-2} dimensions.

Let the resolution be of this formula $x = r^{+m} \sqrt[r+m]{\alpha} + r^{+m} \sqrt[r+m]{\beta} + r^{+m} \sqrt[r+m]{\gamma} + r^{+m} \sqrt[r+m]{\delta} + \&c.$ if $\alpha, \beta, \gamma, \delta, \&c.$ be considered as the r power of the roots of an equation of s dimensions, then will the resulting equation, of which the resolution is given, rise only to an equation of the formula of m^{s-1} dimensions. In the year 1762 I published some reasons, for which this method could not extend to the general resolution of algebraical equations.

XI. Observations on the Total (with Duration) and Annular Eclipse of the Sun, taken June 24, 1778, on Board the Admiral's Ship of the Fleet of New Spain, in the Passage from the Azores towards Cape St. Vincent's. By Don Antonio Ulloa, F. R. S., Commander. From the French. p. 105.

A very favourable, though long, passage gave Don Ulloa the opportunity of observing at sea the eclipse of the sun, which was accompanied by a phenomenon observed by few astronomers, viz. the luminous annulus which surrounds the disc of the moon in such an eclipse. The motion of the ship prevented observing the beginning of the eclipse, by reason of the difficulty

there was in keeping the solar image and a part of that of the moon within the field of the telescope; the object vanished every instant, and it was not till after several fruitless trials that he could get it again. Besides this, the arms grew tired of holding up the telescope and smoked glass, which could not be rested on any thing from the necessity there was of moving the telescope in a contrary direction to that of the ship. He had no calculations but those to be met with in the *Connoissance des Temps*, which he did not find very exact, owing either to some error in the calculations themselves, or to the longitude of the ship's place, not having been accurately determined in that book: he found a pretty sensible difference in the hour set down for the beginning of the eclipse. He observed the total obscurity of the sun's disc at 3^h 44^m, the beginning of the emersion at 3^h 48^m, the end of the eclipse at 4^h 48^m; consequently the middle of it must have been at about 3^h 46^m: the total obscurity lasted 4 minutes, a sufficient time for observing the ring which was formed round the moon.

Five or 6 seconds after the immersion he began to observe round the moon a very brilliant circle of light, which seemed to have a rapid circular motion, something similar to that of a rocket turning about its centre. This light became livelier and more dazzling in proportion as the centre of the moon approached to that of the sun; and about the middle of the eclipse it was of the breadth of about a 6th of the moon's diameter. Out of this luminous circle there issued forth rays of light, that reached to the distance of a diameter of the moon, sometimes more, sometimes less, which made him think they were parts of a weaker light reflected in an atmosphere more subtle than that in which the ring was formed. When the centres of the two planets began to separate, the diminution began, and took place gradually, in the same order which had been observed at its beginning and during the progress of it. It disappeared entirely 4 or 5 seconds before the emersion. The colour of the light was not the same every where; the part immediately joining the disc of the moon was of a reddish cast, from which it changed towards a pale yellow, which about the middle began to clear till, at the external extremity, it ended in an almost entire white. It was equally brilliant throughout, and the whirling motion, common to all the parts of it, seemed to change the form and position of the rays which appeared to the eye sometimes larger, sometimes shorter, at the same time that there was no change either in the colours of the ring themselves, or in the arrangement of them, both which continued as above described.

For 4 or 5 seconds before the appearance of the shining ring, and during as many after it had disappeared, they could see the stars of the 1st and 2d magnitude, as at the entrance of the night; but when it was in its greatest degree of brilliancy, only those of the 1st magnitude could be discovered. The dark-

ness was such, that persons who were asleep, and happened to awake, thought that they had slept the whole evening, and only waked when the night was pretty far advanced. The fowls, birds, and other animals on board took their usual position for sleeping, as if it had been night. Before the edge of the sun's disc emerged from that of the moon, there was discovered near that of the latter a very small point of that of the sun; it was imperceptible to the naked eye, but having looked at it with the glass Don U. estimated it at first to be about the magnitude of a star of the 4th order; after which it seemed to increase to that of one of the 3d. Its first appearance lasted about a minute and a quarter, the luminous circle was still visible, though already much weaker than it had been.

The reddish colour of the ring towards the lunar disc, its deep yellow towards the middle, its clear and very pale yellow at the external extremity, its uniform circumference, and the rays issuing from it to the distance above noticed, convinced him that the whole is the effect of the lunar atmosphere, which is of a substance different from that of the earth, that is, more transparent, more homogeneous, more uniform, and fitter for reflecting the rays of light, since otherwise the ring would not have been equally clear, shining, and coloured throughout the whole circumference of the lunar disc. It cannot be said, he thinks, that this luminous ring is the effect of the rays of the sun reflected by the atmosphere of the earth, because the apparent diameter of the sun is smaller than that of the moon, whose disc entirely hid that of the sun. Besides, if the luminous circle had been made by the atmosphere of the earth, its colours would have been like those of the rainbow, and it would have appeared fixed without motion, instead of which, that which was seen is the same as that which is seen by the naked eye on the sun when it is just above the horizon a little after sun-rise or before sun-set, so that one may conclude, that this luminous circle is a part of the disc of the sun seen after refraction through the moon's atmosphere.

The point of the sun's disc, which was seen before its limb began to emerge from that of the moon, is a very extraordinary phenomenon, which he was not acquainted with before. It was noticed by 3 different observers. This point gradually increased, and when it became of the size of a star of the 2d magnitude, the edge of the sun emerged from that of the moon. The interval between the first discovery of this point and the beginning of the emersion, was about a minute and a quarter. The apparition of the sun, before the beginning of the emersion, can only have taken place through some crevice or inequality on the limb of the moon, not perceivable at the full moon, by reason of the reflected rays which cross each other, and confuse it; whereas at the time of the eclipse, the moon's body being entirely obscured, the light of the sun is behind, and comes

through the smallest openings in the disc without any confusion. The time elapsed between the first appearance of the sun's body through the aperture of the moon's limb and the appearance of the sun's limb out of that of the moon, will serve to determine the depth of the said chink, aperture, or inequality, which is equal to the height of the eminences which form it. The luminous point was towards the north-west part of the moon's disc, a little more to the north than the part of its limb through which that of the sun appeared at the beginning of the emersion; and it is remarkable, that no other luminous speck was perceived in the disc besides this. This aperture is therefore the only one in that part of the disc through which the emersion was to begin; whence we may be certain, that throughout the 4th part of the moon's circumference, reaching from north to west, there is not any perceptible break in its limb besides that which was then observed. There can be no doubt but that the luminous speck which appeared through the aperture was part of the sun's body; this is demonstrated by the red fiery colour (the same as that which is seen when this luminary is looked at through a smoked glass) by its gradual increase in proportion as the limb of the sun came near that of the moon, and in short by the colour, which at its emerging was just the same as that which had been seen through the opening.

It remains to be mentioned, that on the 24th of July, the day on which the above observation on board the *Espagne* was made, the ship's latitude was $37^{\circ} 14'$ north, measured the same day; that since noon it had sailed direct east; that from the end of the eclipse to its being in the meridian of Cape St. Vincent it had sailed 301 sea miles east, making 100 sea leagues, reckoning 20 leagues to a degree; there remains to be known the difference of meridians of the said Cape and the meridian of the different observatories of the capital cities of Europe, in order to determine the part of the sea in which the observation was made relatively to the observatories.

The solar spots were seen very distinctly both before and after the eclipse; there were 6 of them in all: 2 very near each other on the eastern part of the disc; 2 towards the middle of the disc, also very near each other; and 2 towards the north, verging towards the north-west. The corrected altitude of the sun's centre above the horizon, taken at the moment the eclipse ended, was $36^{\circ} 31'$.

XII. On the Theory of Pile-Driving. By Tho. Bugge, Astronomer Royal, and Professor of Astronomy and Mathematics in the Academy of Copenhagen. From the Latin. p. 120.

Among the many conveniences afforded by mechanics to society, the art of pile-driving is not the least. This art was not unknown to the ancients, as appears by many passages in Vitruvius, though he does not particularly describe the machine. For, without this art, it would be impossible to erect those

bridges, moles, bulwarks, pyramids, columns, edifices, the great magnitude and solidity of which we admire, and dare hardly to imitate. All these works require very strong and solid foundations. If the place be marshy or loose earth, piles must be driven in by the force of machines, to place the grated frame on; then more piles with projecting tops, and their intervals filled with large stones, flints, coarse sand, and mortar.

The form of the ancient pile engine is not well known. But several are given by the moderns, as Leopold, Desaguliers, and Belidor. Among all these, that appears to be the best which was invented by Vauloue, described by Desaguliers, and used at piling the foundation at Westminster Bridge. Its chief properties are, that the ram or weight be raised with the fewest men; that it fall freely from its greatest height; and that, having fallen, it is presently laid hold of by the forceps, and so raised again. By which means, in the shortest time, and with the fewest men, the most piles can be driven to the greatest depth.

Belidor has given some theory as to the effect of this machine, but it appears to be founded on an erroneous principle: he deduces it from the laws of the collision of bodies, considering the pile and the falling weight as two striking bodies. But who does not perceive that the rules of collision suppose a free motion and a non-resisting medium? It cannot therefore be applied in the case given, where a very great resistance is opposed to the pile by the ground. We shall now endeavour to explain another theory of this machine.

The problem amounts to this, that we may consider as two machines, a certain weight falling from a certain height, and the head of the pile on which it falls. Now, calling the falling weights w and w ; the altitudes descended A and a ; the masses of the piles M and m ; their surfaces within the earth s and s ; and the depths of the same D and d . The percussion of the falling weight is to be estimated by the product of the mass or weight into the square of the velocity, or by the mass into the altitude descended, since this altitude is as the square of the velocity. But the effects are as the whole forces of their causes. Therefore, if we apply the resistance equally both to the ground and to the weights or masses of the piles, the depths, to which the piles are driven by every stroke, will be in a ratio composed of the direct ratio of the weights and of the altitudes; or $d : D = a \times w : A \times w$.

If we assume the cohesion of the ground as uniform and homogeneous, the resistance will increase in the simple ratio of the rubbing surface only. And if now we make the descending weights equal, with $w = w$; also the altitudes equal, $A = a$; it appears that the effects of the percussions, and hence the depths driven to, will decrease as the superficies and weights or masses of the piles increase. Hence, under the above data, the depths will be in a ratio com-

posed of the inverse ratios of the superficies and of the masses, or $d : D = s \times M : s \times m = \frac{1}{s \times m} : \frac{1}{s \times M}$.

If now all the circumstances be unequal, viz. the falling weights, the heights, the weights of the piles, and their superficies in the earth; then the depths, sunk at each stroke, will be in the ratio composed of the direct ratios of the falling weights and altitudes, and of the inverse ratios of the superficies and masses. Let us now suppose a 3d pile, its mass = M , the superficies sunk s , the falling weight w , the altitude fallen a , and the depth the pile sinks by a given percussion; then, by what is done above, it appears that

$$d : \delta = \frac{1}{m \times s} : \frac{1}{M \times S}, \text{ and } : D = a \times w : A \times W; \text{ therefore } d : D = \frac{a \times w}{m \times s} : \frac{A \times W}{M \times S}.$$

These theorems will serve for practice, and for comparing the effects of different pile-drivers.

PROBLEM.—*To determine the depth sunk by the pile at each stroke of the ram.*—Since in this case both the ram and the weight of the pile are constant and equal, then $w = w$ and $M = m$. Hence the fundamental proportion will be $d : D = \frac{a}{s} : \frac{A}{S}$. And the superficies of the piles in the earth are rectangles of the same base but of different altitude d and D (where D denotes the total depth). Therefore $s : S = d : D$, more simply and conveniently thus expressed $d : D = \frac{a}{d} : \frac{A}{D}$. After a few strokes, so as that the pile may stand firmly in the ground, a new stroke is then given, which may be called the first, then the weight falls from the altitude a , and the pile sinks by the depth d . A 2d stroke is then given, by which the pile sinks the depth x ; the ram falls through the altitude $A = a + d$, and the whole depth of the pile will be $D = d + x$. By substitution then we have $d : x = \frac{a}{d} : \frac{a + d}{d + x}$. From which original analogy is deduced the following equation, $x^2 + dx = d^2 + \frac{d^3}{a}$, and hence $x = \pm \sqrt{\left(\frac{5}{4}d^2 + \frac{d^3}{a}\right)} - \frac{1}{2}d$, the value of the unknown quantity.

To apply this theorem to a given example: let the altitude fallen by the ram be $a = 3$ feet = 36 inches; the depth sunk by the pile at the first stroke $d = 4$ inches; hence $x = 2\frac{2}{3}$ inches nearly. Supposing this 2d stroke to sink the pile 3 inches in round numbers. Then in the 3d stroke the ram will fall through $36 + 4 + 3 = 43$ inches; the depth of the pile will be $4 + 3 = 7$ inches: then the depth sunk by the 3d stroke will come out $x = 2$ inches nearly. For the 4th stroke the altitude fallen will be $36 + 4 + 3 + 2 = 45$ inches, and the depth of the pile $4 + 3 + 2 = 9$ inches: therefore the depth sunk by the 4th stroke will be $x = 1\frac{1}{2}$ inch nearly.

Belidor also solves the same problem. He supposes the pile to sink at first 15 inches: then by his calculus it sinks at the 2d stroke 17, at the 3d 19, at the

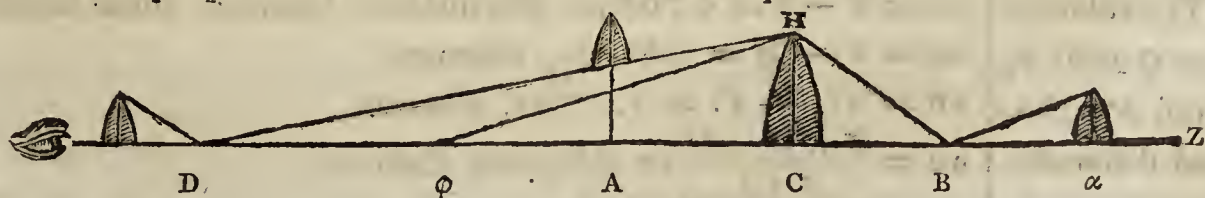
4th 21, at the 5th 23. But who does not easily perceive that this progression is contrary to theory and experience? For the depth acquired by every stroke decreases continually, till at length the pile, after continued strokes, ceases to move. Which happens when the cohesion of the ground and the friction exceed the force of the percussion.

To determine the Greatest Depth, to which a Given Pile can be driven by a Given Machine.—Let the altitude the weight falls at the 1st stroke be $= a$; the depth sunk by the 1st stroke $= d$; after many strokes, when the pile no longer moves, let the pile sink only by the small part m ; after which the strokes have no more effect; the total depth acquired $= x$; and the height the weight then descends $= a + x$. Hence $d : m = \frac{a}{d} : \frac{a + x}{x}$, which gives $x = \frac{ad^2}{am - d^2}$. So, if $d = 4$ inches; $m = \frac{1}{10}$ of an inch; $a = 36$ inches; their theorem gives for the greatest depth $x = \frac{36 \times 16}{\frac{3.6}{10} - 16} = -46.6$ inches: which quantity ought to come out negative, since it is opposite to the altitude, which is taken as positive.*

XIII. Of an Iconantidiptic Telescope, invented by Mr. Jeaurat, of the Academy of Sciences of Paris. From the French. p. 130.

This telescope is called the iconantidiptic heliometer, because it produces 2 images of the objects, the one in a direct position, and the other reversed. These 2 images, of opposite situation to each other, are exactly of the same size, and produce the effect of showing the stars as entering at once both on the right and left sides of the telescope. The first coincidence of the 2 images on the side of each other gives the passage of the first limb; the exact coincidence of the 2 images on each other gives the passage of the star's centre; and the last coincidence of the 2 images at the side of each other gives the passage of the 2d edge: whence it follows, that we not only observe as usual the passage of the 2 sides of a star's disc, but also the direct passage of the star's centre: an observation which could not before be made in a direct manner. Besides, it may be observed, that this invention obviates the difficulty of illuminating the threads of the telescope in observing very small stars, for in this construction there is no occasion to see the threads.

The following is the construction of this iconantidiptic telescope, which appears to be proper for observations made in the plane of the meridian.



* It appears that some mistakes have been committed in the above paper on pile-driving. As 1st, by estimating the friction by $(d + b)b$ instead of $(d + \frac{1}{2}b)b$, where d denotes the depth sunk by the pile after any number of strokes, and b the depth sunk at the next following stroke. And again in bringing out an absurd conclusion at the last, where his theorem brings out a negative quantity, instead of a positive one.

That the solution may be applicable to telescopes, it is proper that $AD = AZ$, $aB = az$. Then put $AD = F$ the focal distance of the lens A ; $aB = f$ the focal distance of the lens a ; $aA = F - f$; $AB = aA - aB = F - 2f$; $BC = x$; $CD = y$; ϕ the focal distance of the lens c . Hence $BD = AD + AB = 2(F - f)$, and $BD = BC + CD = x + y$.

The two values of BD evidently give, 1st, $x + y = 2(F - f)$.

That the image B , given by the lens a , may be seen at the distance BC ; and that the direction of the ray BHD may form a relative focus in D , whose distance may be equivalent to AD , it is necessary that $aB \times CD = AD \times BC$, namely, 2dly, $fy = Fx$.

That the object B seen in the direction BH , may form a focus in D , it is necessary that the focal distance of the lens c (viz. the distance ϕ) have this condition, $\phi \times BD = BC \times CD$, viz. 3dly, $2\phi \times (F - f) = xy$.

From these $\left\{ \begin{array}{l} 1^o \ x + y = 2(F - f), \\ 2^o \ fy = Fx, \\ 3^o \ 2\phi \times (F - f) = xy, \end{array} \right\}$ we easily and incon-
testably find what
follows: viz.

$BC = x = \frac{2f \times (F - f)}{F + f}$ the distance from the focus B to the lens CH ,

$CD = y = \frac{2F \times (F - f)}{F + f}$ $\left\{ \begin{array}{l} \text{the distance of the relative focus } D, \text{ with respect to the} \\ \text{2 lenses } a \text{ and } c, \end{array} \right.$

$\phi = \frac{2Ff \times (F - f)}{(F + f)^2}$ the focal distance of the lens c .

This solution is general: but to adapt it to a particular case, which may be proper for practice, Mr. J. investigates what relation ought to take place between the distances F and f when ϕ is $= f$. This supposition gives $\phi = \frac{2Ff \times (F - f)}{(F - f)^2} = f$; from which we easily extract the relation sought, viz. $F = f(\sqrt{5} + 2)$; or this, which comes to the same thing, $f = F(\sqrt{5} - 2)$.

But $\left\{ \begin{array}{l} \sqrt{5} + 2 = 4.2361 \\ \sqrt{5} - 2 = 0.2361 \end{array} \right\}$ Therefore for the
case in which
 $\phi = f$, we have $\left\{ \begin{array}{l} F = 4.2361f \\ f = 0.2361F \end{array} \right\}$ The relation of
the focal dist.

The Application of the general Formula to the particular case of the equal lenses a and c .

Let $AD = F$, the focal distance of the lens A ,

The relation $\left\{ \begin{array}{l} aB = f = 0.2361F, \text{ the focal distances of the lenses } a \text{ and } c, \\ aA = F - f = 0.7639F, \text{ the distance between these lenses,} \\ f = 0.2361F, \end{array} \right.$
found for the
focal distances,
gives $\left\{ \begin{array}{l} AB = F - 2f = 0.5278F, \text{ distance,} \\ BD = 2(F - f) = 1.5278F, \text{ distance,} \\ BC = \frac{2f \times (F - f)}{F + f} = 0.2918F, \text{ distance.} \\ CD = \frac{2F \times (F - f)}{F + f} = 1.2360F, \text{ distance.} \end{array} \right.$

The numerical application of the particular case of the equal lenses a and c .

$$\begin{array}{lcl} \text{Let } AD = F = 1728 \text{ lines} & \dots\dots\dots & = 12 \text{ Ft. } 0 \text{ In.} \\ \left\{ \begin{array}{l} aB = 0.2361 F = 408 \text{ lines.} \dots\dots\dots 2 \text{ Ft. } 10 \text{ In.} \\ AB = 0.5278 F = 912 \text{ lines.} \dots\dots\dots 6 \text{ Ft. } 4 \text{ In.} \\ aA = 0.7639 F = 1320 \text{ lines.} \dots\dots\dots 9 \text{ Ft. } 2 \text{ In.} \\ aD = aB + BD = 1.7639 F = 3048 \text{ lines.} 21 \text{ Ft. } 2 \text{ In.} \end{array} \right. \end{array}$$

It is from this particular case, in which $\phi = f$, and $f = F \times (\sqrt{5} - 2) = 0.23607 F$, that the following table is constructed.

Focal distance of the lens a and the equivalent AD .			Focal distances of the lenses a and c , and distance ca from the 2d lens c to the third A .			Distance ac from the first lens a to the second c .			Distance aA from the first lens a to the third A .			Whole distance AD .			Distance CD from the 2d lens c to the focus D .		
Fr.	ft.	In.	Fr.	ft.	In.	Fr.	ft.	In.	Fr.	ft.	In.	Fr.	ft.	In.	Fr.	ft.	In.
0	1		0	0	3	0	0	6	0	0	9	0	1	9	0	1	3
0	2		0	0	6	0	1	1	0	1	6	0	3	6	0	2	6
0	3		0	0	8	0	1	7	0	2	3	0	5	3	0	3	8½
0	4		0	0	11	0	2	1	0	3	1	0	7	1	0	4	11
0	5		0	1	2	0	2	8	0	3	10	0	8	10	0	6	2
0	6		0	1	5	0	3	2	0	4	7	0	10	7	0	7	5
0	7		0	1	8	0	3	8	0	5	4	1	0	4	0	8	8
0	8		0	1	11	0	4	3	0	6	1	1	2	1	0	9	11
0	9		0	2	1½	0	4	9	0	6	10½	1	3	10½	0	11	1
0	10		0	2	4½	0	5	3	0	7	8	1	5	8	1	0	4
0	11		0	2	7	0	5	10	0	8	5	1	7	5	1	1	7
1	0		0	2	10	0	6	4	0	9	2	1	9	2	1	2	10
2	0		0	5	8	1	0	8	1	6	4	3	6	4	2	5	8
3	0		0	8	6	1	7	0	2	3	6	5	3	6	3	8	6
4	0		0	11	4	2	1	4	3	0	8	7	0	8	4	11	4
5	0		1	2	2	2	7	8	3	9	10	8	9	10	6	2	2
6	0		1	5	0	3	2	0	4	7	0	10	7	0	7	5	0
7	0		1	7	10	3	8	4	5	4	2	12	4	2	8	7	10
8	0		1	10	8	4	2	8	6	1	4	14	1	4	9	10	8
9	0		2	1	6	4	9	0	6	10	6	15	10	6	11	1	6
10	0		2	4	4	5	3	4	7	7	8	17	7	8	12	4	4
11	0		2	7	2	5	9	8	8	4	10	19	4	10	13	7	2
12	0		2	10	0	6	4	0	9	2	0	21	2	0	14	10	0

XIV. On the Organs of Speech of the Orang Outang. By Peter Camper, M.D., late Professor of Anatomy, &c. in the University of Groningen, and F.R.S. Addressed to Sir J. Pringle. p. 139.*

It is asserted by many travellers, that, though the orang outang does not speak, he

* Dr. Peter Camper, so celebrated for his labours in comparative anatomy, was born in 1722 at Leyden, where he studied the profession of physic. After taking his degree of M.D. at that university, he proceeded on his travels to England, France, and Germany; attending the lectures and demonstrations of the most eminent teachers of physic, surgery, and anatomy, in each of these countries. On his return from his travels he was elected professor of anatomy and surgery at Amsterdam, and was afterwards appointed to a similar professorship at Groningen, which he held with increasing reputation for a great number of years. He died at the Hague in 1789, in the 67th year of

would be able to articulate if he chose it. Several naturalists seem to leave this question undetermined, from not having had the opportunity of dissecting this very uncommon animal; others again overlook it, being deeply engaged in the researches of other parts. Dr. C.'s object in this essay, is to prove the absolute impossibility for the orang and other monkies to speak. Dr. C. being Professor of Natural Philosophy, Anatomy, &c. in the University of Franeker in Friesland, he soon perceived the impossibility of understanding the most valuable works of Galen (especially his anatomical works) without dissecting monkies, with which to compare his exact descriptions. He got for that purpose, in 1754, a cynocephalus, and admired the exactness of almost all Galen's descriptions. The organ of speech however puzzled him much. He discovered in 1757, in another cynocephalus, that the basis of the os hyoides was very large and hollow; and that a membranous bag, lying under the latissimi colli, which touch each other in the middle of the neck in these animals, went up into this bony cavity, having a communication with the inside of the larynx by a hole at the root of the epiglottis. In the cynocephalus he found the whole organ of voice pretty much like that of dogs, except the pouch d, n, o, i, fig. 4, pl. 5. Examining the root of the epiglottis, he found a hole i, p, fig. 3, being the real orifice of the bag d, i, o, n, fig. 4. As all this lies above the rima glottidis i, fig. 3, or i, h, fig. 4. He concluded, that the voice, having passed the glottis, entered this membranous bag, d, n, o, i, by which the neck swelled, and out of which the air was forced by the contraction of the latissimi colli. He had often observed this swelling in some living apes, but now found out the reason

his age. His works, consisting chiefly of academical dissertations, written, some in Dutch, others in Latin, have been translated and published in a collected form, both in German and French. Of the German edition, printed at Leyden, the 1st vol. came out in parts during the years 1782 and 1784; the 2d vol. during 1785 and 1787; and the 3d vol. during 1788 and 1790. The French edition, likewise in 3 vols., was published at Paris in 1803. The most important dissertations on natural history contained in these vols. (German edition) are those which relate to the anatomy of the elephant, to the structure of the bones in birds, to the mode of generation in the pipa of Surinam, to the organ of hearing in whales, to the organ of hearing in fishes, to the organ of speech in the orang outang, and to the structure of the siren lacertina. The subjects of the medical and surgical papers are the contagious diseases of horned cattle, the inoculation of the small-pox, the ruptures occurring in new-born children, the operation for the stone, besides a sort of scientific jeu d'esprit on the best shape for shoes, as adapted to the form and motion of the feet.

Besides the dissertations above enumerated, Dr. C. sent to press, in his life-time, 2 pathological treatises in Latin, the 1st on the structure and diseases of the human arm, and the 2d on the structure and diseases of the human pelvis. After his demise his *Icones Herniarum* were edited by professor Soemmering, and a surgical dissertation in Latin, on fractures of the patella and olecranon, was edited by his son. It only remains to notice his elegant and entertaining work on drawing, in which art he attained great excellence. This work has been translated into English by Dr. Cadogan, under the title of "The Connection of Anatomy with Design."

of it, and was persuaded of the incapacity of this animal to modulate his voice so as to articulate words.

Dr. C. then considered this remarkable passage of Galen's, *de Usu Part.* Ed. Charter. tom. 4, lib. 7, cap. 11, p. 461: "Foramen in utraque lingulæ (epiglottidis) parte unum effecit natura, et foramini ipsi parte internâ ventriculûm supposuit non parvum. In quem quum aer vias nactus amplas in animal ingreditur, rursusque exit, nihil in ventrem depellitur;" and what he, p. 466, further observes, "fissuram potius, quam foramen esse." When he compared this with the organ in the cynocephali, fig. 3, 4, he was at a loss how to explain Galen; for he could by no means apply those ventriculi, by which he seemed to have understood large capacities, to the small holes, h, i, k, fig. 4, above the rima glottidis i, h, which, though much larger in the cynocephali than in men, could not be applied to this very particular definition of no small bags, ventriculûm non parvum.

In November 1758 Dr. C. dissected another monkey, in which the membranous bag, d, n, o, was much larger, so as to occupy almost the whole fore part of the neck, under the latissimi colli. In the apella (the 29th species of Linnæus, *Syst. Nat.* ed. 12, p. 42, or *simia caudata imberbis*, cauda subprehensili, corpore fusco, pedibus nigris) there was no such bag at all, nor any opening at the root of the epiglottis, which was entirely similar to that of dogs. In this monkey, the meatus, or the processus peritonæi, were closed as in men. This he dissected in the year 1768, when he was professor in the university of Groningen. Here he got the opportunity, the year following, of dissecting two papiones or sphinges of Linnæus, *simiæ semi-caudatæ ore vibrissato, unguibus acuminatis*, spec. 6, p. 35, a male and a female; in which the epiglottis was likewise perforated, the os hyoides as in the former, but the pouch very small in comparison of the apes, who were very large.

As these parts are so apparent in many monkeys, and also in the ape, or pithecos, Dr. C. was much surprized that Eustachius did not discover them, especially as he had taken great pains to pursue the anatomical doctrine of Galen, as appears in the 41st plate, where he has given several figures of this organ. Dr. C. was no less surprized that Albinus and Martins did not find this bag; and he wondered how Mr. D'Aubenton, who had the greatest opportunity of any anatomist, could pass over so striking a construction of this organ. He does not mention Riolanus, Fallopius, Gorter, Sylvius, Blasius, and some others, because they had fixed their attention on quite different parts.

As Galen not only dissected the cebi, or the cynocephali, which are all of the tailed or caudati kind, but the pithecos or ape without a tail; and as the celebrated Dr. Tyson had found the organ of voice so similar to that of men in

his pigmy, Dr. C. endeavoured to get one from the East Indies. He got a female one in 1770, and in 1771 another. These and the succeeding years were very favourable to naturalists: for Professor Allamand got a male orang for the Museum of the University of Leyden; Mr. Vander Meulen received one, and Mr. Vosmaer got 2 for the celebrated collection of the Prince of Orange, all females. In the year 1777, Dr. Van Hoey (a physician of celebrity at the Hague, who has a rich collection of natural curiosities) got a male orang, but very young. So that Dr. C. had an opportunity of seeing 7 orangs, besides the living orang, which was sent to the Prince of Orange. All these resembled perfectly in shape and colour that of Mr. Edwards, which is still preserved in the British Museum.

Seven of those, which Dr. C. had seen, had no nails on the great toes of the feet: it surprized him therefore to see them so distinctly represented by Professor Allamand. Dr. C. informed him of it; and he corrected his description accordingly, p. 75, ib. in fine, which was easily done, as the sheets were not worked off at the press. He wrote likewise to Dr. Kooystra, physician to the London Infirmary, to inquire about the orang in the British Museum. The late Dr. Maty examined the orang with him; and both declared, that not a single mark of a nail was to be found on the large toes of that specimen, though Mr. Edwards had represented them on his 213th table so very large. These two instances show, how little we can depend on figures, if not drawn with great exactness. The want of these nails, and of the 2d phalanx of the large toes, is a very remarkable character in this animal. Nature however seems to be inconstant sometimes; for on the great toe of the right foot of the orang in Dr. Van Hoey's collection, there was a small nail and 2 phalanges. The singular red, long hair, and the shortness of the neck, form another very peculiar property; for in the living, as well as in all the rest, the shoulders rise up to the ears; the lower and upper jaws much projected forwards. The country they all came from was Borneo, from which island they are first sent over to Java, and so to Holland by the Cape of Good Hope.

The orang outangs described by Tulpius and Tyson came from Angola, and had both black hair, and large nails on the great toes. The figures by these great men are very deficient in many respects; but, on the whole, the animals are represented and described as very strong and muscular; whereas all the orangs from Borneo were the contrary, and had long and very lean arms and legs. To conclude, it seems very probable, that Africa furnishes a peculiar sort of apes which are not the pithecos of the ancients, though these are not uncommon in Angola. The organs of voice of the Angolese orang, dissected by Tyson, are very different from those of the pithecos which Dr. C. dissected in 1777. This one had the os hyoides like all the papiones or sphinges, &c.; the

epiglottis perforated as in fig. 3. and 4, and therefore different from Galen's description, and from Tyson's, who makes no mention at all of the one, nor of the 2 bags which Galen describes, and which Dr. C. found in the real orang of Borneo; not only in 1 specimen, but in 5, which he dissected for that purpose.

To return to Galen: Dr. C. is very apt to think that he dissected an Asiatic orang, from which he took his description of the ventricles a latere lingulæ, at the sides of the epiglottis; at least that he dissected such an organ, for the bones of the carpus do not entirely agree with his description, though he seems to have been very exact and nice in his dissections. And indeed Dr. C. wondered as often as he compared the structure of the carpus and tarsus of apes, monkeys, and dogs, with Galen's osteological performances on this subject: for though he describes but 8 bones in the carpus, he mentions the 9th, which Dr. C. met with in all monkeys, apes, and dogs, and likewise in the orang. The 10th is not easily seen, being very much attached to the os naviculare. In the Angolese orang, Dr. Tyson met with the vermicular process of the intest. cœcum, which Dr. C. found very considerable in the Asiatic; but of which Galen appears not to have had the least notion. Mr. D'Aubenton has given the description and figure of the same little gut in the gibbon, a species approaching to that of the orang, and likewise an inhabitant of Asia, but also unknown to Galen.

Dr. C. now proceeds to the organ of speech itself, and describes it as it appeared in the first orang he dissected in 1770. And for the clearer understanding he adds some figures to it; 1st, of the fore-part; 2dly, of the larynx from the inside of the pharynx; and, lastly, of the inside of the larynx itself.

In fig. 2 NOP represent the os hyoides; NO, the basis; P the left cornu; NO, the little graniform bones. ATU, the thyroid cartilage; V the aspera arteria; ZX the right ventricle cut open; RS the left; Y the hole leading into the bag. The ventricles form here a kind of meatus, passing over the brim of the thyroid cartilage, under the os hyoides, towards the inside, where they form the fissures ab and ai, fig. 5. In a 2d, Dr. C. found both these ventricles the same in every respect as the others, except that these last were of equal size. In the 3d, which he dissected Aug. 31, 1777, the 2 ventricles were smaller, but of equal size on both sides. The animal was very young. In a 4th, he found both the ventricles united so as to form but one.

The 6th figure gives a sketch of it; acdefghb is the ventricle, having the 2 meatuses a and b, and showing evidently a kind of division in i; gh making a smaller bag. This bag descended downwards to the middle of the breast bone, and spread itself sideways over the sternomastoideus, with appendices underneath the cucullares. The latissimi colli adhered very much to the fore-part, but sideways; and under, from the muscles of the neck, they were easily sepa-

rated by tearing gently, either with the top of the finger, or with the flat part of the handle of a dissecting knife.

As this orang was much larger than the former ones, and consequently older, Dr. C. dares not venture to determine, whether these ventricles or bags, which touch each other in the middle, grow together, so as to make but one bladder; or whether this may be a variety: because in the orang which was alive at the Hague, there was likewise but one bag still larger than these, and proceeding far over the clavicles, backwards under the cucullares, and before down two-thirds of the breast bone. This accidental union can probably make no essential variety; for as these receptacles of air do not seem to serve for any modulation of voice, they will answer their proper purpose, whether united into 1, or divided into 2 cavities. We very often see the kidneys united at the lower ends across the spine in men, without its occasioning any disturbance in the secretion or animal economy. Dr. C. now gives the history of the celebrated orang which belonged to the Prince of Orange, and died in January 1777. This was a female; when alive the head was always deep in the shoulders, and the animal seldom lifted it very high up. The man who took care of her observed a great quantity of air under the skin of the neck on both sides, which, being ignorant of these ventricles, he took for a dangerous disorder, and the symptoms of approaching death. Dr. C. felt the neck himself in December 1776, and discovered the bags to be much larger than any he had dissected. He could remove the air easily with his hand from one side to the other, and divide it, as it were, into 2 parts. The bags appeared sometimes very turgid, sometimes collapsed. She died not long afterwards, and was soon cut to pieces by the order of Mr. Vosmaer, to be stuffed for the museum of his serene highness the Prince of Orange; but, as this cannot be done without preserving the face, with a part of the scull, hands, and feet, Mr. Vosmaer was obliged to cut off the head and the other extremities, and to destroy the most interesting parts for natural knowledge. When the remaining trunk was sent to Dr. C. he found the organ of voice not in the least hurt, and quite entire. After having duly examined, dissected, and delineated the viscera of the breast and belly, he put it in melasses, in a fine phial, to preserve so valuable a preparation.

There was no difference between this organ and that he delineated in the 6th figure, but in extent. The united ventricles covered the greatest part of the breast bone, and had several appendices, which insinuated themselves into all the interstices of the muscles of the neck and shoulders. It had also 2 distinct meatuses coming from the inside of the organ at the sides of the epiglottis, as in fig. 5, and passing between the os hyoides and thyroid cartilage. A large and vermicular process was attached to the cœcum; but the intestines were very different on the inside from those of men. The os femoris was kept in its sockets only by a strong capsular ligament, there being no ligamentum teres.

Having given the structure of the organ of voice in 5 different oranges, and demonstrated their conformity in every other respect but the union he mentioned in some, Dr. C. proceeds now to the internal part of the organ, as it is described by Galen. Fig. 5 shows the inside of the organ, which is represented in fig. 2, consequently of the same orang. *def* is the epiglottis or lingula; *ghkl* the cricoid cartilage, divided in the middle, and expanded sideways; *bdh*, and *gif*, the arytenoid cartilages; *ia*, and *ab*, the holes or fissures at each side of the epiglottis; *b* and *i*, the cords which form the rima glottidis. All this answers exactly to the description given by Galen.

The air which is forced by expiration out of the lungs, and passes with an accelerated velocity the rima glottidis, *bi* being stopped by the hollow epiglottis and the roof of the mouth, narrow nostrils, &c. rushes into these ventricles *zx* and *qrs*, fig. 2, or into the united large ventricle *abdefg*, fig. 6. These are, as Galen rightly observes, seemingly within the animal; for they are covered with the external integuments and the *latissimi colli*. From thence, or out of these ventricles, the air gets out again by the same fissures *ai*, *ab*, fig. 5, through the mouth and nostrils, without entering into the belly of the animal, *rursusque exit, nihil in ventrem depellitur*; by *venter* is to be understood the inside of the body. If this organ does not answer entirely to the description of Galen, Dr. C. does not know how to explain the quotation; for there is no animal, as yet known, whose organ of speech is more applicable to it, at least none of the monkey kind, as before observed.

It is hardly to be conceived, how Dr. Tyson should have overlooked all this, and have pronounced the organ of voice of his pigmy to be exactly like that of men, as he has done p. 51; and yet it is not impossible, when we consider that he has overlooked other and more striking differences in his essay. Nor is it probable that Galen should have overlooked the large vermicular process of the cœcum and other things, if he had dissected the same kind of African orang as Tyson did, unless he dissected the organ of voice in the one, neglecting the intestines, and again the bones of the feet in another, as is often the case with anatomists. This, however, seems probable, that Galen dissected more than one species of *pithecos* or apes without a tail, and that even that species was different from the Angolese pigmy, and from the orang of Borneo.

Having dissected the whole organ of voice in the orang, in apes, and several monkeys, Dr. C. concludes, that oranges and apes are not made to modulate the voice like men: for the air passing by the rima glottidis is immediately lost in the ventricles or ventricle of the neck, as in apes and monkeys, and must consequently return from thence without any force and melody within the throat and mouth of these creatures; and this seems the most evident proof of the

incapacity of oranges, apes, and monkeys, to utter any modulated voice, as indeed they never have been observed to do.

Explanation of the Figures.—Fig. 1, pl. 5, represents the pharynx of the Orang Outang, from which the organs of voice, fig. 2 and 5, are delineated. *A D B C*, the tongue from behind; *D B C M*, the palatum molle, on the back part of which the uvula *B L* is seen; *B E G H F*, the pharynx, divided lengthways in the middle from *E* to *G*; *K M J*, the passage from the mouth into the œsophagus *S K G H*; within this is seen the epiglottis and the glottis shut by the arytenoid cartilages.

Fig. 2 is the same organ of voice from the fore part. *N O P* the os hyoides; *N* and *O* the little graniform bones; *P* the left cornu; *Q T U* the thyroid cartilage; *V* the aspera arteria; *R S* the left ventricle entire; *W* the right, cut open to see the orifice of the duct *Y* from the bag.

Fig. 3 is the back part of the tongue and the glottis of a monkey. *ab* the epiglottis; *abrg* the root and back part of the tongue; *cstd* the œsophagus laid open; *u* the aspera arteria; *fe* the capitella of the arytenoid cartilages; *e* the upper part or top of the little cartilage between the arytenoid cartilage and the epiglottis, which he had likewise met with in men, but less prominent; *if*, the rima glottidis; *pi* the hole at the root of the epiglottis.

Fig. 4 is the inside of the larynx in profile. *abcd* the epiglottis; *e* the cartilage mentioned in fig. 3; *fh* the arytenoid cartilage; *fg* the capitellums forming a kind of crooked hook; *im* the cord of the glottis; *ikh* the lateral sinus above the rima glottidis, forming a pretty large ventricle in these animals; *iml* the cricoid cartilage, *dno* the ventricle, into which the air, coming through the hole at the root of the epiglottis, enters.

Fig. 5 is the same larynx, represented in fig. 1, opened, to see the inner parts. *a*, the union of the cords forming the rima glottidis; *abai* the holes or orifices by which the air enters into the 2 ventricles *rs* and *zx*, fig. 2; *bchd* the right arytenoid cartilage, with its capitellum *d*; *igf* the left arytenoid cartilage; *fed* the epiglottis; *ghkl* the cricoid cartilage, divided and dilated; *kmnl* the wind-pipe.

Fig. 6, the fore part of the Orang, preserved entire in his museum. The skin of the neck and the latissimi colli are laid open to show the ventricles, &c. *A B C* the lower jaw-bone; *A D A E* the genio-hyoides; *F G* the cornua of the os hyoides; *H I* the thyroid cartilage; *K* the cricoid; *L M* the sub-maxillary glands; *acdefghb* the large bladder formed by the union of the 2 ventricles, of which *if* is a mark; *a* and *b*, the 2 meatuses entering towards the inside of the larynx, between the thyroid cartilage and the os hyoides.

*XV. On the Effects of Lightning on Board the Atlas. By Allen Cooper, Esq.,
Master of the Atlas East Indiaman. p. 160.*

The morning and forenoon of the day of this accident, Dec. 31, 1778, were clear and cold, with a strong dry wind from the N. W. At 3 p. m. a squall from the N. N. W. came with a violence scarcely credible, attended with very heavy rain, large hail, and the most severe lightning, which struck the main-mast head, descended down the mast or its rigging, and entered the gun-deck near the main hatchway. Those who were employed in letting down the sheet cable received very smart shocks, and were witnesses to the fire going out at several parts of the ship, and to an explosion equal to that of a well-charged cannon, accompanied with a most sulphureous smell, which lasted all that day and night. It was not till the squall abated that our attention was called to the masts, when

we saw one of our best seamen hanging by his feet in the main catharpins struck dead; another in the main-top was so miserably scorched as to remain senseless, and still continues in a dangerous way. The boatswain's mate, who was near him, had his arm so much hurt by the shock he received, as not to recover the use of it for half an hour. The face of the man killed was quite livid; and from the livid colour of the scorched places it appeared the lightning had entered his head, come out again on the left side of his neck, and spread itself down his left side and over his legs. The other man was struck down in the main-top, his back much scorched, and on the inside of his right leg the stocking burst open a little below the knee. The topgallant-masts, at the time they were struck, had no iron work on them. On a careful examination, no visible track of the lightning could be found on the masts, or any part over which it had passed, nor was any damage done to the ship, masts, or rigging.

XVI. Extracts of Three Letters from John Longfield, M.D. at Cork, containing some Astronomical Observations. p. 163.

By a medium of a great number of meridian altitudes of the sun, and of stars to the north and south of the zenith, the latitude of Dr. L.'s observatory at Cork, was $51^{\circ} 53' 54''$. But the observations of his friend, Mr. Elias Mainaduc, gave him the latitude of Cork $51^{\circ} 54'$, and the longitude $34\frac{1}{2}$ minutes of time west of Greenwich.

Some of the observations are by Mr. Newenham, a young gentleman of considerable abilities, who resides on a hill, about 2400 yards E. answering to a difference of meridians of $7\frac{2}{3}$ seconds of time, and 600 N. of Dr. L.'s observatory, answering to $18''$ difference of latitude, and has a clock with a wooden pendulum; a transit telescope of 30 inches in length, with an acromatic object-glass; and a reflecting telescope of 18 inches focus, made by Dollond. Mr. Mainaduc's observatory was 1600 feet due W. of Dr. L.'s, answering to a difference of meridian of 5 seconds of time.

The magnetic variation 24° W. in July 1778.

The Longitude of Cork settled from the foregoing Observations compared with others made at the Royal Observatory at Greenwich. By Nevil Maskelyne, D.D., F.R.S., and Astronomer Royal. p. 179.

The observations made at the Royal Observatory at Greenwich nearest to those made at Cork are as follow: and the error of the Nautical Almanac with respect to the time observed is set down, and also the correction of the Nautical Almanac, with respect to the time observed, and reduced to the effect of a 3 feet telescope, which shows the immersions of the first satellite sooner, and the emersions later, than the 6 feet reflector does by about 13 seconds.

		Apparent time.		Tele- scope.	State of air.	Correction of Nautical Al- manac.	Cor. of Naut. Alm. for 3 & half teles.
				Feet.			
1772, July 11	Im.	11 ^h	22 ^m 25 ^s	3 $\frac{1}{2}$		-0 ^m 11	-0 ^m 11 ^s
Aug. 26	Em.	14	4 22	6	Air clear.	-0 19	-0 6
Sept. 27	Em.	10	52 43	3 $\frac{1}{2}$	Air very clear.	-0 12	-0 12
Oct. 13	Em.	9	17 4	3 $\frac{1}{2}$	Air clear.	-0 22	-0 22
1773, Aug. 31	Im.	9	51 57	6	Air clear.	-0 3	-0 16
1774, Sept. 10	Im.	15	28 31	6	{ Air clear & 2 ^l 's belts distinct.	+0 57	+0 44
Dec. 29	Em.	11	3 48	6		-0 4	+0 9
1775, Feb. 22	Em.	7	49 37	6	Air clear.	+0 29	+0 42
Aug. 7	Im.	14	53 55	6	Air very clear.	+1 28	+1 15
Oct. 22	Im.	17	37 1	6	Air very clear.	+1 33	+1 20

Hence the times of the Nautical Almanac corrected, and reduced to the effect of a 3 $\frac{1}{2}$ feet telescope, and compared with the observations made at Cork, to find the difference of meridians of Greenwich and Cork, are as follow :

		Observed at Cork.	Correction of Nautical Al- manac.	Nautical Almanac corrected.	Difference of meridians.	Mean.
1772, July 18	Im.	12 ^h 41 ^m 23 ^s	-0 ^m 11 ^s	13 ^h 15 ^m 37 ^s	34 ^m 14 ^s	} per immersions 34 ^m 10 $\frac{1}{2}$ ^s .
1773, Aug. 22	Im.	12 52 29	-0 16	13 26 36	34 7	
1772, Sept. 20	Em.	8 21 16	-0 12	8 55 0	33 44	} per emersions 33 ^m 34 ^s .
1773, Nov. 24	Em.	10 40 16	-0 8	11 14 6	33 50	
1774, Nov. 29	Em.	8 34 19	+0 9	9 8 15	33 56	
1775, Jan. 14	Em.	8 43 23	+0 9	9 17 3	33 40	
Mar. 1	Em.	9 13 16	+0 42	9 46 45	33 29	
Mean of two results.						33 ^m 57 ^s .

Hence the difference of meridians of Greenwich and Cork is 33^m 57^s of time, and the longitude of Cork is 8° 29' 15" west of Greenwich. The latitude of Cork, as determined by Dr. Longfield, by a mean from two quadrants, is 51° 53' 54" north. By Mr. Newenham's observations, compared in like manner, the difference of meridians of Greenwich and his observatory is 34^m 11^s, which, according to Dr. Longfield's observations, allowing 8^s for the difference of meridians, owing to the distance of the two observatories, should be 33^m 49^s, which latter result is most to be depended on. The latitude of Mr. Newenham's observatory being 18" greater than that of Dr. Longfield's, according to the measured distance, is 51° 54' 12" north.

XVII. The Latitude of Madras in the East Indies, deduced from Observations made by William Stevens, Chief Engineer. p. 182.

The observations were taken with an astronomical brass quadrant, on the top of the house usually inhabited by the chief engineer; and the mean latitude is 13° 4' 54" north.

XVIII. Account of an Infant Musician. By Charles Burney, Doctor of Music and F. R. S. p. 183.

That reason begins to dawn, and reflection to operate, in some children much sooner than in others, must be known to every one who has had an opportunity of comparing the faculties of one child with those of another. It has however seldom been found, that the senses, by which intelligence is communicated to the mind, advance with even pace towards perfection. The eye and the ear for instance, which seem to afford reason its principal supplies, mature at different periods, in proportion to exercise and experience; and not only arrive at different degrees of perfection during the stages of infancy, but have different limits at every period of human life. An eye or ear that only serves the common purposes of existence is entitled to no praise; and it is only by extraordinary proofs of quickness and discrimination in the use of these senses, that an early tendency to the art of painting or music is discovered. Many children indeed seem to recognize different forms, persons, sounds, and tones of voice, in very early infancy, who never afterwards endeavour to imitate forms by delineation, or sounds by vocal inflexions.

As drawing or design may be called a refinement of the sense of sight, and practical music of that of hearing; and as a perfection in these arts at every period of life, from the difficulty of its attainment, and the delight it affords to the admirers and judges of both, is treated with respect, a premature disposition to either usually excites the same kind of wonder as a phenomenon or prodigy. But as persons consummate in these arts, and who are acquainted with the usual difficulties which impede the rapid progress of common students, can only judge of the miraculous parts of a child's knowledge or performance, it will be necessary, before speaking of the talents peculiar to the child who is the subject of the present inquiry, to distinguish, as far as experience and observation shall enable, between a common and supernatural disposition, during infancy, towards the art of music.

In general a child is not thought capable of profiting from the instructions of a music-master till 5 or 6 years old, though many have discovered an ear capable of being pleased with musical tones, and a voice that could imitate them, much sooner. The lullaby of a nurse during the first months of a child's existence has been found to subdue peevishness, and perhaps divert attention from pain: and in the 2d year it has often happened, that a child has not only been more diverted with one tune or series of sounds than another, but has had sufficient power over the organs of voice to imitate the inflexions by which it is formed; and these early proofs of what is commonly called musical genius, would doubtless be more frequently discovered if experiments were made, or the mothers or nurses were musically curious. However, spontaneous efforts at forming a tune,

or producing harmony on an instrument so early, have never come to my knowledge.

The arts being governed by laws built on such productions and effects as the most polished part of mankind have long agreed to call excellent, can make but small approaches towards perfection in a state of nature, however favourable may be the disposition of those who are supposed to be gifted with an uncommon tendency towards their cultivation. Nature never built a palace, painted a picture, nor made a tune: these are all works of art. And with respect to architecture and music, there are no models in nature which can encourage imitation: and though there is a wild kind of music among savages, where passion vents itself in lengthened tones different from those of speech, yet these rude effusions can afford no pleasure to a cultivated ear, nor would be honoured in Europe with any better title than the howlings of animals of an inferior order to mankind.

All therefore that is really admirable in early attempts at music is the power of imitation; for elegant melody and good harmony can only be such as far as they correspond with or surpass their models; and as melody consists in the happy arrangement of single sounds, and harmony in the artificial combination and simultaneous use of them, an untaught musician becomes the inventor of both; and those who are at all acquainted with the infancy of such melody and harmony as constitute modern music, can alone form an idea of the rude state of both when an individual discovers them by the slow process of experiment. Every art when first discovered seems to resemble a rough and shapeless mass of marble just hewn out of a quarry, which requires the united and successive endeavours of many labourers to form and polish. The zeal and activity of a single workman can do but little towards its completion; and in music the undirected efforts of an infant must be still more circumscribed: for, without the aid of reason and perseverance, he can only depend on memory and a premature delicacy and acuteness of ear for his guides; and in these particulars the child of whom I am going to speak is truly wonderful.

Wm. Crotch was born at Norwich, July 5, 1775. His father, by trade a carpenter, having a passion for music, of which however he had no knowledge, undertook to build an organ, on which, as soon as it would speak, he learned to play 2 or 3 common tunes, with which, and such chords as were pleasing to his ear, he used to try the perfection of his instrument. About Christmas 1776, when the child was only a year and a half old, he discovered a great inclination for music, by leaving even his food to attend to it when the organ was playing: and about Midsummer 1777 he would touch the key-note of his particular favourite tunes, in order to persuade his father to play them. Soon after this, as he was unable to name these tunes, he would play the 2 or 3 first notes of them, when he thought the key-note did not sufficiently explain which he wished to have played.

But, according to his mother, it seems to have been in consequence of his having heard the superior performance of Mrs. Lulman, a musical lady, who came to try his father's organ, and who not only played on it, but sung to her own accompaniment, that he first attempted to play a tune himself: for, the same evening, after her departure, the child cried, and was so peevish that his mother was wholly unable to appease him. At length, passing through the dining room, he screamed and struggled violently to go to the organ, in which, when he was indulged, he eagerly beat down the keys with his little fists, as other children usually do after finding themselves able to produce a noise, which pleases them more than the artificial performance of real melody or harmony by others. The next day, however, being left, while his mother went out, in the dining-room with his brother, a youth of about 14 years old, he would not let him rest till he blew the bellows of the organ, while he sat on his knee and beat down the keys, at first promiscuously; but presently, with one hand, he played enough of *God save the King* to awaken the curiosity of his father, who being in a garret, which was his work-shop, hastened down stairs to inform himself who was playing this tune on the organ. When he found it was the child, he could hardly believe what he heard and saw. At this time he was exactly 2 years and 3 weeks old. It is easy to account for *God save the King* being the first tune he attempted to play, as it was not only that which his father often performed, but had been most frequently administered to him as a narcotic by his mother, during the first year of his life. It had likewise been more magnificently played than he was accustomed to hear by Mrs. Lulman, the afternoon before he became a practical musician himself; and, previous to this event, he used to teize his father to play this tune on his organ, and was very clamorous when he did not carry his point.

The next day he made himself master of the treble of the 2d part; and the day after he attempted the base, which he performed nearly correct in every particular, except the note immediately before the close, which, being an octave below the preceding sound, was out of the reach of his little hand. In the beginning of November 1777, he played both the treble and base of "*Let ambition fire thy mind*," an old tune which is, perhaps, now better known by the words to which it is sung in *Love in a Village*, "*Hope, thou nurse of young desire*."

On the parents relating this extraordinary circumstance to some of their neighbours, they laughed at it; and regarding it as the effect of partial fondness for their child, advised them by no means to mention it, as such a marvellous account would only expose them to ridicule. However, a few days after, Mr. Crotch being ill, and unable to go out to work, Mr. Paul, a master weaver by whom he was employed, passing accidentally by the door, and hearing the organ, fancied he had been deceived, and that Crotch had stayed at home in order to divert

himself on his favourite instrument; fully prépossessed with this idea, he entered the house, and, suddenly opening the dining-room door, saw the child playing on the organ while his brother was blowing the bellows. Mr. Paul thought the performance so extraordinary, that he immediately brought 2 or 3 of the neighbours to hear it, who propagating the news, a crowd of near 100 people came the next day to hear the young performer, and, on the following days, a still greater number flocked to the house from all quarters of the city; till, at length, the child's parents were forced to limit his exhibition to certain days and hours, in order to lessen his fatigue, and exempt themselves from the inconvenience of constant attendance on the curious multitude.*

The first voluntary he heard with attention was performed at his father's house by Mr. Mully, a music-master; and as soon as he was gone, the child seeming to play on the organ in a wild and different manner from what his mother was accustomed to hear, she asked him what he was doing? And he replied, "I am playing the gentleman's fine thing." But she was unable to judge of the resemblance: however, when Mr. Mully returned a few days after, and was asked whether the child had remembered any of the passages in his voluntary, he answered in the affirmative. This happened about the middle of November 1777, when he was only 2 years and 4 months old, and for a considerable time after he would play nothing else but these passages.

A musical gentleman of Norwich informed Mr. Partridge, that, at this time, such was the rapid progress he had made in judging of the agreement of sounds, that he played the Easter hymn with full harmony; and in the last 2 or 3 bars of hallelujah, where the same sound is sustained, he played chords with both hands, by which the parts were multiplied to 6, which he had great difficulty in reaching on account of the shortness of his fingers. The same gentleman observed, that in making a base to tunes which he had recently caught by his ear, whenever the harmony displeased him, he would continue the treble note till he had formed a better accompaniment. From this period his memory was very accurate in retaining any tune that pleased him: and being present at a concert

* The following letter, corroborative of this account, was received by Dr. Burney, from Norwich:—"There is now in this city a musical prodigy, which engages the conversation and excites the wonder of every body. A boy, son to a carpenter, of only $2\frac{3}{4}$ years old, from hearing his father play on an organ which he is making, has discovered such musical powers as are scarcely credible. He plays a variety of tunes, and has from memory repeated fragments of several voluntaries which he heard Mr. Garland, the organist, play at the cathedral. He has likewise accompanied a person who played on the flute, not only with a treble, but has formed a base of his own, which to common hearers seemed harmonious. If any person plays false, it throws him into a passion directly; and though his little fingers can only reach a 6th, he often attempts to play chords. He does not seem a remarkable clever child in any other respect; but his whole soul is absorbed in music. Numbers crowd daily to hear him, and the musical people are all amazement."—Orig.

where a band of gentlemen performers played the overture in *Rodelinda*; he was so delighted with the minuet, that the next morning he hummed part of it in bed; and by noon, without any further assistance, played the whole on the organ.

His chief delight at present is in playing voluntaries, which certainly would not be called music if performed by one of riper years, being deficient in harmony and measure; but they manifest such a discernment and selection of notes as is truly wonderful, and which, if spontaneous, would surprize at any age. But though he executes fragments of common tunes in very good time, yet no adherence to any particular measure is discoverable in his voluntaries; nor have I ever observed in any of them that he tried to play in triple time. If he discovers a partiality for any particular measure, it is for dactyls of 1 long and 2 short notes, which constitute that species of common time in which many street-tunes are composed, particularly the first part of the *Belleisle March*, which perhaps may first have suggested this measure to him, and impressed it in his memory. And his ear, though exquisitely formed for discriminating sounds, is as yet only captivated by vulgar and common melody, and is satisfied with very imperfect harmony. I examined his countenance when he first heard the voice of Signor *Pacchierotti*, the principal singer of the opera, but did not find that he seemed sensible of the superior taste and refinement of that exquisite performer; however, he called out very soon after the air was begun, "He is singing in F." And this is one of the astonishing properties of his ear, that he can distinguish at a great distance from any instrument, and out of sight of the keys, any note that is struck, whether A, B, C, &c. In this I have repeatedly tried him, and never found him mistaken even in the half notes; a circumstance the more extraordinary, as many practitioners and good performers are unable to distinguish by the ear, at the opera or elsewhere, in what key any air or piece of music is executed.

But this child was able to find any note that was struck in his hearing, when out of sight of the keys, at 2 years and a half old, even before he knew the letters of the alphabet: a circumstance so extraordinary, that I was very curious to know when, and in what manner, this faculty first discovered itself; and his father says, that in the middle of January 1778, while he was playing the organ, a particular note hung, or to speak the language of organ-builders, ciphered, by which the tone was continued without the pressure of the finger: and though neither himself nor his elder son could find out what note it was, the child, who was then amusing himself with drawing on the floor, left that employment, and going to the organ immediately laid his hand on the note that ciphered. Mr. Crotch thinking this the effect of chance, the next day purposely caused several notes to cipher, one after the other, all which he instantly discovered.

and at last he weakened the springs of 2 keys at once, which, by preventing the valves of the wind-chest from closing, occasioned a double cypher, both of which he directly found out. Any child indeed, that is not an idiot, who knows black from white, long from short, and can pronounce the letters of the alphabet by which musical notes are called, may be taught the names of the keys of the harpsichord in 5 minutes; but, in general, 5 years would not be sufficient, at any age, to impress the mind of a musical student with an infallible reminiscence of the tones produced by these keys, when not allowed to look at them.

Another wonderful part of his pre-maturity was the being able, at 2 years and 4 months old, to transpose into the most extraneous and difficult keys whatever he played; and now, in his extemporaneous flights, he modulates into all keys with equal facility. The last qualification which Dr. B. points out as extraordinary in this infant musician, is the being able to play an extemporary base to easy melodies when performed by another person on the same instrument. But these bases must not be imagined correct, according to the rules of counter-point, any more than his voluntaries. He generally gives indeed the key-note to passages formed from its common chord and its inversions, and is quick at discovering when the 5th of the key will serve as a base. At other times he makes the 3d of the key serve as an accompaniment to melodies formed from the harmony of the chord to the key-note; and if simple passages are played slow, in a regular progression ascending or descending, he soon finds out that 3ds or 10ths, below the treble, will serve his purpose in furnishing an agreeable accompaniment.

However, in this kind of extemporary base, if the same passages are not frequently repeated, the changes of modulation must be few and slow, or correctness cannot be expected even from a professor. The child is always as ready at finding a treble to a base as a base to a treble if played in slow notes, even in chromatic passages; that is, if, after the chord of c natural is struck, c be made sharp, he soon finds out that A makes a good base to it; and on the contrary, if, after the chord of D with a sharp third, F is made natural, and A is changed into B, he instantly gives G for the base.

When he declares himself tired of playing on an instrument, and his musical faculties seem wholly blunted, he can be provoked to attention, even though engaged in any new amusement, by a wrong note being struck in the melody of any well known tune; and if he stands by the instrument when such a note is designedly struck, he will instantly put down the right, in whatever key the air is playing. At present, all his own melodies are imitations of common and easy passages, and he seems insensible to others; however, the only method by which such an infant can as yet be taught any thing better seems by example. If he were to hear only good melody and harmony, he would doubtless try to produce something similar; but, at present, he plays nothing correctly, and his volun-

taries are little less wild than the native notes of a lark or a black-bird. Nor does he, as yet, seem a subject for instruction: for till his reason is sufficiently matured to comprehend and retain the precepts of a master, and something like a wish for information appears, by a ready and willing obedience to his injunctions, the trammels of rule would but disgust, and, if forced upon him, destroy the miraculous parts of his self-taught performance.

Mr. Baillet published in the last century a book, "*Sur les Enfants celebres par leurs etudes*;" and yet, notwithstanding the title of his work, he speaks not of infants but adolescents, for the youngest wonder he celebrates in literature is at least 7 years old; an age at which several students in music under my own eye have been able to perform difficult compositions on the harpsichord with great neatness and precision. However, this has never been accomplished without instructions and laborious practice, not always voluntary.

Musical prodigies of this kind are not unfrequent: there have been several in Dr. B.'s memory on the harpsichord. About 30 years ago he heard Palschau, a German boy of 9 or 10 years old, then in London, perform with great accuracy many of the most difficult compositions that have ever been written for keyed instruments, particularly some lessons and double fugues by Sebastian Bach, the father of the present eminent professors of that name, which, at that time, there were very few masters in Europe able to execute, as they contained difficulties of a particular kind; such as rapid divisions for each hand in a series of 3ds, and in 6ths, ascending and descending, besides those of full harmony and contrivance in nearly as many parts as fingers, such as abound in the lessons and organ fugues of Handel.

Miss Frederica, now Mrs. Wynne, a little after this time, was remarkable for executing, at 6 years old, a great number of lessons by Scarlatti, Paradies, and others, with the utmost precision. But the 2 sons of the Rev. Mr. Wesley seem to have discovered, during early infancy, very uncommon faculties for the practice of music. Charles, the eldest, at $2\frac{3}{4}$ years old, surprized his father by playing a tune on the harpsichord readily, and in just time: soon after he played several, whatever his mother sung, or whatever he heard in the street. Samuel, the youngest, though he was 3 years old before he aimed at a tune, yet by constantly hearing his brother practise, and being accustomed to good music and masterly execution, before he was 6 years old arrived at such knowledge in music, that his extemporary performance on keyed instruments, like Mozart's, was so masterly in point of invention, modulation, and accuracy of execution, as to surpass, in many particulars, the attainments of most professors at any period of their lives. Indeed Mozart, when little more than 4 years old, is said to have been "not only capable of executing lessons on his favourite instrument, the harpsichord, but to have composed some in an easy style and taste

which were much approved :” and Samuel Wesley, before he could write was a composer, and mentally set the airs of several oratorios, which he retained in memory till he was 8 years old, and then wrote them down.

Here the difference of education appears : little Crotch, left to nature, has not only been without instructions, but good models of imitation ; while Mozart and Samuel Wesley, on the contrary, may be said to have been nursed in good music : for as the latter had his brother’s excellent performance to stimulate attention, and feed his ear with harmony ; the German infant, living in the house of his father, an eminent professor, and an elder sister, a neat player on the harpsichord, and constantly practising compositions of the first class for that instrument, had every advantage of situation and culture joined to the profusion of natural endowments. Of Mozart’s infant attempts at music Dr. B. was unable to discover the traces from the conversation of his father ; who, though an intelligent man, whose education and knowledge of the world did not seem confined to music, confessed himself unable to describe the progressive improvements of his son during the first stages of infancy. However, at 8 years of age, Dr. B. was frequently convinced of his great knowledge in composition by his writings ; and that his invention, taste, modulation, and execution in extemporary playing, were such as few professors are possessed of at 40 years of age.

Another wish has been formed, that the effects of different genera and divisions of the musical scale might be tried on this little musician ; but the success of such an experiment is not difficult to divine. An uncultivated ear would as naturally like the most plain and common music, as a young mind would best comprehend the most simple and evident propositions : and, as yet, the attention of Crotch cannot be excited by any musical refinements or elaborate contrivance.

It has been imagined by some, that every child might be taught music in the cradle, if the experiment were made ; but to these it may be said, that such an experiment is daily made on every child, by every mother and nurse that is able to form a tune. In Italy the *ninne nonne*, or lullabies, are fragments of elegant melodies, become common and popular by frequent hearing ; and these, though they help to form the national taste, are not found to stimulate the attention of Italian children to melody, or to accelerate the display of musical talents at a more early period than elsewhere.

Premature powers in music have as often surprized by suddenly becoming stationary, as by advancing rapidly to the summit of excellence. Sometimes, perhaps, nature is exhausted or enfeebled by these early efforts ; but when that is not the case, the energy and vigour of her operations are seldom properly seconded, being either impeded and checked “by early self-complacence, or an injudicious course of study ; and sometimes, perhaps, genius is kept from ex-

pansion by ill-chosen models; exclusive admiration, want of counsel, or access to the most excellent compositions and performers in the class for which nature has fitted those on whom it is bestowed.

XIX. Account of a new Method of Cultivating the Sugar Cane. By Mr. Cazaud. From the French. p. 207.

This paper will require but little room and attention in this abridgment; there are extant in the English language so many separate treatises on the sugar-cane, and so many accounts of it in various books of voyages and travels, that it would be useless to dwell upon it in this place. Mr. C. first makes some observations on the climate. In the Windward Islands, he says, the weather is commonly dry from the 15th of February to the 15th of May. The rains are moderate till August; they are very copious the 2 or 3 following months, and afterwards decrease till February; consequently there is a succession of 9 months rain, and of 3 months dry weather. The annexed table shows the quantity of rain fallen at Grenada in the east quarter from the 1st of June 1772 to the first of June 1773, and this is the rain of a common year.

	In.	Ten.
June.....	9	3
July.....	13	9
August.....	11	9
September.....	19	0
October.....	12	7
November.....	9	1
December.....	18	8
January.....	9	5
February.....	6	4
March.....	2	6
April.....	0	8
May.....	2	9
Total 9 ft. 8 in. or	116	0

Then follows the natural history of the cane; the history of the roots of the cane, and of its productions under ground; the history of the joints and growth of the cane in different soils; the history of what Mr. C. calls a revolution in the inside of the cane, and of the arrow which comes out in consequence of that revolution, and constitutes the last stage of the plant's existence; history of the cane according to the two different methods of cultivation, and in different years, favourable, dry, and rainy; and lastly a view of the cane in its different stages.

XX. Account of the Free Martin. By Mr. John Hunter, F. R. S. p. 279.
Reprinted in Mr. H.'s Observations on the Animal Economy.

Meteorological Journal kept at the House of the Royal Society, by Order of the President and Council. p. 295.

This is the usual annual account of the thermometers, barometer, rain, winds, and weather, taken twice in every day of the year 1778; the epitomé of which is contained in the following table.

1778.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Inches.
January . . .	48.0	22.0	37.1	48.5	30.0	38.9	30.22	28.59	29.88	2.111
February . . .	49.0	25.5	38.2	48.0	32.5	39.6	30.35	29.15	29.82	0.527
March	60.0	31.5	42.3	58.0	35.0	43.4	30.43	29.02	29.78	1.243
April	71.5	34.5	50.0	63.0	42.0	51.3	30.24	29.30	29.76	0.734
May	71.5	49.0	58.5	65.0	52.0	58.4	30.28	29.34	29.89	1.133
June	82.0	51.0	65.0	78.0	55.0	66.3	30.36	29.64	30.05	0.957
July	86.0	58.5	70.3	81.0	63.0	71.0	30.29	29.48	29.98	3.053
August	82.0	47.0	67.0	76.5	57.5	68.0	30.51	29.59	30.31	1.039
September . .	71.0	36.5	56.1	67.5	47.0	58.4	30.42	29.15	29.77	0.735
October . . .	64.5	31.5	48.3	68.5	40.0	50.3	30.16	29.17	29.63	2.903
November . .	57.0	32.5	47.0	57.5	38.0	45.1	30.27	28.95	29.68	3.897
December . .	56.0	32.0	44.7	58.5	40.0	46.7	30.81	28.55	29.85	2.440
Whole Year			52.0			53.1			29.86	20.772

The mean variation of the magnetic needle was $21^{\circ} 55' \frac{1}{2}$.

And the mean of the dipping needle was $72^{\circ} 26'$.

XXII. Of the Manner in which the Russians treat Persons affected by the Fumes of Burning Charcoal, and other Effluvia of the same Nature. By Matthew Guthrie, M. D. p. 325.

People of condition in this country have double windows to their houses in winter; but the commoner sort have only single ones; which is the reason that during a severe frost, there is an incrustation formed on the insides of the glass windows. This seems to be composed of condensed breath, perspiration, &c. as a number of people live and sleep in the same small room, especially in great cities. This excrementitious crust is further impregnated with the phlogiston of candles, and of the oven with which the chamber is heated. When a thaw succeeds a hard frost of long duration, and this plate of ice is converted into water, there is a principle set loose, which produces all the terrible effects on the human body which the principle emitted from charcoal is so well known to do in this country, where people every day suffer from it. However, the Russians constantly lay the blame on the oven, when they are affected by the thawing of the crust, as the effects are perfectly similar, and they cannot bring themselves to believe, that the dissolving of so small a portion of ice can be attended with any bad consequence, when they daily melt larger masses without danger: yet the oven does not at all account for the complaints brought on at this period; for, on examination, they generally find every thing right there, and still the ugar, or hurtful vapour, remaining in the room. As the effects of both are similar, and also the mode of recovery, I shall only give an account of the operation of the principle emitted by burning charcoal, and of the method of

bringing those people to life who have been suffocated by it ; this will supersede the necessity of giving the history of both, or rather it will be giving both at the same time.

Russian houses are heated by means of ovens ; and the manner of heating them is as follows. A number of billets of wood are placed in the peech or stove, and allowed to burn till they fall in a mass of bright red cinders ; then the vent above is shut up, as also the door of the peech which opens into the room, in order to concentrate the heat ; this makes the tiles of which the peech is composed as hot as you desire, and sufficiently warms the apartment ; but sometimes a servant is so negligent as to shut up the peech or oven before the wood is sufficiently burnt ; for the red cinders should be turned over from time to time to see that no bit of wood remains of a blackish colour, and that the whole mass is of a uniform glare, as if almost transparent, before the openings are shut ; otherwise the ugar or vapour is sure to succeed to mismanagement of this sort, and its effects are as follow :

If a person lays himself down to sleep in the room exposed to the influence of this vapour, he falls into so sound a sleep that it is difficult to awake him, but he feels, or is sensible of, nothing. There is no spasm excited in the trachea arteria or lungs to rouse him, nor does the breathing, by all accounts, seem to be particularly affected : in short, there is no one symptom of suffocation ; but towards the end of the catastrophe, a sort of groaning is heard by people in the next room, which brings them sometimes to the relief of the sufferer. If a person only sits down in the room, without intention to sleep, he is, after some time, seized with a drowsiness and inclination to vomit. However, this last symptom seldom affects a Russian ; it is chiefly foreigners who are awaked to their dangers by a nausea ; but the natives, in common with strangers, perceive a dull pain in their heads, and if they do not remove directly, which they are often too sleepy to do, are soon deprived of their senses and power of motion, insomuch, that if no person fortunately discovers them within an hour after this worst stage, they are irrecoverably lost ; for the Russians say, that they do not succeed in restoring to life those who have lain more than an hour in a state of insensibility.

The recovery is always attempted, and often effected, in this manner. They carry the patient immediately out of doors, and lay him on the snow, with nothing on him but a shirt and linen drawers. His stomach and temples are then well rubbed with snow, and cold water or milk is poured down his throat. This friction is continued with fresh snow until the livid hue, which the body had when brought out, is changed to its natural colour, and life renewed ; then they cure the violent head-ache which remains by binding on the forehead a cataplasm of black rye bread and vinegar. In this manner the unfortunate man is per-

fectly restored, without blowing up the lungs, as is necessary in the case of drowned persons; on the contrary, they begin to play of themselves as soon as the surcharge of phlogiston makes its escape from the body.

It is well worthy of observation, how diametrically opposite the modes are of restoring to life, those who are deprived of it by water, and those who have lost it by the fumes of charcoal: the one consisting in the internal and external application of heat, and the other in that of cold. It may be alledged, that the stimulus of the cold produces heat, and the fact seems to be confirmed by the Russian method of restoring circulation in a frozen limb by means of friction with snow. But what is singular in the case of people apparently deprived of life in the manner treated of is, that the body is much warmer when brought out of the room than at the instant life is restored, and that they awake cold and shivering. The colour of the body is also changed from a livid red to its natural complexion, which, together with some other circumstances, would almost lead one to suspect, that they are restored to life by the snow and cold water somehow or other freeing them from the load of phlogiston with which the system seems to be replete; for though the first application of cold water to the human body produces heat, yet, if often repeated in a very cold atmosphere, it then cools instead of continuing to heat, just as the cold bath does when a person remains too long in it.

In short, I think it is altogether a curious subject, whether you take into consideration the mode of action of the principle emitted by burning charcoal, and our phlogisticated crust; or the operation of the snow and cold water. However, I shall by no means take upon me to decide, whether the dangerous symptoms related above are produced by the air in the room being so saturated with phlogiston as to be unable to take up the proper quantity from the lungs, which occasions a surcharge in the system, according to Dr. Priestley's theory, or whether so subtle a fluid may somehow find its way into the circulation, and so arrest the vital powers; nor shall I determine whether the livid hue of the body when brought out is changed into a paler colour by the atmosphere somehow absorbing and freeing the blood from the colouring principle, as that author has shown to be the case with blood out of the body: these are curious inquiries that I shall leave to his investigation. I have only endeavoured to collect facts from a number of natives who have met with this accident themselves, or have assisted in restoring others to life. It is so common a case here that it is perfectly familiar to them, and they never call in medical assistance.

XXIII. Of an Apparatus applied to the Equatorial Instrument for Correcting the Errors arising from the Refraction in Altitude. By Mr. Peter Dollond, Optician. p. 332.

The refraction of the atmosphere occasions the stars or planets to appear

higher above the horizon than they really are ; therefore a correction for this refraction should be made in a vertical direction to the horizon. The equatorial instrument is so constructed, that the correction cannot be made by the arches or circles which compose it when the star, &c. is in any other vertical arch except that of the meridian, because the declination arch is never in a vertical position, except when the telescope is in the plane of the meridian. To correct this error, a method of moving the eye-tube, which contains the wires of the telescope in a vertical direction to the horizon, has been practised ; but as the eye-tube is obliged to be turned round, in order to move it in that direction, in the different oblique positions of the instrument, the wires are thus put out of their proper situation, in every other position of the instrument, except when it is in the plane of the meridian ; for the equatorial wire should always be parallel to the equator, that the star in passing over the field of the telescope may move along with it, otherwise we cannot judge whether the telescope be set to the proper declination, except at the instant the star is brought to the intersection of the wires, which is only a momentary observation.

The method I have now put in practice for correcting the refraction of the atmosphere, is by applying 2 lenses before the object-glass of the telescope ; one of them convex, and the other concave ; both ground on spheres of the same radius, which in those I have made is 30 feet. The convex lens is round, of the same diameter as the object-glass of the telescope, and fixed into a brass frame or apparatus, which fits on to the end of the telescope. The concave lens is of the same width, but nearly 2 inches longer than it is wide, and is fixed in an oblong frame, which is made to slide on the frame the other lens is fixed into, and close to it. These 2 lenses being wrought on spheres of the same radius, the refraction of the one will be exactly destroyed by that of the other, and the focal length of the object-glass will not be altered by their being applied before it ; and if the centres of these 2 lenses coincide with each other, and also with that of the object-glass, the image of any object formed in the telescope will not be moved or suffer any change in its position. But if one of the lenses be moved on the other, in the direction of a vertical arch, so as to separate its centre from that of the other lens, it will occasion a refraction, and the image will change its altitude in the telescope. The quantity of the refraction will be always in proportion to the motion of the lens, so that by a scale of equal parts applied to the brass frame, the lens may be set to occasion a refraction equal to the refraction of the atmosphere in any altitude. If the concave lens be moved downwards, that is, towards the horizon, its refraction will then be in a contrary direction to that of the atmosphere, and the star will appear in the telescope as if no refraction had taken place.

There is a small circular spirit level fixed on one side of the apparatus, which

serves to set it in such a position, that the centres of the 2 lenses may be in the plane of a vertical arch. This level is also used for adjusting a small quadrant, which is fixed to it, and divided into degrees, to show the elevation of the telescope when directed to the star; then the quantity of refraction answering to that altitude may be found by the common tables, and the concave lens set accordingly, by means of the scale at the side, which is divided into half minutes, and, if required, by using a nonius, may be divided into seconds.

It must be observed, that when a star or planet is but a few degrees above the horizon, the refraction of the atmosphere occasions it to be considerably coloured. The refraction of the lens acting in a contrary direction would exactly correct that colour, if the dissipation of the rays of light were the same in glass as in air; but as it is greater in glass than in air, the colours occasioned by the refraction of the atmosphere will be rather more than corrected by those occasioned by the refraction of the lens.

A drawing of the refraction apparatus is added, which may serve to give a more clear idea of it. See pl. 5, fig. 7.

AA, the circular brass tube, which fits on to the end of the telescope. BB, the oblong concave lens in its frame, which slides over the fixed convex lens. C, the circular spirit level, which shows when the oblong lens is in a vertical arch. D, the quadrant to which the spirit level is fixed, for showing the angular elevation of the telescope. E, the milled head fixed to a pinion, by which the whole apparatus is turned round on the end of the telescope, in order to set the oblong lens in a vertical arch. F, another pinion for setting the quadrant to the angular elevation of the telescope. By means of these 2 pinions the air bubble must be brought to the middle of the level. aa, is the scale, with divisions answering to minutes and half minutes of the refraction occasioned by the concave lens.

XXIV. Experiments and Observations on Inflammable Air breathed by various Animals. By the Abbé Fontana, Director of the Cabinet of Natural History belonging to his Royal Highness the Grand Duke of Tuscany. p. 337.*

Philosophers believed, till lately, that inflammable air had the power of kil-

* The Abbé Felix Fontana died at Florence in 1805, at the age of 76. He was director of the Florentine Cabinet of Natural History, and member of several learned societies. His scientific pursuits were so numerous and varied, that, while in health, he scarcely allowed himself a moment's leisure. Hence he was enabled to conduct that astonishing number of experiments which we find recorded in his philosophical works, and to execute the most curious models in wax, (some of which and especially the anatomical models; must have required a large portion of time and extreme nicety of labour) besides his 2 wooden statues, which may be taken to pieces, so as to exhibit, with the utmost exactness, the structure of every part of the human body. These statues, one of which he left unfinished, are composed of several thousand different pieces.

Besides the papers inserted in the Phil. Trans. and in Rozier's journal, and some separate tracts on eudiometry, and on nitrous and dephlogisticated air, the Abbé Fontana wrote Osservazioni sopra la Ruggine del Grano, 1767; Osservazioni sopra il falso Ergot e la Tremella, 1775; and Traité sur le Vénin de la Vipere, 2 vols. 4to 1781. It was this last work which procured its author the distinguished rank which he held among modern physiologists and experimenters. In this work (part of

ling animals that breathed it. Dr. Priestley, to whom we are much indebted for many discoveries and observations relating to inflammable air, made in consequence of Mr. Cavendish's excellent paper on that subject, assures us, that inflammable air causes the death of animals as readily as fixed air, and that animals die convulsed in it. The doctor adds, that water absorbed about one quarter of the inflammable air shaken in it, after which a mouse lived in it as long as it would have lived in an equal quantity of common air. This air breathed by the mouse was still inflammable, though not so much as before. Mr. Scheel on the contrary asserts, that inflammable air not only does not kill the animals who breathe it, but that it is even good and innocent air. He relates some experiments to which it seems that nothing can be opposed, and they appear to contradict Dr. Priestley's observations. Mr. Scheel has breathed inflammable air contained in a bladder, without receiving any hurt. Seeing then that the experiments of these celebrated persons contradicted each other, I began to suspect (says the Abbé) that they might possibly be all true; and that their so contradictory effects might be owing to some circumstance not yet attended to.

In order to follow some method in my researches about a point so delicate, and which so nearly interests human life, I first of all thought of assuring myself, whether animals could breathe inflammable air with impunity, when the receivers that contained it were immersed in quicksilver. To this end, I introduced inflammable air, extracted both from zinc and iron, by means of the vitriolic acid, into various tubes filled with quicksilver, in which the air entered pretty free from moisture. I then introduced various birds into those tubes, and observed that they died in a few minutes time, but without any apparent sign of convulsions. These experiments, having been often repeated, were constantly attended with the same event. Being assured, beyond any doubt, that the inflammable air obtained from zinc or iron, and made to pass through quicksilver, was fatal to animals; I next wished to observe, whether it retained the same properties when it had passed through water; in which case the volatile sulphurous acid, or other vapour, is absorbed by the water; but, on trying the experiments, I found that the birds died under these circumstances as under the others, though not quite so soon, showing also some signs of convulsion. I introduced some of this same air, that had passed through water, into a glass tube full of quicksilver, by a method which makes the air lose all its moisture. The birds died in

which had been published in Italian in 1767, under the title of *Ricerche sopra il Veleno della Vipera*) the Abbé treats not only of the poison of the viper, but of the American poisons, of the *Lauro-cerasus*, &c. correcting many errors of former writers on these subjects, and presenting many new and interesting experiments relative to the operation of animal and vegetable poisons on the living body. This work, moreover, contains observations on the structure of the nerves, with experiments on their reproduction, and a description of a new canal of the eye.

it in the same manner as when the experiment was tried on water. In all these cases the air, after the animals had died in it, was still inflammable, nor did its exploding properties seem to have been at all diminished.

The inflammable air extracted from zinc, and that extracted from iron, is fatal to animals even after it has been shaken in water for a minute's time, or something longer. By shaking it a long time, it becomes in some measure respirable; but then it is decomposed in a great measure, and becomes of another kind, though it still preserves the properties of being inflammable, but in a smaller degree. Not only birds but also quadrupeds die in inflammable air, though not so soon, and show some signs of being convulsed.

It seems very strange, that Mr. Scheel could breathe inflammable air with impunity, when animals obliged to breathe it were killed in a very short time. Admitting his experiments to be true, there remains nothing to be said, but that the inflammable air in which animals die does not occasion death because it is conveyed to the lungs, but because it affects some other organs of the animal body exposed to that air, and necessary to animal life. It is not impossible to occasion death by affecting the very sensible nerves of the nose; it being well known, that various liquors, as very concentrated volatile alkali, &c. if they are inspired through the nose, immediately affect the senses, and occasion death if they continue to act on the pituitary membrane. In order therefore to try, whether inflammable air killed only because it was inspired through the nose, I stopped very accurately the noses of various birds with soft wax, and in this manner I introduced them into receivers full of inflammable air extracted from zinc, and from iron, through water. The birds died within a few seconds, that is, just as they did when their noses were unstopped. Quadrupeds were tried after the same manner, and the event was the same.

Having in this manner exploded this new hypothesis, there remained one more, which seemed to suggest a probable reason, since some reason there must be, for Mr. Scheel's experiments being attended with results so different from those of other experimenters. When an animal is introduced into a vessel of inflammable air, its whole body is exposed to that air; and it is not yet known by philosophers what disorders that fluid may occasion to the animal frame. It is true that none are observed to be produced by other noxious kinds of air; but if it be considered, that the vapours of sulphur make a great impression on frogs, even when those animals do not breathe them, but have their *aspera arteria* tied up, it will not seem impossible for the inflammable air, in some way or other, to act on the body of animals. It may perhaps hinder the perspiration; it may insinuate itself through the pores of the skin; in short, its action on the body seems probable till experiments evince the contrary.

I therefore endeavoured to force various four-footed animals to breathe the in-

flammable air through the mouth only, without immerging their whole bodies into it. I chiefly used bladders tied to their mouths, but sometimes I also made use of tubes which entered immediately into the wind-pipe. In both cases the animals died in a very short time: hence it became evident, not only that the inflammable air is pernicious to animal life, but that it does not act on the body of an animal; for I kept some of them immersed in inflammable air, with the mouth only out of it, and did not perceive any effect hurtful to them.

It being thus ascertained that the inflammable air could not be breathed by animals with impunity, it still remained to find out the cause of Mr. Scheel's mistake. I began therefore to breathe the inflammable air contained in bladders, after the manner of Mr. Scheel. The inflammable air used in my experiments was extracted from zinc and from iron by the action of the vitriolic acid, and it was received into bladders that were dry in the inside, but a little moist on the outside. The quantity of air contained in each bladder was about 80 cubic inches. The air coming out of the matrass passed through about one inch of water before it went into the bladders. At first I breathed the inflammable air with a kind of fear; but finding that it occasioned no painful impression, I continued breathing it with courage as long as I could. I breathed in a bladder filled with it eleven times, beginning after a natural expiration. This air when taken out of the bladder, was still inflammable, and being tried with the test of nitrous air it gave II — 28, III + 20.

Before going further I must explain the formula which I use to express the diminution of respirable air, or air of other kind, when mixed with nitrous air. My method is as follows: I have a glass tube of about 18 inches in length, and half an inch in diameter, closed at one end, and of a constant diameter throughout its whole length: this tube has a mark at every 3 inches, which marks or divisions I call measures, and every inch is divided into 20 equal parts; so that every measure is divided into 60 portions, which I call parts. Into this tube, by means of an instrument which measures always one constant quantity of air equal to one measure of the tube, I introduce 2 measures of respirable air and one measure of nitrous air, after which I measure the diminution; then I introduce a 2d measure of nitrous air, and again measure the diminution. The whole measures I express in Roman characters, and the parts of a measure I express in common numbers; for instance, when I say II — 16 and II + 10, the first expression means, that after having introduced into the tube 2 measures of common air, and 1 measure of nitrous air, the space occupied by the mixture of these two airs was 2 measures — 16 parts, or 60ths of a measure: and the 2d expression shows that, after having introduced another measure of nitrous air, the space occupied was 2 measures + 10 parts. The reason and particulars of this method will be given hereafter, in a paper expressly written on the method

of determining the degree of the salubrity of the air by means of nitrous and inflammable air.

Having introduced 8 cubic inches of common air into the same bladder, I breathed it as long as I could; beginning after a natural expiration as in the experiment above related. I breathed it 34 times successively, and afterwards found it very much altered, so that it extinguished a light many times successively. An animal introduced into a vessel of that air immediately gave signs of uneasiness; and the air being tried with the nitrous air gave $\text{II} + 20$, $\text{III} + 15$; whereas, before it had been breathed, it gave with the same nitrous air $\text{II} - 15$, $\text{II} + 18$.

This experiment shows, that the air which remained in the bladder in the first experiment, was not so good as that breathed 34 times successively. In order to make this experiment with more precision, I breathed in 80 cubic inches of common air, introduced into the same bladder, only 11 times; beginning after a natural expiration. I then examined this air with the nitrous air, and found that it gave $\text{II} - 13$, $\text{III} + 28$. Hence it is plain, that the mixture of inflammable and pulmonary air, breathed 11 times, is much inferior to common air breathed an equal number of times; so that there can remain no doubt but that inflammable air is at least worse than common air.

Willing, however, to ascertain this matter still better, I tried to breathe it immediately through a large receiver, partly immersed in water, and swimming in it, so that the air within the receiver was of the same elasticity with the external air. For this experiment I made use also of a glass tube bended in 2 different directions. The air contained in the receiver was about 250 cubic inches. In all the trials made in this manner, I was never able to breathe the inflammable air more than 3 times, and even at the 2d inspiration I felt a great oppression. As these experiments can be depended on, because they were often and at different times repeated, there seems to be reason enough to suspect, that the bladder might possibly alter the nature of inflammable air, and render it more fit for respiration, notwithstanding that the mere contact of the bladder seemed not sufficient to produce such an effect, which is irreconcilable with other facts: yet some reason must certainly exist sufficient to explain Mr. Scheel's experiments, which directly prove that the inflammable air contained in bladders can be breathed with impunity.

When I breathed this air according to Mr. Scheel's manner 11 times successively, I not only breathed it without any inconvenience, but observed that the first inspirations were even pleasing; more so than when I breathed common air. I felt a facility of dilating the breast, as if the air was as light as that at the top of high mountains. I never felt a like sensation, even when I have breathed the purest dephlogisticated air. I do not think that I was mistaken in these sensa-

tions, or gave a loose to imagination, because I was rather prejudiced against the inflammable air, after I had seen various animals immediately die in it, and I was rather fearful when I first began to breathe it : besides, this facility of breathing it, accompanied with a pleasing sensation, I have constantly observed in all my experiments on this subject.

This pleasure however I paid very dear for in another experiment, in which I was near losing my life. Having filled a bladder of the largest sort with about 350 cubic inches of inflammable air, extracted from iron filings through water, which air was not at all diminished by the mixture of nitrous air ; I began to breathe it boldly, owing to the encouragement received from the above related experiment, and resolved to breathe it as long as my strength would permit me, after having made a very violent expiration in order to evacuate the lungs of the atmospheric air. Having made the first inspiration I felt a great oppression on my lungs. Towards the middle of the 2d inspiration I heard Mr. Cavallo, who favoured me with his assistance in these experiments, say, that I was become very pale : by this time the objects appeared confused to my eyes. Notwithstanding this, I made the 3d inspiration ; but now my strength failing, I lost my sight entirely, and fell on my knees. In this situation I breathed the air of the room, but my knees not being able to support me, I fell entirely on the floor. However, in a short time I came to myself, so as to be able to get up, &c. ; but my respiration continued to be affected with difficulty and pain, as if I had a great weight on the breast ; nor did I perfectly recover till the next day. It must be observed, that during this experiment I kept my nose close stopped.

This same inflammable air contained in the bladder, which I had breathed 3 times, was examined in various manners, and was found to be as inflammable as before ; it exploded as usual, when mixed with dephlogisticated air ; but after having been shaken in water for a short time, being tried with the nitrous air, it gave III — 10, IV — 10, whereas before it was not at all diminished. At this time the common air, with the same nitrous air, gave II — 14, II + 10. Hence it appears that the inflammable air, after being breathed, is rather better than before, because in that case it is a little diminished by the addition of nitrous air.

In order to ascertain whether this alteration was occasioned by the bladder, I made the following experiment, which, having been often repeated, was constantly attended with the same event. I introduced into a bladder, which was sometimes moist and sometimes dry, a quantity of inflammable air, extracted as well from zinc as from iron, through water, and having kept it in that situation for some minutes, beating in the mean while the bladder, to keep the air in agitation, I afterwards took it out, and by the mixture of nitrous air observed, that it suffered no diminution, exactly as it suffered none before it had been put into

the bladder. Having thus ascertained that the bladders do not in any manner contribute to render the inflammable air extracted from metals better in its nature, there remained no other way of ascertaining Mr. Scheel's experiments, and of understanding why I had been able to breathe it eleven times, than by supposing that the air of the lungs, which can never be thoroughly emptied, by being mixed with the inflammable air, alters it, &c. It is well known, that in an ordinary expiration about 35 cubic inches of air are expelled from the lungs. In a very violent expiration, following a natural inspiration, the air expelled may amount to 60 cubic inches. These 40 inches of pulmonary air are mixed with the inflammable air, and are expelled from the lungs in proportion to the remaining air that is breathed after the lungs have been thoroughly emptied. In the experiment above related, of the 3 inspirations made into the inflammable air, it may be easily supposed that 20 inches or more of pulmonary air were joined with the inflammable air, and entered into the bladder. This pulmonary air, though it is itself partly phlogisticated, is however diminished by nitrous air; and when it stands in the bladder it is nearly equal to $\frac{1}{7}$ of the inflammable air of the bladder breathed 3 times; hence this lost 10 parts by the mixture of nitrous air.

This explanation, which it is necessary to adopt after having exploded all the other hypotheses, is very analogous to the above related experiment of the smaller bladder filled with inflammable air which was breathed 11 times successively. This air was breathed after a natural expiration, so that there still remained in the lungs about 75 inches of common air. These 75 inches of pulmonary air, together with the 80 inches of inflammable air, were mixed together during the 11 inspirations and expirations; hence the air of the bladder was a mixture of nearly equal portions of inflammable and common air; and accordingly, when tried with the nitrous air, it was found to be much better (though it had been breathed 11 times) than the air of the large bladder breathed 3 times only, after the lungs had been emptied as much as possible.

All the other experiments that I have made in confirmation of this hypothesis seem universally to favour it. If a guinea pig is introduced into a receiver containing 400 cubic inches of inflammable air, or a small bird into only 50 inches of it, and they be left in it till they are dead, that air afterwards will not be sensibly diminished by nitrous air; but if a much larger animal be introduced into the 400 inches of inflammable air, or a small animal into a few cubic inches of that air, then it will be found to be sensibly diminished by nitrous air; and this diminution will be greater as the animal is larger in proportion to the quantity of inflammable air. A larger animal imparts a greater quantity of its pulmonary air to the inflammable air; and the inflammable air will be found joined to a quantity of pulmonary air, which is so much the less as the animal is smaller.

Mr. Scheel says, he found that the inflammable air, after being breathed some time, entirely loses its inflammability; whence he concludes, that the lungs, instead of imparting some phlogiston to, imbibe it from, whatever substance it can be extracted. Though all the direct experiments which show that a phlogistic principle is continually detached from the lungs, and joins itself to the common air, were wanting, still Mr. Scheel's consequence could not be drawn, because the experiment is not true. With respect to my own experience I may safely say, that I have always found it inflammable in every circumstance, even after I had breathed it 11 times successively: and I have not only found it inflammable in the bladder, but I have fired it in the act of letting it out of my mouth. In this manner a flame may be produced from the mouth, of several inches in length, and 2 or more inches in breadth.

But whence originates that sensation of levity and facility of breathing the inflammable air, described above? At present I can only have recourse to a mere mechanical cause for a solution, for I do not observe in inflammable air any property that seems capable of altering the lungs on a chemical principle; neither have I observed any decomposition of air, or alteration of the fluids of the animal. It has been observed, that inflammable air, after being breathed, comes out of the lungs with the same properties it had before. It is also known, that inflammable air is not sensibly absorbed by water, at least after a short time. The lungs, or more properly the pulmonary vesicles, are continually moistened with fluids; but that air cannot be absorbed by them, except it be first decomposed. Nothing else therefore remains to which we can have recourse for an explanation of the above-mentioned sensations, but the well-known levity of the inflammable air compared with common air. And indeed the sensation I felt when I breathed that air, is like that of a very light fluid which does not oppress the lungs, and is hardly felt. This explanation agrees exactly with some experiments I have made with common air rendered more light by fire. This air I have found may be breathed much easier, though not for so long a time, as when it is more condensed. It must be said indeed, that this is occasioned by another particular cause, which has nothing to do with the case of the inflammable air, and which cannot be properly examined in this place.

After all, it still remains to be known, why inflammable air, which kills animals so soon, may be breathed without any oppression, when in a small quantity, viz. when it is mixed with common air; and the following experiments, which are very analogous to those related above, will show that the question is not uninteresting. I introduced 350 cubic inches of common air into a bladder, and after having made as strong an expiration as I could, I applied the neck of the bladder to my mouth, and breathed the air it contained 40 times successively. Afterwards, having taken the air out of the bladder, I found that it extinguished

a light several times successively. It formed various crystals with the oil of tartar, but after a very considerable time; some of these crystals had the shape of needles, others were like flowers: being tried with the nitrous air, it gave II — 18, III + 18. This air therefore was very much phlogisticated, nor could I possibly have breathed it longer than I did, without falling on the ground, as I already felt my strength failing, and the objects appeared confused before my eyes. Into 10 cubic inches of this air I introduced a small bird, which, as soon as it began to breathe it, made various contortions with its body, and seemed to suffer a great deal. It died in 10 minutes time; whereas another little bird introduced into a like quantity, that is, into 10 cubic inches of common air, lived in it 52 minutes; nor did it show any sign of uneasiness before it had been in 5 minutes.

It remains to be accounted for, why the bird could breathe 5 minutes longer in the air of the bladder than a man could. It will be sufficient to consider, that when a man in this experiment has made the last expiration into the bladder, he is in a state of pain; and his lungs are loaden with a superfluous quantity of phlogiston, which is not communicated to the air of the bladder; whereas nothing of this takes place with the bird, which, besides its being in vigour, has a quantity of common air in its lungs. This seems confirmed by an experiment, which admits of no doubt. Having breathed the air of the bladder as long as I could, I stopped the neck of the bladder with my finger, then breathed the common air several times; and afterwards putting the neck of the same bladder to my mouth again, I breathed that very same air 4 times successively. Now there is no doubt but that a bird could have breathed it much longer: the reason of which diversity seems to be the following, viz. that a small bird is in want of a small quantity of air for every time it breathes, whereas a man is in want of a much greater quantity; hence the air is rendered more easily noxious, and unfit for respiration. From all which it may be concluded, that we are in want of a certain quantity of common air necessary for respiration, and for the support of life; and that this air, after being inspired, comes out of the lungs less fit to be breathed a 2d time.

It has been observed, that the inflammable air cannot be breathed when the lungs are emptied of common air as much as possible; but that it may be breathed when the lungs are in a natural state, in which state a quantity of common air, equal to about 40 cubic inches, is known to exist in the lungs of an adult person. This pulmonary air is not infected so far as to be incapable of being breathed several times, and of supporting life. After having made a natural expiration, I have with force expelled from my lungs about 30 inches of air into an empty bladder; and this pulmonary air I have generally been able to breathe 8 times successively, but never longer. It is true however that I breathed

it with some oppression, even from the beginning: which does not happen when the inflammable air contained in a bladder is breathed, the lungs being in a natural state.

And now it seems no longer difficult to give an answer to the question proposed above, and to account for the small difference observed in the breathing of the 2 different kinds of air in the bladders. The inflammable air, when joined to a great quantity of common air, may be breathed safely, because there is a quantity of common air sufficient for various inspirations, and that the mixture of the 2 airs may be breathed till this common air is thoroughly infected. But the inflammable air itself is not altered nor decomposed by the respiration. We must therefore conclude, that the inflammable air is not such a kind of air as can by itself alone be directly useful for respiration. It must rather be considered as if there was nothing of that air in the case of the bladder; and indeed it is found by experience, that the pulmonary air itself may be breathed 8 or 9 times in an empty bladder. The not being able to breathe it 11 times successively, as was done when there was inflammable air in the bladder, and the feeling an oppression in the first case and not in the second, must be entirely attributed to the want of 35 cubic inches of air expired, which are necessary to give the lungs all the necessary expansion; whereas, in the other case, the inflammable air serves to fill up space, and, together with the common air, contributes to fill the lungs; so that the inflammable air, considered under these circumstances, and under this point of view, may be said to be useful for animal respiration. This explanation seems most evidently demonstrated by the following experiment. If 35 cubic inches of common air are introduced into the bladder, and this air be breathed when the lungs are in a natural state, it will be found that we may breathe it 20 times or more; whereas, when the bladder was empty, it could not be breathed more than 9 times at most.

Before finishing this paper it will be proper to mention another cause, which perhaps also contributes to render the inflammable air of the bladder less noxious: this is the levity of the inflammable air itself with respect to common air, which hinders the inflammable mixing with the common air. The inflammable air swims continually on the common air, just as æther swims on water; and the inflammable air swims still better than æther, because it is much lighter in comparison than æther. Various experiments made on volatile substances have convinced me of this truth. If equal quantities of common and inflammable air, or dephlogisticated and inflammable air, be put into a tube, and 2 birds introduced in it, so that one of them may stand at the top, and the other at the lower part of the inverted jar; it will be found, that the first of these

birds not only will die considerably sooner than the other, but will show signs of uneasiness as soon as it is come to that place. The inflammable air therefore, when breathed together with a considerable quantity of common air, must always swim at the top of it, filling the cavity of the windpipe, &c. while the common air occupies the lower place, and filling the smallest pulmonary vesicles is subservient to the ordinary functions of the lungs.

Here I put an end to my observations on inflammable air considered with respect to respiration; but I beg leave to add a few words respecting a property of the inflammable air, which, as far as I know, has been overlooked by the most diligent observers. I mean here to speak of such inflammable air as is extracted from metals, by means of oil of vitriol, especially that extracted from iron and zinc. The air of these metals, when presented to the flame of a candle, not only burns with a whitish flame inclining to green, as is well known, but exhibits a kind of sparks or explosions, which may be easily distinguished between the body of the flame by their vivid light. These sparks, which are of a vivid colour, dart in every direction. They might be easily taken for those sparks that are emitted from red-hot iron; or they might be compared to very small grains of gunpowder, if these were inflamed successively, and without smoke; or they might even be compared to charcoal that sparkles, but without any noise. This phenomenon seems very interesting, as it respects the nature of the inflammable air itself. What seems most singular is, that this appearance forms a distinctive character between the inflammable air of metals, and that extracted from animal or vegetable substances; at least I may safely say, that I never found the inflammable air of animal or vegetable substances sparkle like that extracted from metals. In several of the former kinds of air I could observe no sparkling at all; in others the sparks were so few that they might be considered as nothing in comparison to the sparkling of the inflammable air from metals.

The inflammable air of metals itself, if left in contact with water for a long time, or shaken in it till it becomes less inflammable, will in great measure lose its sparkling property, and at last loses it entirely, when it is become in a state of being hardly inflammable. I have observed, that the inflammable air is the more difficult to be decomposed, by being shaken in water, as the number of the sparks it shows when burning is greater; and according to this number of sparks, the inflammable air makes greater or weaker explosions when mixed with the dephlogisticated air: so that it proves by experiments, that the phlogistic principle is more fixed, and in greater quantity combined, with the inflammable air of metals, than with that of vegetable or animal substances. I do not mean to deny the possibility of finding other species of inflammable air,

extracted from other substances besides metals, which may explode like that extracted from metals; but I only say, that in those cases the inflammable air will also sparkle more, and will be found less easy to be decomposed by water. There are other substances that give the inflammable air in great quantity, and which cannot be considered as animal or vegetable substances, but come rather near the nature of metals; as, for instance, the spathose iron, from which I extract a good deal of inflammable air by the action of fire only, applied to a matrass. But the metal in this substance is not in its pure state, and it may be considered rather as a calx of iron than true iron. Accordingly, this air can hardly sparkle at all; it explodes more like the inflammable air of vegetable or animal bodies, than that of metals, and it is easily decomposed in water.

This property of inflammable air of metals, which I have discovered, throws great light on the analysis of the decomposition of that air, which I have made in 2 different ways. The first is to fire it together with common or dephlogisticated air, in vessels filled with very pure quicksilver, and also in vessels filled with distilled water. The 2d method is to decompose it by shaking it in pure distilled water. In the first process a great number of experiments are required in order to obtain a sensible residuum; besides, the igneous part is lost. The 2d method requires an exceedingly long time, but it is the most complete; for which reason I have used it for the decomposition of other kinds of air.

XXV. On the Variation of the Temperature of Boiling Water. By Sir Geo. Shuckburgh, F. R., and A. S., &c. p. 362.

The heat of boiling water having for some years been used as one of the terms for graduating the scale of thermometers; together with the particular attention the society has lately given to this branch of inquiry; and I may add the singular success with which this age and nation has introduced a mathematical precision, hitherto unheard of, into the construction of philosophical instruments; will render it unnecessary for me to say more in respect of the following experiments, than simply to lay them before the R. s.

That the heat of boiling water was variable, according to the pressure of the atmosphere, seems to have been known to Fahrenheit as early as the year 1724.* A few years after this, M. Le Monnier and Cassini made some decisive observations, to show that this quantity was very considerable. It was left however for Mr. De Luc to make a much more complete series of experiments, which he had described and reduced into system in his *Recherches sur la Variation de la Chaleur de l'Eau bouillante*. It remained only that these should be verified. Towards the end of 1775 I had an opportunity of repeating these observations,

* Vide Phil. Trans., N° 385, where a curious project is proposed, of determining the weight of the atmosphere by means of a thermometer alone, under the title of “*Barometri novi descriptio*.”—Orig.

with a small pocket thermometer of about 6 inches long, made by Mr. Nairne; an instrument, it must be confessed, not very accurate for such an examination, but with which I thought I could observe to within a quarter of a degree; my object at that time, amidst a variety of other philosophical pursuits, being to assure myself that the variation took place, rather than critically to examine the quantity of it. I shall relate these observations, as the result on my return to England led me to some more accurate.

Observations of the boiling-point, made in a journey over the Alps.

Place of observation.	Height of the barometer.	Heat of boiling water by observation.	Heat of boiling water by Mr. De Luc's rule.	Difference.	Difference in $\frac{1}{100}$ of total.
	Inches.	°	°	°	
Bologna.	30.21	213.5	213.5	— 0.5	0
Geneva.	28.60	210.4	210.9	— 0.5	— 16
Modane.	26.61	207.3	207.4	— 0.1	— 2
Lannebourg ..	25.75	205.1	205.9	— 0.8	— 9
Mount Cenis,	24.03	201.2	202.6	— 1.4	— 11
Ditto.	23.91	201.1	202.4	— 1.3	— 10

Mean $\frac{9}{100} = \frac{1}{11}$

Hence it might be concluded, that the motion or variation of the boiling point, with a given variation in the pressure of the atmosphere, was $\frac{9}{100}$ or $\frac{1}{11}$ greater than by the theory alluded to.* These were indeed but gross experiments, and perhaps unworthy of such a competition. They induced me however to make the following. In the beginning of last year (1778), with the assistance of Mr. Ramsden, I procured a most excellent thermometer, every way adapted for this purpose. It was about 14 inches long, but the interval between freezing and boiling only $8\frac{1}{4}$ inches,† and though every degree was something less than the 20th of an inch, yet by means of a semi-transparent piece of ivory, which applied itself close behind the glass tube, sliding up and down in a groove cut in the brass scale for that purpose, carrying a hair-line

* The same instrument immersed in snow just melting at the top of Mount Cenis fell to 32° , the point of freezing observed at the level of the sea.—Orig.

† It may possibly be suggested, that if this interval had been greater, viz. 20, 30, or 40 in. I should have had a much larger scale and more convenient instrument; but in this, as in most other mechanical contrivances, our progress beyond certain limits is prevented; for if the perpendicular height of the column of quicksilver be much increased, the weight of it will be such as to distend the ball, and the instrument may differ from itself in a vertical and horizontal position by half a degree; as I have seen in a tube only 15 inches long; and if this circumstance be endeavoured to be corrected by making the bulb of the thermometer thicker, its sensibility will be proportionably diminished. If my experience were to lead me to conclude any thing, I should consider a tube of a foot long as a maximum, and the bore of such a diameter as to admit a ball of a 4th or a 5th of an inch.—Orig.

division, at the extremity of which was a vernier dividing each degree into 10; with a lens of an inch focus: this apparatus being made moveable, first by the hand, and more delicately by means of a micrometer screw, whose head was divided into 25 divisions, each equal to the 40th of a degree (for so truly cylindrical was the tube, which had been with care expressly selected from a great quantity of glass, that the divisions in the neighbourhood of the freezing point did not differ from those near the boiling point by so much as a 40th of a degree, and this variation appeared in other parts of the tube strictly uniform, as was found by breaking the column of mercury); by means then of this apparatus I was enabled to read off any height of the thermometer to within the 50th of a degree. The vessel, in which the water was boiled, which was always spring water, was a cylindrical tin pot, 13 inches high and $4\frac{1}{4}$ inches wide, with a top something resembling that described in Mr. De Luc's work, contrived to carry off the steam without incommoding the observer, with a waste pipe for the superfluous water in boiling, which might otherwise fall on the fire and extinguish it. The ball of the thermometer was immersed to within $2\frac{1}{2}$ inches of the bottom of the vessel, and $10\frac{1}{2}$ inches below the surface of the water, so that as near as might be the whole column of mercury was exposed to the heat of boiling water, there being only 15° or 20° of the scale, equal to about the 50th part rising out of the water exposed to the temperature of the steam, which in 1 or 2 experiments was found to be 180° or 190° , so that the correction for this defect of heat would only amount to a very few hundredths of a degree, perhaps about .04 or .05, which, as the instrument was exposed to the same circumstances as near as might be in all the observations, I have taken no notice of. I thought it necessary to say thus much respecting the precision of the instrument and the apparatus, and shall now relate the observations at length. A long table of the observations is then given, containing a series of the heights of the barometer all at the heat of 50° , between 26.498 inches, the lowest, and 30.967 the highest, with the annexed correspondent regular series of the heat of the boiling point, from $207^{\circ}.07$ the lowest, up to $214^{\circ}.15$ the highest.

Having collected this series of experiments, I was anxious to see how far they corresponded with Mr. De Luc's, and on comparison of N^o 1 and N^o 15, I found that the decrease of the boiling heat was $\frac{.47^{\circ}}{100}$ greater than the rules admitted of from an alteration of the pressure of the atmosphere of $4\frac{1}{4}$ inches. This difference led me into an examination of all my observations, to see how far they were consistent with themselves; how far they disagreed from Mr. De Luc's; and lastly what general conclusion might be drawn from them. The result was, that the mean motion of the boiling point for 1 inch of the barometer when the mercury stands at 28.886 inches, is $= 1^{\circ}.743$; according to

Mr. De Luc this is $1^{\circ}65$. That the mean motion of the boiling point at 27.908 inches, for 1 inch, is $= 1^{\circ}.779$; by Mr. De Luc $= 1^{\circ}.73$. And lastly, that the mean motion of the boiling point, at 29.925 inches, for 1 inch, is $= 1^{\circ}.709$; by Mr. De Luc $= 1^{\circ}.59$. It should follow then, that, within the limits of my experiments, the alteration or motion of the boiling point is greater by $\frac{1}{18}$ than from that gentleman's observations; that the heat of boiling water is not directly in the simple ratio of the height of the barometer, nor yet is the progression so rapid as Mr. De Luc observed it.

I will now add the annexed general table, for the use of artists in making this instrument, both according to my own observations, and those of Mr. De Luc, that the preference may be given as it shall be thought due; not that it is a matter of any great consequence which is made use of under small variations of the atmosphere; but even under

Height of the ba- rometer.	Correct. of the boiling point.	Diff.	Correct. according to Mr. De Luc.	Diff.
26.0	$-7^{\circ}.09$		$-6^{\circ}.83$	
26.5	$-6^{\circ}.18$.91	$-5^{\circ}.93$.90
27.0	$-5^{\circ}.27$.91	$-5^{\circ}.04$.89
27.5	$-4^{\circ}.37$.90	$-4^{\circ}.16$.88
28.0	$-3^{\circ}.48$.89	$-3^{\circ}.31$.87
28.5	$-2^{\circ}.59$.89	$-2^{\circ}.45$.86
29.0	$-1^{\circ}.72$.87	$-1^{\circ}.62$.83
29.5	$-0^{\circ}.85$.87	$-0^{\circ}.80$.82
30.0	0.00	.85	$-0^{\circ}.00$.80
30.5	$+0^{\circ}.85$.85	$+0^{\circ}.79$.79
31.0	$+1^{\circ}.69$.84	$+1^{\circ}.57$.78

these circumstances, the object of this paper will be sufficiently obvious to all who wish to verify a new theory, or aim at accuracy in these days of precision.

XXVI. Account of a new Kind of Inflammable Air or Gas, which can be made in a Moment without Apparatus, and is as fit for Explosion as other Inflammable Gases in Use for that Purpose; with a new Theory of Gunpowder. By John Ingenhousz., M. D., F. R. S. p. 376.

The important discoveries on different kinds of air have opened a new field for one of the most pleasing and interesting scenes which ever were exposed to the contemplation of philosophers, and therefore could not fail of exciting in the lovers of natural knowledge a decided curiosity to see the pursuit of such striking novelties, and an almost irresistible temptation to imitate them, and to pursue farther the road already so far opened by Priestley, Fontana, Lavoisier, and other learned men. Who can, indeed, without the greatest satisfaction, look, amidst many other objects of admiration, on the discovery of that new aerial fluid, which in purity and fitness for respiration so far supasses the best atmospherical air, that an animal protracts his life 5 times as long, or even more, in it than in common air of the best quality? Dr. Priestley, the first who discovered this wonderful fluid, extracted it from bodies which we should rather have suspected to have contained deleterious exhalations. He afterwards found it existed in many other bodies in which the most accurate observer never found any thing of an approaching nature.

When I consider, (says Dr. I.), the immense quantity of this pure aerial fluid, called by Dr. Priestley with so much propriety dephlogisticated air, which exists as it were in a solid state in nitre, in calcined metals, and even in some other of the most common substances, I cannot express the greatness of my expectation, as a physician, from such an important discovery.* I flatter myself, that ere long an easy and cheap method will be discovered, by which such quantities of this beneficial air may be obtained as will serve to cure several diseases which resist the power of all other remedies, and so prolong, as it were, human life. We may expect, with some degree of confidence, that this new element, when it shall be used for the benefit of respiration, will be found more fit than the best common air to free our body from that quantity of phlogiston or inflammable principle which seems to exist sometimes in too great a quantity in the mass of our blood; or from which it seems sometimes, as it were, to be let loose in too great abundance, producing perhaps in consequence fevers and other symptoms, the causes of which have not yet been clearly elucidated by the best medical writers.

This dephlogisticated air, free from the inflammable particles with which the best common air is always infected, will probably be found capable of absorbing a greater quantity of those phlogistic particles with which the air coming from our lungs is found to be pregnant, and thus of ventilating, as it were, much more expeditiously the mass of our blood of that which a constant exertion of the organs of respiration is not always able to free it from in a sufficient quantity. These important pursuits have led Dr. Priestley to the discovery of one of the benefits, and perhaps the principal, that we derive from respiration; that function of the animal economy which is so important, that without its constant influence an animal, once born, has but a few moments to live.

The criterion of the degree of goodness of respirable air, by the quantity which is absorbed or destroyed by the addition of nitrous air, is one of those useful discoveries of Dr. Priestley's from which mankind will, perhaps, hereafter reap a considerable benefit. The discovery of the various kinds of inflammable airs or gases becoming powerfully explosive, when they are mixed with a sufficient quantity of common air, and still more so when they are combined with dephlogisticated air, is one of those improvements in natural philosophy which, giving occasion to various amusing and interesting experiments, have cast at the

* Since Dr. Priestley's last publication on air, he discovered, that the same water which, if examined immediately, gives only a small quantity of bad air, yields spontaneously about 10 times the quantity of pure dephlogisticated air after standing some time exposed to the sun. Of this I was an eye-witness. The important discoveries of Abbé Fontana on this subject, which he showed me when I was last in Paris a year and a half ago, will soon be published by himself. He extracted this wonderful aerial fluid from different kinds of water by boiling them over a fire.—Orig.

same time a new light upon some powerful agents, whose mischievous force was known, though their nature was still in the dark.

Being at Amsterdam in Nov. 1777, Messrs. *Æneæ* and Cuthbertson, two ingenious philosophers of that city, showed me some curious experiments with explosive and inflammable airs of different kinds. They produced an inflammable air, by mixing together equal quantities of oil of vitriol and spirit of wine and applying heat, to the phial containing the compound. A great quantity of white vapour was extricated, which, passing up the inverted receiver filled with water, settled at the top and depressed the water, as other airs do. This air soon became clear, the white fumes being absorbed by the water. This air was easily lighted in an open cylindrical glass, and burnt almost as clear as a candle, the flame descending gradually lower and lower till it reached the bottom. A very little quantity of this air mixed with common or dephlogisticated air, for instance, a 14th or a 10th part, and kindled by an electrical spark, exploded with a very loud report, and shattered the glass to pieces in which it was kindled, when it did not find a ready vent.

They had contrived a kind of pistol for the purpose, consisting of a strong cylindrical glass tube with a piston adapted to it. To the end of this tube was fixed a brass barrel, like that of a common pistol: into this barrel a brass bullet was put loose, so that the barrel was placed a little above the level, to prevent the bullet rolling out. The barrel was directed to a board of oak at 8 or 10 feet distance. A proper quantity of common and inflammable air, produced in the manner abovementioned, being drawn into the glass tube by means of the piston, it was fired by directing an electrical explosion through it. The explosion was very loud: the ball struck the board with such a force that it made a strong impression in it, and recoiled with a considerable force, so as to strike the wall behind us, and to put us in some danger of being hurt by its rebounding force.

The same gentlemen told me, that this inflammable air had in some respects the advantage over the inflammable airs extracted from metals by the vitriolic or marine acid, and that extracted from mud or marshes; because this air, being heavier than either of these airs, and even than common air, is not so easily lost out of an open vessel; and, that when it escapes into the open air, it agreeably perfumes the room with the smell of spiritus vitrioli dulcis or æther; whereas the other inflammable airs, which from their less specific gravity escape easily into the common air, yield an offensive, disagreeable stench.

Mr. *Æneæ*, having examined the specific gravities of the different inflammable airs compared with common air, favoured me with the following result of his inquiries. A vessel, which contained the weight of 138 grains of common air, contained 25 grains of inflammable air extracted from iron by vitriolic acid, and

92 grains of inflammable air extracted from mud or marshes, and 150 grains of that extracted from oil of vitriol and spirit of wine.

I was much pleased with the abovementioned experiment, and immediately thought that the operation of extracting this inflammable air or vapour could be dispensed with by employing vitriolic æther, which in reality is contained in the vapour expelled by heat from oil of vitriol and spirit of wine, which vapour, condensed in the process of distillation, yields æther. Having arrived in this capital in the beginning of January 1778, I lost no time in pursuing my idea. For this purpose I poured some drops of æther into a strong glass tube, and directed an electrical explosion from a Leyden phial through it; but, to my mortification, no explosion happened. I then threw a bit of cotton, dipped in æther, into the same tube, but it would not take fire. Though these first trials proved unsuccessful, I was too much persuaded, that in some way or other it must succeed, to be discouraged. I tried it in several different ways, and at last, before the end of January, I succeeded once or twice in producing a loud explosion, by throwing into the tube a little bit of paper dipped in æther. But as the experiment often failed, I did not venture to show it to my friends till I had hit on a method, the certainty of which would prevent my being exposed to some confusion by exhibiting an experiment which was so apt to fail. However, I told some few of my friends, early in the spring, that I had found out a method of firing an inflammable air pistol without being at the trouble of making inflammable air in the ordinary way; as I produced it at pleasure in an instant, without any trouble or apparatus; and that I would show them the experiment as soon as I was sure of succeeding constantly. In the mean time I continued to produce this air before my acquaintance in the way I had seen it produced at Amsterdam. Soon after, hitting on better and surer methods of succeeding, I began to show it to those who came to visit me, and in the beginning of the summer I made no scruple of showing it to every body.

The reasons why I did not succeed in the beginning I found afterwards to be, either that I employed too great a quantity of æther, or that the air or vapour of the æther poured into the air pistol, which would not produce an explosion when the pistol was not shaken, made a very loud one when it was forcibly agitated. The surest method of succeeding I find to be the following: I dip a small glass tube, open on both sides, and the bore of which is a 12th of an inch in diameter, into a phial containing æther, and when 2 or 3 drops of the liquid have entered the tube, I apply my finger to the upper end of it, to keep the liquor suspended. I take the tube out of the phial, and thrust it immediately into a small caoutchouck, or elastic gum bottle: I then withdraw my finger from the tube, and take it out of the caoutchouck; thus the little quantity of æther, suspended in the end of the tube, is dropped into the caoutchouck, the neck of

which is to be immediately inverted into the orifice of the air pistol, and, after giving it a gentle squeeze, withdrawn out of it: after which, a bullet or a cork is to be thrust into the mouth of the pistol, when it is ready for firing. This whole operation may be performed in the space of 5 or 6 seconds.

The considerable force of explosion, and the loud report of the ordinary inflammable airs, induced Mr. Volta, of Como, to believe, that these airs might perhaps become a substitute for gunpowder. If this expectation had been well founded, the greatest desideratum would have been to find out a way to produce such air at any time without trouble, and to carry it about in as little compass as possible: which two conditions I should have pretty nearly fulfilled, as all the inflammable air requisite for the explosion of the pistols contrived by Mr. Volta, is contained in the bulk of one single drop of æther; which drop, poured in the pistol itself, is fully sufficient to produce a very powerful explosion. But how far this expectation of Mr. Volta, as to the use of inflammable air in offensive arms, is well grounded, I shall attempt to explain in the subsequent part of this paper.

This inflammable air (which perhaps might more properly be called vapour, as it is absorbable by water) has the principal properties of the other inflammable airs, viz. it will not inflame but where it is in contact with common or dephlogisticated air; and therefore only takes fire at the top of the vessel containing it, the flame going gradually downwards till the whole is consumed, if the vessel is of a cylindrical form, and pretty wide. If it is diluted with common air, but not sufficiently, it will burn as other inflammable air without exploding. Being sufficiently diluted with common air, especially with dephlogisticated air, it explodes with a very great report and a considerable force. It is unfit for respiration in a concentrated state, and is as mortal for an animal plunged into it as any other of the inflammable airs; though in a diluted state it seems to be rather pleasant to the organs of respiration.

It differs in some respects from the common inflammable airs; as, for instance, it is much heavier, as is already observed, than any of the other inflammable airs, and even than common air. It does not inflame or explode with so small a spark of electrical fire as the other inflammable airs, requiring the explosion of a Leyden phial to be fired with certainty, though one single inch of coated glass will be sufficient; so that a Leyden phial, containing 12 or 14 square inches of coating, may fire an air pistol loaded with this kind of air several times, if the charge be divided by taking it out with a small glass tube, fitted up in the manner I described in the paper I had the honour of laying before the R. S. last year; where I describe a kind of an electrical match to light a candle with. This air being in contact with water, is absorbed by it in a few days, though the water be not stirred: and much sooner loses its inflammability

by contact with water than the other inflammable airs do. However, I found that in such a situation this air had not yet lost all its explosive force in 6 days, though the water in which the cylindrical glass, 10 inches long and 1 in diameter, was inverted, after I had poured into it 5 or 6 drops of æther, had gradually ascended till, on the 3d day, it reached to the half of the height of the glass: so that it seems as if these drops of æther, by their expanding in the form of air, had forced out half of the common air contained in the glass, instead of which the water afterwards ascended in proportion as the air or vapour generated by the æther was absorbed by the water.

That this air is specifically heavier than common air, and does not readily incorporate with it, is easily demonstrated by the following experiment. I poured 5 or 6 drops of æther into a cylindrical glass, 10 inches long, and 1 inch in diameter: the æther being soon evaporated, I clapped my hand on the mouth of the glass, and inverted it to incorporate the air generated by the æther with the common air; after which I left the glass open during half an hour, when I dipped in it a piece of wax taper burning, stuck on a bended wire. As soon as the taper reached the brim of the glass, a flame burst out with a weak explosion. In a quarter of an hour I again thrust the wax taper into the same cylinder, and no flame was observed till the wax taper reached the place where the flame ended the first time. This 2d explosion did not set fire to the whole at the bottom of the glass. I again waited a quarter of an hour, and then again thrust the wax taper into it, by which the remainder of this inflammable gas, which had remained settled all that time at the bottom, was exploded.*

I found that æther, in which as much urinous phosphorus is dissolved as will make it luminous in the dark, when some drops are poured on water, is very brisk in taking fire, when employed for an inflammable air pistol; but that the experiment, when repeated, will be apt to fail, because the phosphoric acid which remains in the pistol, and by its nature attracts the humidity of the atmosphere, will soon fill the inside of the pistol with a coat of moisture, and prevent the electrical spark from kindling the inflammable air. It appeared, that a little camphire dissolved in æther increases its explosive force, and makes it less

* It is remarkable, that æther, the most volatile liquor yet known, and so apt to spread itself through the air by a quick evaporation that a drop of very fine æther, which falls from the height of a few feet, is quite evaporated before it reaches the ground, and no glass stopper of itself is able to keep it from evaporating: it is remarkable, I say, that notwithstanding this extreme volatility, the air or vapour, into which it changes by evaporation, should be so far from participating of the same volatility, that it may be kept hours together in an open cylindrical vessel without evaporating, mixing with the common air, or losing its inflammability; so that this substance seems to undergo a sudden metamorphosis, and to change in an instant from the lightest of all liquors to one of the heaviest of aerial fluids, fixed air, which being one half heavier than common air, according to the experiments of Mr. Cavendish, is probably much heavier than this æther air.—Orig.

apt to fail. As this inflammable air is heavier than common air, it is clear that the mouth of the air pistol should be kept upwards at the time of charging it; whereas it is better to invert the pistol when the ordinary inflammable airs are employed, which, being specifically lighter than common air, rise of themselves in the pistol when its mouth is placed inverted on the orifice of the vessel which contains them.

It is true, that the squeezing the elastic gum bottle, when placed on the pistol, forces some of the inflammable gas out of it, which is lost in the common air; but notwithstanding this waste, the inflammable air which remains in the pistol is sufficient to produce a loud report, which is all that is required. Indeed, one single drop of the æther could be easily shaken out of the glass tube immediately into the pistol, without making use of the elastic gum bottle; but this drop, evaporating into elastic air, leaves behind it a good deal of moisture, whether inherent in the æther itself, or attracted from the atmosphere. This moisture, in the way I use to load the pistol, remains in the elastic gum bottle, which is therefore always found moist when the experiment is repeated several times.

It was, indeed, known before this time, that æther and other volatile inflammable liquors spread by evaporating inflammable effluvia through the surrounding air, especially when they are heated; and that these effluvia have sometimes by the imprudent approach of a candle taken fire, and conveyed the inflammation to the liquor itself: but I never heard that any body employed these liquors instead of ordinary inflammable air in communicating to common air an explosive quality, or in firing inflammable air pistols, before I communicated the experiment to my friends. As it will, I think, appear very probable, by what will be said hereafter, that little more than a pleasing amusement can be expected from the force of any inflammable air; the ready production of such inflammable air always ready for use, when an explosion is intended to be produced, may be of some importance to philosophers, whose time must be sparingly taken up.

Comparative view of the expanding force of explosive air and gunpowder.—The force of gunpowder has been ascribed by Sir Isaac Newton, and all other philosophers, to the sudden extrication of an amazing quantity of elastic permanent aerial fluid within a narrow space incapable of containing it; which quantity ought to be attentively attended to, in order to estimate the comparative power of the two substances. Benjamin Robins, whose work, entitled, *New Principles of Gunnery*, passes in this country for a standard book, affirms, that gunpowder, when fired, generates a permanent elastic fluid of 250 times the bulk of the powder before it was fired. He found that common air, which is heated by the contact of a red-hot iron, expands to 4 times its former bulk. Hence he concludes, that the elastic air, disengaged from gunpowder, must expand also to 4 times its dimension; and that it occupies about 1000 times the bulk of the powder in the moment of inflammation.

Count Saluce remarks,* that the elastic air generated by the inflammation of gunpowder occupies, when cold, 222 times the bulk of the powder; which agrees, as he finds, with the computation made by Messrs. Hauksbee, Amontons, and Belidor. This calculation he confirms in the *Melanges de Philosophie et des Mathematiques de la Societ  Royale de Turin*, which is a continuation of the former work. He is of opinion, that this elastic fluid is of the same nature with common air, which was likewise the opinion of Dr. Hales; and that the prodigious force of gunpowder depends on the action of the fire on all its parts, by which this fluid exercises all the force of its elasticity.

The extrication of such a considerable quantity of a permanent elastic fluid by the firing of gunpowder has been put to a particular use by several philosophers. Mr. De La Condamine gives an account of a brass air gun contrived by one Mr. Maty, of Turin; which he loaded with air condensed by firing in it 2 ounces of gunpowder, which, by its inflammation, let loose such a quantity of air as was sufficient to shoot a leaden bullet 60 paces, and to repeat the process 18 times, the strength of the explosion diminishing gradually as in other air guns.

In the learned work of Mr. Antoni, an Italian gentleman, I find the same experiment, the account of which is accompanied with a figure of such an air gun, in which the author fired 1 ounce of gunpowder, the barrel being very stout, and of a size capable of containing 10 ounces. He afterwards let a quantity of this compressed air out by a valve in the same way as it is done in the common air guns. This 1 ounce of gunpowder yielded air enough to propel a leaden bullet through a board 3 lines thick, at the distance of 40 paces, and to repeat the process 16 or 18 times.

The difference of the quantity of elastic fluid obtained from the firing of gunpowder by Mr. Robins and others might be owing to the difficulties attending the investigation, or to the different proportion of the ingredients used in the composition of the powder; as it is well known, that gunpowder for the use of the army is made of 5 or 6 parts of nitre to 1 of charcoal, and 1 of sulphur; when 7 parts of nitre are used, it is called *poudre d'artifice*.

Mr. John Bernouilli calculates the density of the air contained in a solid state in gunpowder to be $\frac{1}{1000}$ of what this fluid is when it constitutes a part of our atmosphere. But he does not consider this air as existing in all the component ingredients of the gunpowder, but chiefly in the nitre: and Count Saluce supposes, that that part of the gunpowder which contains this air constitutes a considerable part of its bulk, though somewhat less than the half. Let us now suppose, that part of the gunpowder which contains this air to be not much less than the half of the whole mass; for it would be difficult to demonstrate

* *Miscellanea Philosophico-Mathematica Societatis privatae Taurinensis*, p. 125.—Orig.

accurately to what proportion of the whole mass this part amounts in reality. On this supposition we shall find, that the whole mass of gunpowder contains a quantity of air in a solid state which is reduced in bulk to near $\frac{1}{500}$, or, in other words, that 1 square inch of gunpowder contains near 500 square inches of air; which being heated in the moment of inflammation will expand to 4 times its diameter; so that according to this calculation gunpowder must expand in the moment of explosion to near 2000 times its own bulk.

It seems very probable, that this calculation of Mr. Bernouilli is much nearer the truth than that of Mr. Robins and Count Saluce; but yet the evaluation of the 2 last-mentioned writers, though short of one half, proves in reality Mr. Bernouilli's calculations to be as just as can be expected, when it is considered, that this evaluation was made before the new discoveries on the nature of nitre and charcoal. But this assertion will be better understood when I have explained the nature of gunpowder somewhat fuller.

If we continue to say, as we have hitherto done, that the charcoal taking fire decomposes the nitre, and extricates from it that amazing quantity of elastic fluid which was shut up within its substance, we only say what we see in reality is the consequence of setting fire to this ingredient. But this explanation does not convey a clear idea of the manner in which the extrication is carried on; nor of the reason why one single spark of fire, thrown into an immense heap of gunpowder, should almost in an instant spread the conflagration through the whole mass. Neither does it explain clearly, why nitre and charcoal, which separately yield no flame at all, though ever so much heated, when combined and intimately mixed together, explode with as loud a report as a large ordnance piece, surpassing even in loudness thunder-claps; nor why this forcible explosion is accompanied by a most brilliant flame.

Nitre is composed of 2 different ingredients, viz. an acid, called from its peculiar nature the nitrous acid, and the vegetable alkali. Neither of these 2 ingredients are capable of inflammation; nay, they even extinguish actual fire. When they are both combined and constitute the neutral salt we speak of, they have not, even by their coalition in one body, acquired an inflammable quality, for nitre may be made red-hot in a crucible without showing the least appearance of inflammation, not even when a red-hot stone or piece of iron is thrown into it. But if any common combustible substance, as wood, charcoal, or such like, is thrown into the melted nitre, a flame issues with a kind of explosion, though only at the very place where the two substances come into contact. The same flame and explosion is observed when cold nitre is thrown upon a combustible body, in a state of real ignition, on a piece of red-hot charcoal for instance.

The true reason of this wonderful phenomenon has not been considered hitherto with that degree of attention it deserves, and could not have occurred

to any body before our modern philosophers had discovered the nature of various kinds of air, and the manner of extracting them from bodies. Inflammable air, the gas flammeum of Van Helmont, was considered as an aerial fluid, susceptible of inflammation by itself. But we now know, that inflammable gas concentrated will not burn at all; but on the contrary extinguishes flame. Mr. Cavendish was the first who set this matter in a proper light. He discovered, by experiments, that a mixture of a small quantity of common air with a great proportion of inflammable air, as, for instance, 2 parts of common air with 8 parts of inflammable air, caught fire without noise, and consumed gradually; but that 3 parts of inflammable air and 7 parts of respirable air exploded with a very loud sound.

It was Dr. Priestley's important discovery which suggested to me the theory I intend to lay before the R. S. This acute philosopher found that, if instead of common air, dephlogisticated air is mixed with a due proportion of inflammable air, the explosion is considerably louder. The principal ingredients of gunpowder, and those to which it owes its force, are nitre and charcoal; for these 2 ingredients, well mixed together, constitute gunpowder at least equal if not superior in strength to common gunpowder, as I found by experience, and may be seen in the Memoire of Count Saluce, inserted in the *Melanges de Philosophie et de Mathematiques de l'Acad. Royale de Turin*. The sulphur seems to serve only for the purpose of setting fire to the mass with a less degree of heat.

Nitre yields by heat a surprizing quantity of pure dephlogisticated air. Charcoal yields by heat a considerable quantity of inflammable air. The fire employed to inflame the gunpowder extricates these 2 airs, and sets fire to them at the same instant of their extrication. Thus the difference between the inflammation of gunpowder and that of a mixture of inflammable air with dephlogisticated air in an ordinary air pistol, as these last are now contrived for a philosophical amusement, seems to be, that the compound of the 2 airs in the air pistol takes fire, when already extricated and existing in a space without compression or condensation; that is to say, when they are in no condition of exerting a much greater elasticity than what they acquired by the heat generated in the moment of their explosion; which heat can only expand them to 4 times their former bulk, according to Mr. Robins: whereas in gunpowder the 2 airs, existing in a solid state before their extrication, and occupying, according to Mr. Robins, about $\frac{1}{250}$ of the space they take up after they are set loose, but most probably even less than $\frac{1}{250}$, as will be seen by and by, are extricated all at once, when confined, as in fire arms, in a space 250 or rather 500 times less than they can occupy when reduced to the temperature of the common atmosphere, and of consequence 2000 less than they can occupy when heated in the moment of in-

flammation; so that the difference of the explosive force in the inflammation of the 2 compounds can be no less than as 4 is to 2000.

It must be here remembered, that air being a very compressible body, a moderate resistance acting against its rarefaction easily overcomes the force of its expansion, when this expansion or rarefaction does not amount to more than 4 times its bulk; that such a power ought not to be very great, we know by the force employed in ordinary wind guns and condensing machines; whereas no condensing machine has yet been contrived by which air could be condensed to any thing approaching the state of condensation of this fluid as it exists in the substance of nitre.

It might be here objected, that air compressed to $\frac{1}{10}$ in a wind gun, possesses a power not much short of gunpowder, though only $\frac{1}{18}$ or $\frac{1}{20}$ of it is let loose at a time; and that thus inflammable air, though expanded only 4 times in the moment of inflammation, may exert a force approaching that of the wind gun, the whole mass of the charge being employed in one and the same explosion. This comparison is very inadequate; for in the case of a wind gun the air compressed to $\frac{1}{10}$ is ready to exert all the force of elasticity existing in the whole mass, and may therefore be compared to a strong spring forcibly bent. But the inflammable air is far from exerting the force of expansion and elasticity through its whole mass at the same instant: for the inflammation is propagated through it successively, beginning where the electrical spark kindles it, and reaching gradually farther till the whole is consumed. Now as I have demonstrated, that inflammable air is reduced to more than half its bulk by inflammation, it must follow, that that portion of it which is consumed the first by the inflammation, leaving more room by its diminution, diminishes in proportion the propelling powers of what remains still to be inflamed.

The very great difference between the explosive force of the 2 compounds is illustrated by what happens after their inflammation. The compound of inflammable with common or dephlogisticated air, is very much reduced in bulk after inflammation. I found this by the following experiment: I fired a brass inflammable air pistol, which had a piston in the cylinder, by which a proper quantity of respirable and inflammable air was drawn in. I had rammed into the barrel, adapted to it, a leaden bullet wrapped up in a piece of leather so strongly, that I did not expect the resistance could be overcome by the explosion. I fired it by an electrical spark; the inflammation took place, the pistol became hot, the ball was not propelled, and the piston was driven more than half way down the cylinder by the pressure of the atmosphere, acting on it when the explosive air was consumed by the inflammation. The case is quite different in the firing of gunpowder, as there remains after its inflammation a mass of air which occupies about 250 times the former bulk, according to Mr. Robins.

As, in the foregoing experiment, the compound of inflammable and common air was reduced above the half of its former bulk, it seems more than probable, that the quantity of dephlogisticated and inflammable air extricated in the firing of gunpowder must also undergo a similar diminution by its inflammation; so, that when there remains a mass of air, 250 times the bulk of the gunpowder, the quantity of air extricated from the powder must have been in reality not less than 500 times the bulk of the powder, which agrees nearly with the calculation of Mr. John Bernouilli. Let us now see how far this computation agrees with the analysis of gunpowder. Abbé Fontana, so advantageously known by his important discoveries in natural philosophy, more especially by those he has made on the various kinds of air, favoured me with the following result of his experiments. An ounce of nitre, exposed to a great degree of heat for the purpose of extracting its air in the usual way, yielded about 800 cubic inches of dephlogisticated air. An ounce of charcoal, treated in the same way, gave about 150 cubic inches of air, partly fixed, partly inflammable, mixed with some common air.

Let us now calculate, without however being too scrupulous about the accuracy of the result, what quantity of elastic permanent fluid a cubic inch of solid gunpowder will give in the moment of deflagration: a cubic inch of solid gunpowder contains in weight 442 grains (which is 38 grains short of an ounce Troy weight) of which $331\frac{1}{4}$ grains is nitre, $55\frac{1}{4}$ charcoal, and as much sulphur; supposing the proportion of the ingredients of the powder to be 6 parts of nitre to 1 of charcoal and 1 of sulphur; $331\frac{1}{4}$ grains of nitre will give about 552 cubic inches of dephlogisticated air; $55\frac{1}{4}$ grains of charcoal will produce about 17 cubic inches of air, chiefly inflammable, according to the calculation of Abbé Fontana.

By this calculation, which will, perhaps, be found more accurate than the former, one cubic inch of solid gunpowder will yield above 569 cubic inches of permanent elastic fluid: I say, above 569 cubic inches, for I do not put into the account the elastic fluid which is generated by the sulphur, nor that which charcoal, consumed by the inflammation of the gunpowder, yields above the quantity mentioned, which it gives when heated in a glass vessel, by which it is by no means consumed, an ounce losing by this operation only 60 grains of its weight. As this elastic fluid will increase to 4 times its bulk, it follows, that 1 cubic inch of solid gunpowder will extricate in the moment of explosion above 2276 cubic inches of elastic air. Which computation is not far from the result of my former calculation, and that of Mr. Bernouilli.

An accurate calculation of the expansion of gunpowder* would be a very

* This is probably most accurately calculated in prob. 17, vol. 2, of Dr. Hutton's Course of Mathematics, p. 345, &c. 5th edition; as deduced by accurate and numerous military experiments. The expansion there deduced, in the conclusion of that problem, p. 353; is nearly 2000 times.

difficult undertaking. The expansion of the moisture always contained in gunpowder, however dry, may also contribute its share towards the amazing powers of this ingredient. Nitre contains from its nature a great share of water, which is necessary for its crystallization, and charcoal is always found to contain it. We know, that very hot vapour is capable of occupying almost 2000 times the space it did in the state of cold water. The generation of dephlogisticated and inflammable air by the inflammation of gunpowder is the reason why this ingredient is almost the only one known, which does not want a free access of common air to be consumed by fire; and therefore it may be said to feed, as it were, on its own air.

This theory of gunpowder induces me to venture a new one of the pulvis fulminans, which consists of 3 parts of nitre, of 2 of fixed alkaline salt, and of 1 of sulphur. This powder much surpasses the force of gunpowder in exploding, with a very loud report, in the open air, when it is heated to a certain degree. It is commonly said, that in the heating of this powder the sulphur joins with the alkaline salt and constitutes an hepar sulphuris, which rising up in bubbles confines the air contained in them, which air at last becomes so powerfully expanded that it overcomes and breaks through the resistance of the coercive bubbles of the hepar sulphuris, with all the force of its elasticity; which sudden emission must naturally occasion a proportionable sound. But I think, that the nitre contained in this powder, being heated, yields its dephlogisticated air when the melting sulphur yields inflammable air; at the same time the sulphur constitutes with the alkaline salt, an hepar sulphuris, which rising in tough bubbles confines this explosive air generated. At length however the increasing heat, which sets fire to the sulphur, sets this explosive air on fire also; which then, following its own nature, explodes with so much the more force from its having been entangled and confined within the bubbles of the hepar sulphuris.

After what has already been said, it will not be difficult to explain, why a single spark of fire propagates the combustion with great rapidity through the whole mass of gunpowder, however great. If we put a single grain of gunpowder on a red-hot iron, we see the particles of red-hot charcoal projected with great rapidity in every direction by the forcible explosion of the 2 airs extricated in the manner before explained. Thus, if 1 or more grains, among a heap of others, are set fire to, the particles of red-hot charcoal, being driven with great violence against the surrounding grains, communicate their heat to all the particles of charcoal they hit, which particles, by heating the particles of nitre in close contact with them, extricate their dephlogisticated air, at the same time that the charcoal yields its inflammable air; in consequence of which a more powerful explosion happens. This secondary explosion projects with a much greater force the particles of charcoal surrounded by the explosive flame of the 2 airs; and thus the conflagration spreads with a very great velocity through the

whole mass, though always by succession. The quickness of this propagation of fire depends in a great measure on the intervals or interstices which remain among the grains of gunpowder, through which the particles of heated charcoal are driven in every direction, together with the flame of the 2 airs. Hence gunpowder reduced into impalpable powder, and rammed into a squib, does not inflame with an explosion, but burns slowly farther and farther, till the combustion reaches the extremity of the squib, where it meets a mass of gunpowder in grains, when immediately a loud explosion issues, by which the squib is shattered into rags. Hence the size of the grains of gunpowder must be proportionate to the size of the fire-arms to which it is destined, the greatest fire-arms requiring in general grains of the largest size.

If this wonderful and awful ingredient had not been discovered by accident, could the secret have escaped a long while the penetration of our modern philosophers, who have found out the way of combining the air of the 2 constituents after they had extricated them, without any regard to the known properties of gunpowder? Nothing more was to be done than combining the 2 substances, instead of combining the 2 airs first separated from them.

Appendix.—In the foregoing paper I attempted to give a comparative view of the explosive force of gunpowder and inflammable explosive air, which latter I had found to be so far short of the explosive force of gunpowder, as not to conceive any well grounded hope that it could ever become a substitute for this ingredient. At that time I had not yet tried the effect of very pure dephlogisticated air, combined with that inflammable air into which I had found that vitriolic æther is changed in an instant. I must acknowledge that I had but small expectations from the force of these 2 airs combined; for as I had always observed, that æther air combined with common air is less brisk in taking fire, and less powerful in exploding, than inflammable air extracted from the vitriolic or marine acid, I thought that the same æther air combined with very pure dephlogisticated air would also be less powerful than common inflammable air from metals. But how far experience contradicted this theoretical analogy will be seen in the following lines.

Abbé Fontana was so good as to assist me in this pursuit. Having produced a good quantity of pure dephlogisticated air from red precipitate by heat, we first filled a strong 2-ounce phial (the orifice of which was so wide that it could scarcely be covered with the thumb, so that the bottle was almost cylindrical) with this air, in the usual manner, by filling it first with water, inverting it, and letting the air rise in it; which being done, we dropped one drop of æther (in which a small quantity of camphire was dissolved) into it, and shut it immediately with the thumb. After having given it some concussions, the orifice was applied to the

flame of a candle, by withdrawing the thumb when the orifice was close to the flame: the air instantly took fire, and exploded with such a strong report, that, if the phial had not been very stout, it would most probably have been shattered into pieces, notwithstanding its wide orifice. We repeated the same experiment with the same success. I was the more astonished at the uncommon loud report, considering the wide orifice of the phial, because, having often tried æther air in the same way with common air, I never found it explode with any considerable degree of force; and therefore I found it necessary, in order to procure a loud report, to kindle it by an electrical spark directed through the pistol, when its orifice was shut up by a cork, the resistance of which was the chief cause of the report.

This wonderful effect in an open vessel could not fail of giving me a good expectation of a very powerful effect, if this compound air was shut up in an air pistol by a cork squeezed into its orifice. As it had been now kindled twice by the flame of a candle, I wanted to kindle it by the same means in an air pistol; for this purpose we drilled a small hole in the side of the pistol, which was made of tin, and contained about 9 cubic inches of space. We filled it with dephlogisticated air in the same manner as we had filled the phial by means of water; and after having poured into it one drop of æther by means of a glass tube, in the manner above described, we shut the orifice by thrusting a cork into it, and kept a finger applied to the touch-hole drilled in the side of the pistol. To avoid accidents if the pistol should burst, we thought it prudent to squeeze the cork very gently into the orifice, so that the resistance should be very moderate. Abbé Fontana wrapped a towel round the pistol for security's sake, leaving only the touch-hole uncovered; which being brought near the flame of a wax taper, the air instantly took fire, and exploded with such a strong report, that his hearing, as well as mine, was much hurt by it. The cork, which was a very sound one, flew to pieces against the wall; and the Abbé felt such a considerable shock in his hands, that he did not think it safe to repeat the experiment, unless a stronger pistol could be procured.

Encouraged by such uncommon and unexpected effects, I went immediately to Mr. Nairne to inquire, whether he still had in his possession a strong brass air pistol, which he had made last summer according to my direction? I was lucky enough to find it: nothing was to be done to it but to drill a touch-hole in the left side of it, in order to kindle it by a flame if required. This touch-hole was to be shut up by a brass male screw fitted exactly to it, when the pistol was intended to be fired by an electrical spark. The air box of this pistol was a cylinder 4 inches long and 2 inches in diameter. The fore part of the air box, to which the pistol barrel, fitted to receive a leaden ball or a cork, was fixed, had a broad

shoulder, which was fastened to the body of the air box by 6 strong brass screws, which never had been loosened by former explosions. A leaden bullet, wrapped up in leather, was forcibly rammed into the pistol barrel as far as the screw, which joins the barrel with the air box. The pistol was filled with pure dephlogisticated air, which was drawn in by the piston from an elastic gum bottle, and 1 drop of æther being poured into it, the air within was kindled by an electrical spark directed through it. The air took fire: the explosion was as loud as that of a common musket, and the force so great, that the whole fore part of the air box with the pistol barrel flew off, all the 6 screws were broken, and the strong and tough metal of which they were made was rent. Three strong brass screws, by which the bottom of the air box was fixed to the wooden handle, were loosened, and the whole frame of the pistol was out of order. The substance of the air barrel, where it was torn, was of the thickness of about a half-crown piece.

Being now convinced, that though inflammable air from metals with dephlogisticated or common air, is far inferior to the force of gunpowder, the explosive force of the compound of dephlogisticated and æther air approaches it much nearer, I thought it worth while to fit the pistol up in such a manner as to be out of all danger of bursting. For this purpose I desired Mr. Nairne to adapt, and solder to the fore part of the air box, a hollow cone of brass, the extremity of which should terminate in the gun barrel. As the piston could not reach to the extremity of this conical hollow, which consequently must be always filled with common air, I desired him to fix to the piston an ivory cone, through which the 2 wires would pass to meet each other at the surface of the cone, leaving an interstice between them of about 1 line, through which the electrical spark should leap and set fire to the air. This ivory cone shutting up exactly the whole cavity of the air box, no air could come into it but what was drawn in by the piston.

The pistol thus fitted up answered tolerably well. The cone, instead of ivory, may be made of solid glass, which is a better non-conductor than ivory. The canals in the ivory, through which the 2 wires pass, may be made wide enough to contain a glass tube, through which the wires pass; or to be filled with a non-conducting cement, as sealing-wax, for the same purpose. The cone may even be made of brass, provided 2 glass tubes are lodged in it, to give a passage to the 2 wires.

I kindle this pistol sometimes by putting in the touch-hole a little bit of a cotton thread soaked in moist gunpowder and dried afterwards; or a bit of those paper matches which the Chinese put into those little squibs, which go by the name of India crackers. I sometimes kindle it by holding the flame of a candle or a burning paper to the touch-hole. In this case it is to be observed, that the touch-hole must be kept upwards, if the pistol is loaded with inflammable air from metals, because this air being lighter than common air, will rise out of the

hole and meet the flame. The contrary must be done when æther air is employed, it being heavier than common air, and thus disposed to descend and fall on the flame kept under it.

To fill this pistol with any air, I commonly first fill an elastic gum bottle with it, the orifice of which is just large enough to receive that part of the gun barrel which is fixed to the air box: thus, by squeezing between my feet the elastic gum bottle, I draw in at the same time the air by drawing up the piston. A bladder is also very fit for this purpose, and has the advantage above an elastic gum bottle in not requiring to be squeezed to draw the air out of it. Inflammable air from metals will rise in the pistol of itself, when its orifice is kept on the bottle containing it.

If the pistol is destined to be always kindled by the flame of a candle or a match, as I have described, it would be better to have no piston to it, as it may then be filled by the means of water, and the explosive force will be so much the greater, as some of the flame makes easily its way over the leather of the piston, and rushes out backward, which I find is often the case, if the bullet is rammed in the barrel somewhat too tightly.

It would perhaps not be an easy undertaking to give a satisfactory reason, why a drop of æther communicates to dephlogisticated air a much stronger explosive force than common inflammable air from metals. May it not be said, that common inflammable air from metals, having only about a 5th of the specific gravity of the dephlogisticated air, the 2 fluids do not penetrate each other so readily and so intimately as the compound of dephlogisticated and æther air, which are both nearly of the same specific gravity, each being somewhat heavier than common air? for it seems not improbable, that the swiftness with which the flame is propagated through the mass of this compound air, depends partly on the intimate mixture of the phlogiston with the dephlogisticated air. Might not this phenomenon be ascribed to the greater bulk of inflammable air from metals, compared with the small compass which one single drop of æther occupies, which last ingredient, when pure, seems to be an essence of the inflammable principle of the spirit of wine, a pure phlogiston concentrated in the form of a liquid? indeed the inflammable air from metals seems to be rather a compound of phlogiston and some kind of elastic permanent fluid, than a pure inflammable fluid; for this air, after having lost all its inflammability, by being kept a long while on water, occupies still a considerable space, and is then become phlogisticated air; that is to say, such an air as is not to be diminished by nitrous air, or to be inflamed.

Though I have no reason to alter my former assertion, that the force of gunpowder is proportionable to the sudden extrication of a great quantity of the elastic fluid generated in the moment of conflagration, and the expansion of this fluid

by heat, communicated to it in the same moment of its extrication; and that the force of inflammable explosive air can only be proportionable to the sudden expansion by heat in the moment of the inflammation, for no new extrication here takes place; yet I did not consider enough in the account the suddenness of this expansion, which may make a considerable difference in the force of the explosion. And indeed the above-mentioned experiments seem to demonstrate, that the inflammation of the compound of pure dephlogisticated and æther air spreads with such a velocity through the whole mass as to be almost instantaneous.

It is well known, that mechanical power chiefly depends on the velocity with which a body is endowed in the instant of exerting it; or that the momentum, or force of a body, must be computed by multiplying the quantity of matter into the velocity with which it moves. Thus, if this new compound of dephlogisticated and æther air expands with 10 times greater velocity than any other inflammable explosive air, its force will be about 10 times greater.

As it seems to be probable, from what is already said, that this compound of explosive air may be put to more uses than that of an amusing experiment, I think it worth while, for men engaged in this branch of natural philosophy, to look out for a method of producing at pleasure any quantity of dephlogisticated air required. Considering the rapid progress which is daily made on the important subject of air, I cannot but flatter myself that this great discovery is not far off. The benefit which would arise from such a discovery for animal life, must encourage every philosopher to pursue this object. Indeed, if we consider that nitre contains this wonderful aerial fluid in a most concentrated state, and that the nitrous acid seems to be nothing else but this beneficial fluid combined with phlogiston, which seems to be imbibed by the vegetable alkali, when the acid is expelled by heat in the form of this air; that this beneficial aerial fluid exists also, in a most concentrated state, in bodies almost every where to be found, as are calces of metal, principally that of iron; that common water contains it in great abundance, so that the light and warmth of the sun extracts it to $\frac{1}{15}$ of the bulk of the water, as Dr. Priestley found, that even the mass of our atmosphere is nothing else but this very air soiled with impurities. If we consider, I say, all this, is it not reasonable to hope, that we are near the important instant, when this salubrious aerial fluid will be procured for many useful purposes in a sufficient quantity, either by the discovery of a ready way to let loose this air from the bodies in which it is as it were imprisoned, or by filtrating or purifying common air from its impurities?

XXVII. The Description of Two New Micrometers. By Mr. Ramsdèn, Optician. p. 419.

When an observer finds, that with the micrometer, which depends on move-

able parallel wires, he cannot measure any diameter of a planet, except that which is at right angles to the direction of its apparent motion, he cannot withhold his preference to that construction which measures the angle by the separation of the images. It appeared therefore a matter of some importance, to investigate the causes of the uncertainty which has been found in the observations made with the micrometer with a divided object-glass. The result of my examination convinced me, that were it possible to execute the construction of that micrometer with the degree of accuracy required, it must still be subject to inaccuracy from its principle.

By the position of the micrometer, every error of its glass is magnified by the telescope; and if each surface of the micrometer glass has not, in every part, precisely the same radius, which opticians must allow to be exceedingly difficult to give, there will be a considerable error in the angle to be measured; and the eye applied to the different parts of the pencil will, without moving the micrometer, see the images of the object in the telescope fluctuating, sometimes appearing to overlap, and sometimes to separate from each other. But supposing the glass itself to be perfect in its substance and in its curvature, there will still remain imperfections which arise from its principle. A micrometer glass applied to a telescope causes a very considerable aberration. If the focus of the glass is positive, the extreme aberration will be within the geometrical focus; if negative, it will be beyond it; and the aberration not only affects the distinctness of the image, but also the angle measured by the micrometer.

At the time I took up this subject, the divided object-glass micrometer was the only one which measured angles by the separation of 2 images. Since that time, a very ingenious application of the prism to this purpose has been invented by the Rev. Dr. Maskelyne, Astronomer Royal; and though experience has not yet ascertained the extent of its merit, it will always deserve great consideration from its ingenuity; but the more I considered the subject, I became more fully convinced, that the principle of reflection applied to micrometers would have great advantages over those hitherto constructed on the principle of refraction; and the catoptric micrometer I have the honour to describe, besides the advantage it derives from the principle of reflection, of not being disturbed by the heterogeneity of light, avoids every defect of other micrometers, and can have no aberration, nor any defect which arises from the imperfection of materials, or of execution, as the extreme simplicity of its construction requires no additional mirrors or glasses to those required for the telescope: and the separation of the image being effected by the inclination of the 2 specula, and not depending on the focus of any lens or mirror, any alteration in the eye of an observer cannot affect the angle measured. It has, peculiar to itself, the advantages of an adjustment to make the images coincide in a direction perpendicular to that of their motion; and also of measuring the diameter of a planet on both sides

the zero, which will appear no inconsiderable advantage to observers who know how much easier it is to ascertain the contact of the external edges of two images, than their perfect coincidence. A short explanation of the annexed drawings will make the construction and the properties of this micrometer obvious.

I divided the small speculum of a reflecting telescope, of Cassegrain's construction, into 2 equal parts, by a plane across its centre; and by inclining the halves of the speculum to each other, on an axis at right angles to the plane that separated them, I obtained 2 distinct images. The satisfaction I received on the first trial was checked by the apparent impossibility of reducing this principle to practice. The angular separation of the 2 images in this case being half the angular inclination of the 2 specula, it required an index of an unmanageable length, to allow the quantity of 1 second of a degree to become visible. Some time afterwards, on revising the principle, I considered, that if both the halves of the mirror turned on their centre of curvature, there could be no alteration in their relative inclination to each other from their motion on this centre; and that any extent of scale might be obtained, by fixing the centre of motion at a proportional distance from the common centre of curvature. This will be better understood from the annexed drawing.

In fig. 8, pl. 5, A represents the small speculum divided into 2 equal parts; one of which is fixed on the end of the arm B; the other end of the arm is fixed on a steel axis x, which crosses the end of the telescope c. The other half of the mirror A is fixed on the arm D, which arm at the other end terminates in a socket y, that turns on the axis, x; both arms are prevented from bending by the braces aa. G represents a double screw, having one part e cut into double the number of threads in an inch to that of the part g: the part e having 100 threads in 1 inch, and the part g 50 only. The screw e works in a nut F in the side of the telescope, while the part g turns in a nut H, which is attached to the arm B; the ends of the arms B and D, to which the mirrors are fixed, are separated from each other by the point of the double screw pressing against the stud h, fixed to the arm D, and turning in the nut H on the arm B. The 2 arms B and D are pressed against the direction of the double screw eg by a spiral spring within the part n, by which means all shake or play in the nut H, on which the measure depends, is entirely prevented. From the difference of the threads on the screw at e and g, it is evident that the progressive motion of the screw through the nut will be half the distance of the separation of the 2 halves of the mirror, and consequently the half mirrors will be moved equally in contrary directions from the axis of the telescope c.

The wheel v fixed on the end of the double screw has its circumference divided into 100 equal parts, and numbered at every 5th division with 5, 10, &c.

to 100, and the index *i* shows the motion of the screw with the wheel round its axis, while the number of revolutions of the screw is shown by the divisions on the same index. The steel screw *k* may be turned by a key, and serves to incline the small mirror at right angles to the direction of its motion. By turning the finger head of a screw, the eye tube is brought nearer to or farther from the small mirror, to adjust the telescope to distinct vision; and the telescope itself has a motion round its axis, for the conveniency of measuring the diameter of a planet in any direction. The inclination of the diameter measured with the horizon, is shown in degrees and minutes, by a level and vernier on a graduated circle, at the breech of the telescope.

The method of adjusting and using the catoptric micrometer is too obvious to require any explanation: it is only necessary to observe that, besides the table for reducing the revolutions and parts of the screw to minutes, seconds, &c. it may require a table for correcting a very small error which arises from the eccentric motion of the half mirrors. By this motion their centres of curvature will, when the angle to be measured is large, approach a little towards the large mirror; the equation for this purpose in small angles is insensible, but when angles to be measured exceed 10 minutes, it should not be neglected. Or, the angle measured may be corrected by diminishing it in the proportion the versed sine of the angle measured, supposing the eccentricity radius, bears to the focal length of the small mirror.

The telescope to which the catoptric micrometer is applied, is of the Cassegrain construction. The great speculum is about 22 inches focus, and bears an aperture of $5\frac{1}{2}$ inches, which is considerably larger than those of the same focal length are generally made: indeed the apparent utility of this micrometer makes me wish to see the reflecting telescope meet with further improvements. I believe it would tend more to the advancement of the art of working mirrors, if writers on this subject, instead of giving us their methods of working imaginary parabolas, would demonstrate the properties of curves for mirrors which, placed in a telescope, will show images of objects perfectly free from aberration; or, what will be still more useful in practice, of what forms specula might be made, that the aberration caused by one mirror may be corrected by that of the other. If mathematicians assume data which really exist, they must see, that when the 2 specula of a reflecting telescope are parabolas, they cause a very considerable aberration, which is negative, that is, the focus of the extreme rays is longer than those of the middle ones. If the large speculum is a parabola, the small one ought to be an ellipse; but when the small speculum is spherical, which is generally the case in practice, if concave, the figure of the large speculum ought to be an hyperbola; if convex, the large speculum ought to be an ellipse, to free the telescope from aberration.

This will be easier understood by attending to the positions of the 1st and 2d images; when a curve is of such a form that lines drawn from each image, and meeting in any part of the curve, make equal angles with the tangent to the curve at that point, it is evident, that such curve will be free from aberration. This is the property of a circle when the radiant and image are in the same place; but when they recede from each other, of an ellipse, of such form that the radiant and image are in the two foci, till, one distance becoming infinite, the ellipse changes into a parabola, and to an hyperbola when the focus is negative; that is, when reflected rays diverge, and the focus is on the opposite side of the mirror. These principles made me prefer Cassegrain's construction of the reflecting telescope to either the Gregorian or Newtonian. In the former, errors caused by one speculum are diminished by those in the other.

From a property of the reflecting telescope, which has not been attended to, that the apertures of the 2 specula are to each other very nearly in the proportion of their focal lengths, it follows, that their aberrations will be to each other in the same proportion, and these aberrations are in the same direction, if the 2 specula are both concave; or in contrary directions, if one speculum is concave, and the other convex. In the Gregorian construction, both specula being concave, the aberration at the 2d image will be the sum of the aberrations of the 2 mirrors; but in the Cassegrain construction, one mirror being concave, and the other convex, the aberration at the 2d image will be the difference between their aberrations. By assuming such proportions for the foci of the specula as are generally used in the reflecting telescope, which is about as 1 to 4, the aberration in the Cassegrain construction will be to that in the Gregorian as 3 to 5. I have mentioned these circumstances in hopes of recommending the demonstration of curves, suited to the purposes of optics, to the attention of mathematicians, which would be of great use to artists.

I shall conclude this paper with the description of a new micrometer suited to the principle of refraction; being sensible that both principles have their peculiar advantages. Though the former part of this paper proves my partiality to the principle of reflection applied to micrometers, yet the very favourable opinion I have of the refracting telescope made me attentively consider some means of applying a micrometer to it, which might obviate the errors complained of in the former part of this paper. The application of any lens or medium between the object-glass and its focus must inevitably destroy the distinctness of the image; I therefore have employed, for the micrometer-glass, one of the eye-glasses requisite in the common construction of the telescope; but if it should be found necessary to apply an additional eye-glass, for the conveniency of enlarging the scale, I am able by it to correct both the colours and spherical aberration of the first eye-glass.

"This micrometer is applied to the erect eye tube of a refracting telescope, and is placed in the conjugate focus of the first eye-glass: hence arises its great superiority to the object-glass micrometer. It has been before observed, that if a micrometer is applied at the object-glass, the imperfections of its glass are magnified by the whole power of the telescope; but in this position, the image being considerably magnified before it comes to the micrometer, any imperfection in its glass will be magnified only by the remaining eye-glasses, which in any telescope seldom exceeds 5 or 6 times. By this position the size of the micrometer-glass will not be the 100th part of the area which would be required if it was placed at the object-glass; and, notwithstanding this great disproportion of size, which is of great moment to the practical optician, the same extent of scale is preserved, and the images are uniformly bright in every part of the field of the telescope.

Fig. 10 represents the glasses of a refracting telescope; *xy* the principal pencil of rays from the object-glass *O*; *tt* and *uu* the axes of 2 oblique pencils; *a* the 1st eye-glass; *m* its conjugate focus, or the place of the micrometer; *b* the 2d eye-glass, *c* the 3d, and *d* the 4th, or that which is nearest the eye. Let *p* be the diameter of the object-glass, *e* the diameter of a pencil at *m*, and *f* the diameter of the pencil at the eye; it is evident, that the axis of the pencils from every part of the image will cross each other at the point *m*; and *e*, the width of the micrometer-glass, is to *p* the diameter of the object-glass, as *ma* is to *go*, which is the proportion of the magnifying power at the point *m*; and the error caused by an imperfection in the micrometer-glass placed at *m*, will be to the error had the micrometer been at *O*, as *m* is to *p*.

Fig. 9, represents the micrometer; *A* a convex or concave lens, divided into 2 equal parts by a plane across its centre; one of these semi-lenses is fixed in a frame *B*, and the other in the frame *E*, which 2 frames slide on a plate *H*, and are pressed against it by thin plates *aa*: the frames *B* and *E* are moved in contrary directions by turning the button *D*; *L* is a scale of equal parts on the frame *B*; it is numbered from each end towards the middle with 10, 20, &c. There are 2 verniers on the frame *E*, one at *M*, and the other at *N*, for the conveniency of measuring the diameter of a planet, &c. on both sides of the zero. The first division, on both these verniers, coincides at the same time with 2 zeros on the scale *L*, and, if the frame is moved towards the right, the relative motion of the 2 frames is shown on the scale *L* by the vernier *M*; but if the frame *B* be moved towards the left, the relative motion is shown by the vernier *N*.

This micrometer has a motion round the axis of vision, for the conveniency of measuring the diameter of a plant, &c. in any direction, by turning an endless screw *F*, and the inclination of the diameter measured with the horizon is shown on the circle *g* by a vernier on the plate *v*. The telescope may be

adjusted to distinct vision by means of an adjusting screw, which moves the whole eye tube with the micrometer nearer to or farther from the object-glass, as telescopes are generally made; or the same effect may be produced in a better manner, without moving the micrometer, by sliding the part of the eye tube on the part n, by help of a screw or pinion. The micrometer is made to take off occasionally from the eye tube, that the telescope may be used without it.

XXVIII. Of the Airs extracted from different Kinds of Waters; with Thoughts on the Salubrity of Air at different Places. By the Abbé Fontana. p. 432.

The following experiments were made at Paris in 1777 and 1778 on the air extracted from various kinds of waters.

Mr. F. extracted the air from the water of a well by means of common fire. The water was then made to boil in a large matrass of tin, which had a long tube of the same metal, which being bent into 2 different directions was with its extremity immersed in a tub of cold water. The matrass and its tube were entirely filled with water: the air which came out of it was received into 3 different vessels. The air of the 1st vessel, by being shaken in water, was diminished a little; the air of the 2d was diminished of half its bulk, or rather more; and the air of the 3d vessel was diminished exceedingly. The residuums of air that remained unabsorbed were more or less phlogisticated.

Another time Mr. F. obtained almost entirely fixed air, excepting a little which remained unabsorbed by water, and was partly phlogisticated. A 3d time the air of the well water was made to pass through mercury into a tube anointed with oil of tartar, and it occasioned a crystallization just like that which the purest fixed air is used to do. A 4th time he impregnated with this air a quantity of common water, which absorbed its own bulk of it, and became by these means acidulous, exactly like water with the purest fixed air. This water turned the tincture of turnsole red, and precipitated the lime in lime water. Another time a light was successively extinguished, and a bird died instantly in this air.

The water of the river Seine, filtrated through sand, as it is drank at Paris, was treated in the same manner as the well water. The air extracted from that water was $\frac{1}{2}$ absorbed by water, when shaken in it; the remainder, when treated with the test of nitrous air, gave $11 - 4$, $11 + 1$;^{*} when the common atmospheric air treated with the same nitrous air gave $11 - 4$, $11 + 8$. It was therefore sensibly better than the atmospheric air, which during 3 years of experiments made at Paris, he constantly found to be inferior to the air of the Seine water, extracted as above. Having repeated the experiment, he received the air into 2 different receivers. The 1st of which, by being shaken in water, was dimi-

* See p. 529 of this Abridgement, for an explanation of this measure.

nished in the proportion of 10 to 7; and by the test of nitrous air gave II — 14, II + 1, III + 1, when the common air gave II — 12, II + 6, III + 6. The 2d quantity of air was diminished in the proportion of 3 to 1; and when examined by the test of nitrous air gave II \pm 0, III \pm 0. Whence it may be concluded, that the 1st air was better than the atmospheric air; whereas the 2d was worse, and mixed with much fixed air.

Being in doubt whether the tin vessel employed in the experiment above-mentioned might not alter the nature of the air, &c. he made use of glass vessels. Having therefore filled 1 of these vessels, having a long neck bent in 2 directions, with the Seine water, he obtained some air which seemed not sensibly diminished when shaken in water. Having introduced 1 measure and 37 parts of this air into the tube used to try the diminutions, it gave with the nitrous air 1 + 19, 1 + 48, when the same quantity of nitrous and atmospheric air gave II + 26, II + 6: it is therefore certain, that the air extracted from Seine water is purer than common air. Another time he extracted, in the same manner, and from the same water, the air; 1 measure and 24 parts of which being introduced into the tube, &c. and shaken, was reduced to 1 measure — 31 parts, that is, $\frac{1}{5}$ of it was absorbed. Treated with the nitrous air it gave I — 4, when equal measures of common and nitrous air gave I \pm 0: it was therefore better than common air. A 3d time he extracted the air from the water of the river Seine, contained in 3 matrasses; this air was about $\frac{1}{8}$ of the bulk of the water, and it gave with the test of nitrous air, II — 14, II — 9, III — 9; when the common air mixed with nitrous air, as usual, gave II — 14, II + 8, III + 8. It is therefore clear, that the air extracted from the Seine water, by the action of fire in glass vessels, is much better than common air, or than the air which is extracted from the same water when boiled in tin vessels.

Another time he filled a glass retort, which had a long and doubly bent neck with Seine water. The water weighed about 3 lb. The air that came out of it lost $\frac{1}{4}$ of its bulk by being shaken in water; and afterwards being tried with nitrous air it gave II — 16, II — 16, III — 16, when common and nitrous air gave II — 12, II + 12. This experiment being repeated, the air was diminished of $\frac{1}{4}$ by being shaken in water. One measure — 16 parts of this air introduced into the measuring tube gave II — 32, II — 2, when common air and the nitrous gave II — 28, II + 4.

The water d'Arquail at Paris is considered as very pure. Mr. F. filled the tin vessel with it, and received the air that came out of it into 3 vessels. Being shaken in water, the 1st of them was diminished $\frac{1}{5}$; the 2d, $\frac{3}{4}$; and the 3d $\frac{1}{8}$, by the operation in water. A light burned with a flame, more luminous than in common air, in the 1st air after it had been shaken in water. This air being tried with the nitrous air gave II — 10, II — 10, III — 10. The 2d gave II — 10,

II — 17, III — 30, when common and nitrous air gave II — 2, II + 14, III + 14. The 3d air, before it was shaken in water, crystallized with the oil of tartar like fixed air. An equal bulk of it was absorbed by water, which by this mixture became acidulous; it precipitated the lime in lime water, extinguished a light several times, and killed an animal instantly. It is therefore partly fixed air, and partly air which is not only better than common air, but likewise than that extracted from Seine water, even when this last has been boiled in glass vessels. The experiments being repeated with the same water of Arqueil, but in glass vessels, the air obtained, after being shaken in water, was much better than that obtained from the same matter, when boiled in vessels of tin.

He also extracted the air from distilled water in glass vessels, and having shaken 1 measure — 32 parts of it in water, it was reduced to 1 measure — 35 parts. With the test of nitrous air it gave I — 6, when equal parts of common and nitrous air gave I — 2, which showed that it was better than common air. He extracted the air again in the manner above described; but it was not sensibly diminished when shaken in water. Two measures — 49 parts of it, with the test of nitrous air, gave I — 2, I + 8, when common air, &c. gave I — 1, I + 18: It is therefore better than common air.

He extracted the air from a great quantity of distilled water in the usual manner, and found that it did not sensibly diminish in water. With nitrous air it gave II — 14, II — 25, II + 25, when common and the same nitrous air gave II — 14, II + 10, III + 10; consequently it was dephlogisticated air, viz. purer than the air of the Seine and Arqueil, which are much better than common air.

He tried whether any difference would arise from boiling distilled water in a matrass of tin, instead of glass vessels; and found that the 1st air was diminished $\frac{1}{6}$ by being shaken in water, and afterwards with the nitrous air gave II — 13, II — 16, III — 18, when common air gave II — 12, II + 8: which shows that it was dephlogisticated air, but not so good as that extracted from the same water when boiled in glass vessels. The 2d quantity of air was not sensibly diminished in water, and with nitrous air gave II — 13, II — 20, III — 30; that is, it was more dephlogisticated than the first.

The air extracted from distilled water, is to that extracted from the water of the river Seine, as 13 to 32 nearly; whence distilled water does not give more air than $\frac{1}{6}$ of its bulk: but as the air extracted from the water of the Seine is $\frac{1}{2}$ fixed air, it may be concluded, that the quantity of respirable air produced by both kinds of waters is nearly the same, and that they only differ a little in purity. It is however true, that other experiments had shown him that water in general absorbs about twice as much of dephlogisticated as of common air; for which reason, he thinks that the respirable air of Seine water is rather less than

that of distilled water. Accordingly he found that Seine water, after it had been boiled for a long time, absorbed in 40 days about $\frac{1}{14}$ of its own bulk of dephlogisticated air, when in the same length of time it does not absorb more than $\frac{1}{2}$ of common air. This seems to be an experiment of very great consequence, as it discovers a new characteristic by which dephlogisticated air may be distinguished from common air; and shows that water absorbs a greater quantity of those kinds of air, which contain a less quantity of phlogiston.

However it is impossible to determine exactly the quantity of air that is extracted from vessels filled with water, by means of fire; because a portion of the air is absorbed by the water of the tub in the act of its coming forth. It will certainly be more exact to receive the air in vessels immersed in quicksilver; but then there are many other inconveniencies to encounter.

The above experiments are very useful in explaining the reason why some kinds of water have a peculiar sharp taste more than others; and especially why some of them precipitate the lime in lime water, rendering it a calcareous earth, and change the tincture of turnsole into a red colour, as he had generally experienced with the well waters at Paris. Hence may also be explained why some kind of waters can dissolve iron, and keep it in solution without deposition; whereas other kinds of water are incapable of doing it, at least do it much less than the purest distilled water. This is soon discovered by boiling the water, which will then deposit the iron which before was dissolved.

Mr. F. had not only extracted from waters the different kinds of air they contained naturally, but had also made various experiments on waters deprived of air, which, being exposed, had again imbibed the atmospherical air, as above hinted. He had determined the quantity and quality of those airs. In general, he might say, that distilled water, deprived of air, imbibes again an equal quantity of air of the same kind as that it had lost, and that in less than 50 days. Other kinds of water do the same, but with this difference, that the air they absorb, after being boiled, is better than that they had lost; and in this particular they come very near to the nature of distilled water itself.

By means of pure water, especially distilled water, common air may be changed into dephlogisticated air, that is, into air much more salubrious than the best common air which we breathe; and this, for what he knows, is the only means of meliorating common air: for all the artificial methods (great numbers of which he had tried) had proved either useless or noxious, but never such as promised to be of any great utility to mankind.

Though he had long thought of applying those experiments to some use for the purposes of life, the want of time and a proper apparatus had hitherto hindered him doing any thing; he now began to hope to be able to do something. In the mean time he thought it of some importance to be known, that

water not only possesses the property of diminishing the noxious part of tainted air, but has also the power, and that in a very high degree, of dephlogisticating common air; which must certainly be one of the methods by which nature keeps the atmosphere in a state constantly fit to support animal life; it being certain, that the water in various circumstances, must lose either a part or the whole of that air which it hath absorbed from the atmosphere.

It might be suspected, that in these experiments of extracting the air from water by the action of fire, the air might be considerably altered by the vapour of the water itself. As this difficulty was of some force, he endeavoured to remove it in the following manner. He introduced into a tube, through water, a quantity of common air of known goodness, and he caused the steam of water boiling in a matrass, from which the air had been previously extracted, to pass through it. The heat of the steam sometimes made the water occupy above 5 times the space it did when cold; yet the air so treated was not at all altered by it, as appeared by the test of nitrous air. The event of the experiment, repeated at various times, was constantly the same.

He observes, lastly, that having once caused the air of boiling water to pass into receivers filled with, and standing in, quicksilver, he found that the air was better than usual. He had observed the same thing when he caused the air to go through distilled water into receivers filled with it: which observation, if the event of the experiment should be constantly the same, would induce him to believe, that the air loses some of its good properties by going through water not very pure; or, which seems to be rather more probable, that a quantity of air less good is, by the action of the vapours and the heat, extricated from that impure water, and is mixed with the air that comes out of the matrass, whence this air is debased.

Mr. F. mentions a new character of equal importance with that which distinguishes dephlogisticated from common air. This new character has been equally unknown, and deserves the attention of philosophers, because at the same time it discovers a new property of the atmospherical air, which he should never have suspected if experience had not offered it to him. He had found, that common air shaken in water, instead of being diminished, is sensibly increased in its bulk. The increased space is in proportion to the time the air is shaken in water, and it begins to be sensible even from the beginning, that is, after a few seconds. This augmentation he had sometimes brought to be $\frac{1}{12}$ of the bulk of the air, and even more: it must however be confessed, that he met with great variety in the experiments of this kind made at different times. After the bulk of the air shaken in water is increased to a certain degree, it then begins to decrease continually; and, in proportion to this decrease, the air becomes gradually less good. When the experiment is tried in close vessels, the diminution cannot be observed.

If dephlogisticated air be shaken in the tube, in the manner above-mentioned, not only it does not increase its bulk, but it begins to diminish from the very beginning of the operation, and it continually loses more and more of its bulk, and with its bulk of its purity. This last mentioned property of dephlogisticated air seems to show, that this is a fluid very different from common air, because it has its peculiar properties by which it differs from common air, not from more to less only, but entirely; as is shown by the property this fluid has of being absorbed by water; whereas common air receives an increase of bulk and elasticity by being shaken in water.

All that he had been saying above, in order to give an idea of his method, and the words he uses to express the diminutions made by the mixture of nitrous air and other kinds of respirable air, is not sufficient to obtain results constant and certain, so as to deduce any consequences from them. Even after all the elements are corrected, and all the causes of error hitherto unknown or neglected by the most diligent observers, are avoided, it is absolutely necessary to follow always a constant and equal method, not only in the act of introducing the various kinds of air into the tube, but also after the mixture of the 2 kinds of air. He had not the least hesitation in asserting, that the experiments made to ascertain the salubrity of the atmospherical air in various places, in different countries and situations, mentioned by several authors, are not to be depended on; because the method they used was far from being exact, the elements or ingredients for the experiment were unknown and uncertain, and the results very different from each other.

When all the errors are corrected, it will be found, that the difference between the air of one country and that of another, at different times, is much less than what is commonly believed, and that the great differences found by various observers are owing to the fallacious effects of uncertain methods. This he advances from experience; for when he was in the same error, he found very great differences between the results of the experiments of this nature which ought to have been similar; which diversities he attributed to himself rather than to the method he then used. At Paris he examined the air of different places at the same time, and especially of those situations where it was most probable to meet with infected air, because those places abounded with putrid substances and impure exhalations; but the differences he observed were very small, and much less than what could have been suspected, for they hardly amounted to $\frac{1}{50}$ of the air in the tube. Having taken the air of the hill called Mont Valerien, at the height of about 500 feet above the level of Paris, and compared it with the air of Paris taken at the same time, and treated alike, he found the former to be hardly $\frac{1}{50}$ better than the latter. In London he had observed nearly the same. The air of Islington and that of London suffered an equal diminution by the mixture of ni-

trous air; yet the air of Islington is esteemed to be much better. He examined the air of London, taken at different heights; for instance, in the street, at the 2d floor, and at the top of the adjoining houses, and found it to be of the same quality. Having taken the air at the iron gallery of St. Paul's cupola, at the height of 313 feet above the ground, and also the air of the stone gallery which is 111 feet below the other; and having compared these 2 quantities of air with that of the street adjoining, he found that there was scarcely any sensible difference between them, though taken at such different heights.

In this experiment a circumstance is to be considered, which must have contributed to render the above-mentioned differences more sensible; this is, the agitation of the air of the cupola, for there was felt a pretty brisk wind on it, which he observed to be stronger and stronger the higher he ascended; whereas in the street, and indeed in all the streets he passed through, there was no sensible wind to be felt. This experiment was made at 4 in the afternoon, the weather being clear. The quicksilver in the barometer at that time was 28.6 inches high, and Fahrenheit's thermometer stood at 54° . After having related all these circumstances, it will be necessary to give the mean result of all the various experiments made on each of those quantities of air treated after his method with the nitrous air. The air of the street gave $\text{II} - 13$, $\text{II} + 6$; the air of the stone gallery, which was 202 feet high, gave $\text{II} - 14$, $\text{II} + 5$; and the air of the iron gallery, which was 313 feet high, gave $\text{II} - 14$, $\text{II} + 5$. The results of the last 2 experiments are exactly the same; and that of the first is hardly at all different from them. From this is clearly seen how little the experiments hitherto published, about the difference of common air, are to be depended on. In general, the air changes from one time to another; so that the differences between them are far greater than those of the airs of different countries, or different heights; for instance, he found that the air of London, in the months of Sept. Oct. and Nov. 1778, when treated with the nitrous air, gave $\text{II} - 6$, $\text{II} + 15$, which is a mean result of many experiments which differed very little from each other. The 26th of Nov. last, he found the air for the first time much better, for it gave $\text{II} - 12$, $\text{II} + 12$; but the 14th of Feb. the air gave $\text{II} - 18$, $\text{II} + 7$ whence it appears, that the air of this 14th of Feb. was better than it had been for 6 months before. There can be no doubt of the accuracy of the experiments, because he compared the air taken at different times with that which he had first used in the month of Sept., and which he had preserved in dry glass bottles accurately stopped. Now if the formulæ expressed above are compared together, it will be found, that the difference between the first terms is of 12 parts, and that between the latter of 7; that is, of $\frac{1}{10}$ and $\frac{1}{8}$ of the whole quantity of air: which are much greater differences than those above-mentioned. Yet he could not perceive any particular change of health, or facility of breath-

ing, arising from those changes of the salubrity of the atmospherical air; and he was informed, that no particular diseases appeared which could indicate any remarkable change of air.

Nature is not so partial as is commonly believed. She has not only given us an air almost equally good every where and at every time, but has allowed us a certain latitude or a power of living and being in health in qualities of air which differ to a certain degree. By this he did not mean to deny the existence of certain kinds of noxious air in some particular places; but only meant, that in general the air is good every where, and that the small differences are not to be feared so much as some people would make us believe. Nor did he mean to speak here of those vapours and other bodies which are accidentally joined to the common air in particular places, but do not change its nature and intrinsic property. This state of the air cannot be known by the test of nitrous air, and those vapours are to be considered in the same manner as we should consider so many particles of arsenic swimming in the atmosphere. In this case it is the arsenic, and not the degenerated air, that would kill the animals who ventured to breathe it.

In this place therefore he did not mean to speak of those changes which do not immediately alter the nature of the air itself. The other states of that fluid are of another kind, and they are not to be examined by means of nitrous or inflammable air; the uses of which last, he means to show on another occasion. The same thing may be said of those vapours or particles which may be good for respiration, and do not change the nature of the air. Some vegetables, for instance, can diffuse through the air such exhalations as may be of real use to the animal economy, when they are breathed for a long time, or imbibed by the pores of the skin. He remembered to have often put various flowers, as roses, pinks, &c. in vessels full of common air confined by quicksilver, where he left them for several hours; after which time he found, that the air was not at all altered, but that various animals seemed to breathe it very well, notwithstanding the flowers had filled the greatest part of the vessels. On the contrary, he had found, that the vapours arising from lime slacked in water, either do not alter the air at all, or very little; though when breathed with the air, they occasion the death of animals.

He would not have any one suppose, that he thought it of little importance to know the goodness of the atmospherical air, and the changes it undergoes. On the contrary, he believed it to be a very useful inquiry for mankind, because we do not yet know how far one kind of air more than another may contribute to a perfect state of health; nor at what time small differences may become very considerable, when one continues to breathe the same kind of air for whole years, especially in some kind of diseases. An exact method of examining the good-

ness of common air may even be useful to posterity, in order to ascertain whether our atmosphere degenerates in a length of time. This curious inquiry, together with the method, &c. are the production of the 18th century; and our descendants must have some gratitude for the philosophers who found out, as well as for those who improved it. If our ancestors had known and transmitted it to us, we should perhaps at present be able to judge of one of the greatest changes of our globe, of a change which very nearly interests human life.

XXIX. Of some Experiments in Electricity. By Mr. Wm. Swift. p. 454.

One particular addition Mr. S. had made to his apparatus, consists in what he calls an anti-conductor: it is exactly like the prime conductor; but is fixed to the cushion of the machine, and consequently, when the cylinder is put in motion, the anti-conductor is charged negatively, that is, the electric matter is diminished in it, in the same proportion as it is increased in the prime conductor in the same time. Another thing peculiar to this machine is, that the whole is insulated; so that being able to collect the electric matter without any connection with the earth, and having at the same time bodies or conductors positively and negatively electrified, he was enabled by this apparatus to exhibit many experiments more analogous to the natural effects of lightning from the clouds, than it is possible to do with only one conductor positively electrified; because in nature clouds are constantly flying in the air which are differently electrified, and, discharging themselves into each other, produce the lightning often seen in the atmosphere.

It may be proper to mention a few common experiments and observations, to show that the 2 conductors are differently charged, that is, the one positively or plus, the other negatively or minus, as soon as the cylinder is put in motion by turning the wheel.

1. When the cylinder moves, and a body approaches the prime conductor, such body will draw a spark from that conductor at the same distance, and consequently of the same length, as will be drawn from the approaching body by the anti-conductor. And a pith ball is equally attracted by both. Which sufficiently shows that both conductors are charged or electrified.

2. The following common experiments will show, that they are differently electrified. Take a wire with a small piece of cocoa wood, about $1\frac{1}{2}$ inch long, pointed, fastened to one end of the wire, and connecting the other end to the anti-conductor; as soon as a conducting body approaches it, there is a bright spark resembling a star, which appears to settle on the end or point of the wood; but when the wire is connected with the prime conductor, there issues from the end of the wood a pencil of rays diverging to the point towards the approaching body; which, Mr. S. apprehends, demonstrates the conductors to be differently electrified.

3. It may also be seen by another experiment. Take 2 jars, coated as in the Leyden experiment, and charge one by the prime conductor, the other by the anti-conductor; the 1st will be positively, and the 2d negatively electrified; which is proved by applying a discharging rod to the balls connected with the inside of the jars, when both immediately discharge themselves, which they would not do, if both jars were charged from the same conductor.

Some few more experiments are added, to the same effect: after which Mr. S. adds the following conclusion.

Thus, I humbly apprehend, the whole current of these experiments tends to show the preference of points to balls, in order to diminish and draw off the electric matter when excited, or to prevent it from accumulating; and consequently the propriety, or even necessity, of terminating all conductors with points, to make them useful to prevent damage to buildings from lightning. Nay the very construction of all electrical machines, in which it is necessary to round all the parts, and to avoid making edges and points which would hinder the matter from being excited, will, I imagine, on reflection, be another corroborating proof of the result of the experiment themselves.

XXX. Description of the Sitodium Incisum and Macrocarpon, with their Fruits.

By Charles Peter Thunberg, M. D. From the Latin. p. 462.

The authors who have described the bread tree or its fruit, are but few in number, viz. Rheede in the *Hortus Malabaricus*. Rumphius in his *Herbarium Amboinense*. Zanoni in his *Historia Stirpium rariorum*. Hawksworth in his *Voyages round the World*. Mr. John Ellis in his *Description of the Mangostan and Bread-Fruit Tree*. Lond. 1775, 4to. The two Forsters, father and son, viz. John Reinhold, the father, and George the son, have given the generic characters, with a figure, in their work entitled *Characteres generum plantarum*, &c. Lond. 1776. 4to.

I myself (says Dr. T.) while at Batavia, described, according to the sexual system, the species of this genus, under the names of *Radermachia incisa*, and *R. integra*, in the Stockholm Transactions for 1776, and in the same year sent the seeds of the *Sitodium macrocarpum* to the Amsterdam physic-garden. Afterwards, on my return from Japan, in 1777, I sent some very young living plants of both species to the same garden from Batavia; and lastly, on my return to Europe, in 1778, I brought with me a great number of both species, from Ceylon, with the seeds of one species; some in papers included in a box, and sometimes exposed to the air in the shade; some in glass jars well secured; some in wax, and some in dry sand: these last 2 methods are the best. Every month, during the voyage, I planted some of the seeds in mold, in order that they might have a chance of vegetating in a different air, climate, and season.

The *Generic Description* of these trees is as follows :—

Male Flowers.—*Cal.* None. *Spadix* cylindric, or subclavated, gradually thickened, and covered on all sides with flowers.—*Cor.* *Petals* two, oblong, concave, obtuse, villose, including the filament.—*Stam.* *Filament* single, within each corol, filiform, diaphanous, length of corol. *Anther* pyramidal.

Female Flowers.—*Cal.* None. *Pericarp* ovate, covered with germens.—*Cor.* None.—*Pist.* *Germens* convex, very numerous, hexagonal.—*Style* filiform, permanent.—*Stigma* one, or two, capillary, revolute, a line in length.—*Pericarp.* Berry, ovate, muricated, fleshy, multilocular. *Seeds* imbricated in a multiple series, ovate, obliquely triquetrous, involved in a soft flesh.

Generic Character.—♂. *Cal.* None. *Corol.* Dipetalous.—♀. *Cal.* None. *Corol.* None. *Style* one. *Berry* Multilocular.

Specific Descriptions.—1. *Sitodium incisum*. *S. foliis incisis, ramulis floriferis*. Bread-fruit. *Hawksw. Voy. vol. 2. p. 18. pl. 11.*

This species grows naturally in the islands of Amboyna, Banda, and the other Moluccas. In Java, about the city of Batavia, it is common enough, as well as in the Maldives, &c. and is cultivated in Malabar, at Surat, at Cape Comorin; in Coromandel, at Succotor, Tranquebar, and Nagapatnam; and in Ceylon about Columbo, Gale, Matura, Jafna, Trinquimale, the Marian Isles, &c. It flowers and bears ripe fruit, twice a year, viz. during the first eight months.

The *trunk* is arboreous, upright, of the thickness of a man, and of about the height of five fathoms: the branches are thickly opposite, subverticillato-quaternate, spreading.—*Branchlets* subverticillato-quaternate, floriferous.—*Leaves* alternate, footstalked, oblong, deeply cut into nine very entire lobes, which are villose-rough, and spreading: their colour above is green, with pale nerves: beneath pale: the length of the leaves is two feet, and the breadth one: the younger leaves are plaited, and the smaller ones viscid: the *foot-stalk* is thick, subtriquetrous, villose, and about an inch in length: the young *stipules*, involving the leaves, are two, sessile, lanceolate, acuminate, concave, very entire, smooth within, hirsute without, deciduous, and a palm long.

The *Flowers* grow on the ultimate branchlets, the males and females distinct, on the same branchlet; and are footstalked; the *footstalk* roundish, villose, upright, two inches long, and the thickness of a finger: the *spadix* is a span long, bending, and deciduous. The *Pericarp* is of the size of a child's head, and sterile, or filled with sterile seeds.

2. *Sitodium macrocarpon*. *S. foliis indivisis trifidisve, caudice ramisque floriferis*. Tsjakamaram. *Rheede. Hort. Mal. 3. Jaca Zanoni Hist. Stirp. rar. Jacca sive Jaccas Portugallis.*

This species grows naturally in the Moluccas, as Amboyna, Banda, &c. rather sparingly in Java and Sumatra: in Ceylon common: in Coromandel,

Tranquebar, and Cape Comorin. It flowers and fruits twice a year, during the first eight months. The *stem* is arboreous, upright, the thickness of a man, five fathoms and more in height: the *branches* alternate, spreading; *branchlets* alternate, again ramified, and hirsute with long pile. *Leaves* alternate, footstalked, ovate-oblong, obtuse with an obtuse point, obscurely serrated, undivided, nerved: above of a bright green, very smooth; beneath paler, and bristled with stiff hairs, spreading, and a span long: the younger ones are evidently toothed, the teeth vanishing as the leaves become full-grown: footstalk subtriquetrous, smooth, and an inch long: *stipules* as in the former. Sometimes, but rarely, one or two leaves are cut or lobed: the *flowers* are male and female distinct, on the same bough, or same part of the stem: *footstalk* simple, or branched, pendulous, an inch long, and a foot thick: stalklets three, five, or more, about a finger's length and thickness.—*Spadix* size of a finger, upright, spreading.—*Pericarp* of vast size, weighing thirty pounds and upwards, fertile, and constituting by far the largest of all known berries.—*Seeds* thrice and four times the size of almonds, often two or three hundred in number, ovate-oblong with one extremity acute, the other obtuse, and the sides a little flattened.

Observations relative to both species.—The whole vegetable, both in branches and fruit, abounds with a white, milky, tenacious juice, which cannot be washed away without oil. The outside crust of the fruit is coriaceous, and every where muricated with tubercles: this crust being removed, the seeds come in view, which are involved or inclosed in flesh, and intermixed with various integuments. The fruit of both species when ripe, is of a somewhat disagreeable or nauseous smell, though delicate within.

The *S. macrocarpon* is propagated by seeds, which readily grow; but the *S. incisum*, which is sterile, is propagated by the roots, planted immediately under the surface of the ground.

It should be added, the now established name of this genus is *Artocarpus*.

XXXI. A Second Paper concerning some Barometrical Measures in the Mines of the Hartz. By Mr. J. A. De Luc, F. R. S. From the French. p. 485.

Combining his first observations, Mr. De Luc found that the George gallery was 127.15 lachters, or Hartz fathoms, below the entrance of the mine; and that the whole depth was 215.86 lachters, which makes 801 English feet for the first depth, and 1359 for the latter. Mr. Uslar, having taken a memorandum of the places where the above observations were made, determined the correspondent geometrical measures. The depth of the George gallery he found to be 127.87 lachters, that is, only 4 feet more than had been found by the barometer; and the whole depth was only $215\frac{1}{2}$ lachters; or 2 feet less than that which resulted from his observations. Thus the barometric measurement

gave 4 feet less than the geometrical measure for the former of the depths, and 2 feet more for the latter.

General Observations.—So long as in barometrical measurements we shall consider as given only the differences in the weight and heat of the air at the places of observation, we shall be subject to errors; because there are many other causes of modification in the air: and all the exactness to which we can pretend, will be to determine a formula which preserves a mean among the possible variations. This is, says Mr. De Luc, what I have proposed to do in my own formula, and it seems to answer this end. In the different trials which have been made of it, it has sometimes given the heights too great, at other times too little, without distinction of climate. Thus, for example, the experiments at Spitzbergen by Lord Mulgrave, and at the Pike of Teneriffe by Mr. De Borda, one of the French academicians, gave the heights too great; those of Col. Roy and Sir George Shuckburgh, made in mean latitudes, and partly in the places where I myself had observed, gave them too little.

These differences do not seem to depend on the climate, and indeed I have frequently observed them to happen in the same places. Thus, for example, my observation on the Glaciere de Buet, cited by Sir George Shuckburgh, gives the height of that mountain a little less than the geometrical measure; but Mr. De Saussure having repeated the barometrical observation, it agreed with that measure by the same formula; and Mr. Marc Pictet, by a 3d observation, found the height a little too great. In these 3 observations, the corresponding point was Geneva, distant about 10 or 12 leagues. At that distance there are doubtless some causes of variations which are irremediable; since the formula supposes that the observations are made in the same column of air. It is therefore only in the cases in which that supposition approaches near the truth that we can hope to perfect the rule. But this can only be by introducing new conditions into it; that is, other modifications of the air, of which we have not as yet taken any account.

In meditating on the causes of the diversity of results in experiments, it has always appeared to me, that the differences of the effects of heat on the air, according to the different states of it, was the principal; that is, that the air not being always of the same nature, heat, that grand cause, whose effects we ought principally to determine, does not always act equally. Besides the particular experiments which prove it, we can attribute to these differences only those of the results of the researches of some philosophers concerning the dilatations of the air by heat, applied to various physical uses.

In a paper lately read at the R. S., on the subject of refractions, I analysed and compared different formulæ of this kind, given by philosophers on whom we can depend. The result of that examination was that, supposing the volume of

air = 1000 when the English thermometer of Fahrenheit is at 32° , if the heat of the air be increased 22.8 degrees of this thermometer, its volume will be,

According to Mr. Abbé De La Caille. 1040

Mr. Professor Mayer. 1046

Mr. Bonne. 1047.7

Sir George Shuckburgh. 1050.5

Dr. Bradley. 1054.4

Here then are great differences in this point only, namely, the effect of heat on the density of the air; differences which must have a visible effect in all meteorological phenomena. Doubtless they proceed, in a great measure, from the different degrees of dryness of the air, which we can no longer doubt of, since the interesting experiments of Col. Roy with the manometer. This is the same cause to which I imputed the greatest variations in my experiments in open air, and which obliged me to conclude my correction for the effects of heat on the air from a mean among my numerous observations. Now this mean, reduced to the same term of comparison as above, is 1047, which is also a mean among those different results.

XXXII. On the Precession of the Equinoxes produced by the Sun's Attraction.

By the Rev. Mr. Isaac Milner, M. A., and Fellow of Queen's College, Cambridge. p. 505.

If the actions of the sun and moon on the different parts of the earth were equal; or if the earth itself were perfectly spherical, and of a uniform density from the centre to the surface; in either case the attractions of those remote bodies would have no effect on the position of the terrestrial equator, and the equinoctial points would constantly be the same in the heavens. But it was impossible to give the earth a rotatory motion round an axis without giving at the same time a centrifugal force to its parts. This force is greatest at the equator, and is in a contrary direction to that of gravity; on either side of the equator the force is less; and besides, only part of its effects is opposed to that of gravity. It is usual, in determining the figure of the earth, to consider the whole mass as in a state of fluidity, and the different columns as sustaining each other at the centre. If the earth be considered as a hard body, firmly cohering in its parts by some other force besides that of gravity, it does not seem necessary that the different columns should be supposed to sustain each other at the centre, though in both cases the direction of gravity must at every point of the surface be perpendicular to the tangent of the figure. But we know that there is a considerable quantity of water on the surface of the earth; and therefore, if the equatorial regions were not higher than the polar, they certainly would be overflowed by the ocean, which is contrary to experience; and for this reason the

proportion of the diameters of the earth, determined on the false supposition of an entire fluidity, cannot differ much from the truth.

§ 2. But the precession of the equinoxes, which depends on the unequal actions of the sun and moon on the protuberant parts of the earth at the equator, will not be the same in these different hypotheses; at least we can never be certain that it will be so till we have computed their effects, and the computation itself must proceed on different principles. Suppose the earth to be fluid under the form of an oblate spheroid; or, what is more simple, suppose the region of the equator to be surrounded with a ring of fluid matter; then the unequal action of the sun will disturb the figure of the ring, and communicate a motion to its parts. Suppose we knew the precise disturbing force of the sun on any one particle of this ring, according to its situation; in that case we could easily find the velocity which would be communicated to such a particle in any given time; but the mutual actions of the fluid particles on each other could never be exactly estimated, much less their effects in endeavouring to turn the whole earth round its centre. However, it is easy to see, that in the case of a hard ring of matter cohering close to the surface of the earth at the equator, both the law by which the particles act on each other, and on the whole mass of the earth, will be widely different from the case of fluidity, and the effects much greater in altering the position of the axis of rotation.

To explain this by an easy example, let A, B, c, fig. 1, pl. 6, represent 3 small bodies in the same horizontal line AE. Suppose A to descend by any accelerating force, as gravity; B to descend by the same force, a less or a greater; and c not to be acted on at all: in every one of these cases the bodies A and B will descend with their respective velocities, and the body c will preserve its situation. If A and B are small particles of fluid of any form, and c a hard one, and if the particle A be placed in contact with B, and have its centre of gravity a little above the centre of gravity of B, and is acted on by the greater accelerating force; in this case we may conceive how the action of A may disturb the motion of B, and in the same way how the hard particle c may receive a small motion from the actions of A and B. This motion must be extremely little, compared with the whole motions of A or B, and still a great deal less if c be strongly connected with a string of hard particles along the line CE, so that c cannot be moved without the whole line CE turning round the immoveable centre E. Now if A, B, c, be supposed hard particles, firmly connected to the lever AE; then it is plain that the velocity of c, whatever it is, must be in proportion to that of A and B as their respective distances from E the centre of motion, and this, whatever the impulsive forces are with which A and B are urged in their respective directions.

The body c being still supposed void of gravity, let the bodies A and B be

urged, by forces perpendicular to AE in any small equal times, through the unequal spaces s and s' ; and let the magnitudes of the bodies be represented by A , B , and C respectively. Then the space through which A is actually urged in that time will easily appear, from mechanics, (see art. 13) to be represented by $\frac{A \times AE \times s + B \times BE \times s'}{A \times AE^2 + B \times BE^2 + C \times CE^2} \times AE$, and the space described by C is $\frac{A \times AE \times s + B \times BE \times s'}{A \times AE^2 + B \times BE^2 + C \times CE^2} \times CE$.

§ 3. The preceding article being well understood, whatever doubts may remain concerning the motion of a ring of matter considered as detached from the earth, we may be certain that the motion of the nodes of the equator can never be the same, whether we suppose the ring at the equator to be fluid and to rest on the surface of the earth, partaking of the diurnal motion, or whether we suppose it hard and compact, and by its cohesion communicating a proportional degree of motion to the different parts of the earth. In fact, the problem of the precession of the equinoxes, which has hitherto been considered as extremely difficult, and in its solution drawn out by authors to a vast length, requires no principles but the received doctrine of motion, and the application of the lever, which have been used in the last article. In that article we supposed the bodies A and B to be impelled by different forces in parallel lines, and we estimated the real space, which either A or C in any small time would describe, in consequence of those impulsive forces, and their mutual connection by an inflexible lever. Now this is precisely what is required to be done in the case of the sun's unequal action on the protuberant parts of the equator. The excesses or defects of that unequal action are to be considered as forces applied to those parts, which would move them according to the different circumstances through unequal spaces proportional to the forces in equal times of action, provided the particles were at liberty to move freely in the directions in which they are urged; and, lastly, the real space must be computed through which a particle moves at some known distance from the centre of the earth in consequence of these various forces. This whole process will not differ from the easy example already described, except in the length of the calculation, and the proper management of the doctrine of fluxions; and it seems advisable in difficult subjects always to begin with simple instances, before we proceed to those that are more complex, and to distinguish the algebraical operations from the principles on which they are founded.

§ 4. In order to determine how much any particle of the earth is affected by the unequal action of the sun, let $CADB$ (fig. 2) represent the earth, s the sun at a great distance, and CD a plane perpendicular to the line ST joining the centres of the sun and earth. If SK or ST represent the accelerating force of the sun on a particle at the earth's centre, and SL be taken to SK in the duplicate ratio of ST to SP ; SL will represent the attraction on any particle P , and by the

resolution of motion TM or PL will represent the perturbing force of the sun on the same particle. By the construction $SL:SK::SK^2:SP^2$, and by division $KL:SK::SK^2-SP^2:SP^2::(SK+SP)\times PK:SP^2$, and PL or TM is nearly equal to $3PK$, and as $3PK$ is to SK or ST , so is the space described by P in any small time in the direction PK , to the space described in the same time by the centre of the earth in consequence of the sun's attraction. This last space is equal to $\frac{\dot{z}^2}{2ST}$, where \dot{z} represents the arc described by the earth's centre during any small motion in its orbit, and the former is equal to $\frac{3PK \times \dot{z}^2}{2ST^2}$. This is the space which would be described by P in the direction PK if the particle was at liberty to move freely. Let us at present suppose that no other particle is disturbed by the sun's attraction except this one, and then proceed to inquire into the effects of this disturbance when P , by its cohesion, communicates a motion to the different parts of the earth, which is further constrained to turn round an axis T , the common intersection of the plane CD and the terrestrial equator. From the laws by which motion is communicated, and the property of the lever, it easily appears, as in the 2d article, that the space through which any particle of the earth's equator is impelled at the greatest distance from the axis T , is to $\frac{3PK\dot{z}^2}{2ST^2}$, the space which would be described in the same time by any particle at liberty, the magnitude of which is represented by P , as $P \times KT \times$ the radius of the equator, to the sum of all the particles of the earth multiplied into the squares of their respective distances from the said axis.

To compute this sum in the easiest way, and by an approximation, which is quite sufficient when the polar and equatorial diameters differ little from each other; let DPE (fig. 3) be a sphere whose radius is unity, divided into an infinite number of thin cylindrical surfaces, whose bases are the circles NAQ ; it is obvious, that all the particles in any one of these surfaces are at the same distance $CA = x$ from the axis of motion perpendicular to the plane of the circle NAQ . Call AP , y , and A , the area of the circle DPE ; then the fluent of $4Ax^3\dot{x}y$, or of $-4Ax^2y^2\dot{y}$, because $x\dot{x} = -y\dot{y}$, gives the sum of all the particles in the sphere multiplied into the squares of the respective distances from the axis. This fluent corrected is equal to $\frac{8}{15}A$, and must now be diminished in the ratio of 1 to $1-2p$, if we suppose the earth to be an oblate spheroid whose equatorial diameter is to the polar as 1 to $1-p$; and, lastly, the space described by a particle at the greatest distance from the axis is equal to $\frac{45P \times PK \times KT \times \dot{z}^2}{16A \times ST^2 \times (1-2p)}$.

§ 5. In fig. 4, let $PIA\hat{P}DK$ represent the earth orthographically projected on the plane of the solstitial colure, P, \hat{P} , the poles, IK a lesser circle parallel to the equator, and $pape$ a sphere described with the polar radius PT : then, since the particles without the globe only are concerned in changing the position of the

axis of rotation, let L represent such a particle situated in the circumference of the circle IK ; then by the preceding article its effect will be $\frac{45L \times LM \times MT \times \dot{z}^2}{16A \times ST^2 \times (1-2p)}$; and by the same way of reasoning, when 2 equal particles L, l , are supposed to be disturbed by the sun's attraction, the space described by that point of the equator, which is at the greatest distance from the axis of rotation, or the common intersection of the plane CD and the equator A , will be equal to $\frac{45\dot{z} \times (L \times LM \times MT + l \times lm \times mT)}{16ST^2 \times A \times (1-2p)}$; and the same argument holds for every other particle without the sphere.

The sum of all the $L \times LM \times MT + \&c.$ must now be found; and for this purpose Sir Isaac Newton's construction is, perhaps, as convenient as any that has hitherto appeared. In the same figure nn is parallel, and xy perpendicular, to CD ; take $Lx = xl$, and let m, n , represent the sine and cosine of the angle CTP to the radius unity. It is easy to prove in his way that $L \times LM \times MT + l \times lm \times mT$ is equal to $2L \times m \times n \times (Lx^2 - Tx^2)$, and the fluent of Lx^2 multiplied into the fluxion of the circular arc Lx is easily found in the following manner, without having recourse to tables of fluents, or the methods of continuation. From a known analogy, the fluxion of the arc Lx is to the fluxion of its versed sine, as the radius ix of the same circle, to Lx the right sine. Lx multiplied into the fluxion of the versed sine is the fluxion of the area of the semicircle Ll , and calling ix, y , the fluent of Lx^2 multiplied into the fluxion of the arc Lx is evidently equal to $\frac{y^3 A}{2}$, where A still represents the area of a circle whose radius is unity: Ay is equal to the semi-circumference IK , and $Ay \times Tx^2$ is equal to the fluent of Tx^2 multiplied into the same fluxion; and calling Tx, v , and substituting for ix its equal py , the sum of all the $L \times LM \times MT + \&c.$ in the annulus ii , is equal to $mnpA \times (y^4 - 2y^2v^2)$. This last quantity multiplied into the fluxion of v , and the fluent taken by the common method, when v is equal to TP or unity nearly, comes out $\frac{4pAmn}{15}$, and twice this quantity gives the sum of all the $L \times LM \times MT$, without the whole sphere pap , and therefore the space described by a particle of the equator in the circle of the sun's declination, while the centre of the earth is carried through the space \dot{z} of its orbit, is equal to $\frac{3pmn\dot{z}^2}{2ST^2 \times (1-2p)}$, and may be supposed to be equal to $\frac{3pmn\dot{z}^2}{2ST^2}$, the alteration produced by the correction in art. 4, on account of the spheroidical figure of the earth being too inconsiderable to affect the conclusion.

§ 6. We are to observe, that the space $\frac{3pmn\dot{z}^2}{2ST^2}$, described by that point of the equator which is the intersection of the circle of the sun's declination, is generated by the perpetual attraction of the sun. This attraction may be reckoned constant during the very small time of the earth's describing \dot{z} in its annual mo-

tion; and therefore the said point of the equator, at the end of that time, will have acquired a velocity which would carry it through $\frac{3pmn\dot{z}^2}{ST^2}$ in the same time.

§ 7. Let τ represent the time of the earth's revolution in its orbit, t the time of its rotation round its axis, and suppose w to be a small arc similar to \dot{z} in a circle whose radius is unity. In fig. 5, let Aa be the equator, and take Ab equal to $\frac{w\tau}{t}$, and bt , perpendicular to Ab , equal to $3pmn\dot{w}^2$, and Ab , bt , will represent the directions and quantities of the two different motions of the point A , and consequently At will be the direction of the new equator, and as Ab or At is to bt , so is the radius unity to the sine of the angle tAb ; and if Aa or AG be taken equal to a quadrant, Ga , the measure of the angle GAA , is equal to $\frac{3pmn\dot{w}t}{T}$.

§ 8. Suppose s the sun's place in the ecliptic ns , N the equinoctial point, NA the sun's right ascension, and ro a perpendicular on AN ; then ro is to Ga as the sine of AN to the radius, and rN to ro as the radius to the sine of the angle at N , the inclination of the ecliptic to the equator, and, ex æquo perturbatè, rN to Ga as the sine of AN to the sine of N , and rN the small precession of the equinoxes is equal to $3pmn \times \frac{\dot{w} \times t}{T} \times \frac{\sin. NA}{\sin. N}$.

§ 9. In the spherical triangle ASN , the sine of $AN = \cotang. N \times \tang. AS = \frac{m \times \cos. N}{n \times \sin. N}$, and the sine of $SN = \frac{m}{\sin. N}$, also rN is equal to $\frac{3pt \times (\sin. w)^2 \times \dot{w} \times \cos. N}{T}$, whose fluent, or $\frac{3pt}{4T} \times (2w - \sin. 2w) \times \cos. N$, gives the precession of the equinoxes during the sun's motion through the arc ns of the ecliptic: when ns is equal to a circle, then the whole fluent becomes equal to $\frac{3ptA}{T} \times \cos. N$, and as $2T$ is to $3pt \times \cos. N$, so is $60 \times 60 \times 360$ to $21'' + 6'''$ the annual precession of the equinoxes in seconds produced by the sun's attraction.

§ 10. We might now proceed in a similar way to investigate the effects of the moon's disturbing force, the rotation of the earth's axis, and the equation of the precession; but since these propositions are purely mathematical, and the computations have already been gone through by other authors, it will be needless to repeat them here.

§ 11. Newton was the first who attempted to explain the precession of the equinoxes from its causes. Since his time various other solutions have been given by the most celebrated mathematicians; and it deserves to be noticed, that, in a case where there can be little doubt that he was mistaken, other authors have found it difficult to agree among themselves in differing from him. Mr. D'Alembert, in the year 1749, printed a treatise expressly on the subject, and has since said,* that himself is acknowledged to be the first who determined

* D'ailleurs, des géomètres, vraiment capables d'apprécier mon travail, ont abondamment suppléé à tout autre témoignage, en déclarant que j'ai ouvert le premier la route pour résoudre ce genre de questions. See Opusc. Math. vol. 5, sur la Précession des Equinoxes.—Orig.

rightly the method of solving such problems. Euler, De La Grange, Frisius, Silvabelle, Walmesley, Simpson, Emerson, have each considered the subject, and perhaps the importance of the inquiry would justify a minute examination into the cause of the agreement or disagreement of their several methods; but I am deterred from entering into such a discussion by the length of time which it would require; especially as I think those who have read those authors will easily conceive the substance of what I should have to observe, and to those who have not read them I should hardly be able to say any thing intelligible.

§ 12. The above solution, if it had no other advantages, is, I apprehend, much more concise than any that has hitherto been given. Abstracted from what is said by way of illustration, articles 4th to 9th contain all the calculation requisite, and as I have studiously avoided the ambiguous use of the terms force, vis, efficacia, momentum, &c. as well as every doubtful representation of times, spaces, and velocities, which are often substituted by authors in equations, I believe the whole process will appear easy, and the evidence on which the conclusion rests be exactly ascertained.

§ 13. The principles described in articles 2 and 4 depend on the 3d law of motion, and the property of the lever, and are demonstrated in the following manner. Every thing remaining the same as in art. 2, (fig. 6) let AV and BR , perpendicular to the right line or axis AE , represent the forces and directions with which those bodies are respectively urged, when at liberty to move freely in those directions; and let Av , Br , cc , represent the accelerative forces of the respective bodies, as altered by their mutual actions on each other: then, because $c \times cc$ is the moving force gained by c , and $A \times vV + B \times rR$ the moving force lost by A and B , regard being had to the lengths of the different levers AE , BE , we shall have $A \times vV \times AE + B \times rR \times BE$ equal to $c \times cc \times CE$, that is, $A \times AE \times (AV - Av) + B \times BE \times (BR - Br)$ equal to $c \times cc \times CE$, and by transposition $A \times AE \times AV + B \times BE \times BR$ equal to $c \times CE \times cc + A \times AE \times Ar + B \times BE \times Br$. Let s , s' , represent, as in art. 2, the spaces which would be described by the bodies A and B at liberty in any very small portion of time, and let x be the space which A actually describes in that time, when connected with B and c by the lever AE . The quantities $\frac{x \times BE}{AE}$, $\frac{x \times CE}{AE}$, will then be the spaces described by B and c respectively; and lastly, because the spaces described in given times are as the accelerating forces, the above equation gives x equal to
$$\frac{A \times AE \times s + B \times BE \times s'}{A \times AE^2 + B \times BE^2 + c \times CE^2} \times AE.$$

The same method extends itself easily to more difficult cases, and by its assistance several very important theorems are briefly demonstrated.

§ 14. The reasoning used in art. 6 will appear very evident to any one moderately versed in the elements of mechanics and the doctrine of moving forces;

and therefore I must believe that it is by mistake that one author of note entirely omits so necessary a step which affects the conclusion by just one half. When a body moves with any velocity in the direction AM (fig. 7,) which would carry it through the space AD in a small particle of time, and any force, which may be reckoned constant for that time, urges the body through the space DC perpendicular to AM , the body at the end of that time will arrive at the point c ; but joining AC , we are not to suppose that, if that force ceased to act, the body would proceed in the direction ACL : for take cm equal and parallel to AD , and cd in CD produced equal to $2CD$, then the direction of c at that point will be cl , the diagonal of the parallelogram $cdlm$.

Thus, when a body revolves in any curve by a centripetal force (fig. 8,) we may, with Sir Isaac Newton, suppose the curve to be composed of an indefinite number of right lines, and the body to move either in the chords or the tangents of the curve; but then we are to take care that we make not suppositions inconsistent with each other. Let the curve be a circle, and AD a tangent at the point A the direction of the body's motion when it arrives at that point, and let DC , parallel to the diameter AL , be the effect of the centripetal force: then, if we suppose the body to move along the chord AC , and say, that the angle CAD measures the deflection of the path in the time of the body's moving through the arc or chord AC , we shall mistake by one half of the true quantity; for draw the tangent at c , then since Ad is equal to dc from the property of the circle, the angle cdD of deviation is equal to twice the angle CAd . The practice of Newton in a similar case, where he is investigating the horary motion of the lunar nodes in a circular orbit, is entirely consistent with this. See the *Principia*, lib. 3, prop. 30.

§ 15. M. D'Alembert has lately charged Simpson's account of the precession of the equinoxes with some mistakes of this nature in his 2d lemma; but, in justice to Simpson, I must say, that, whatever other defects there may be in his paper, I am convinced, after the most diligent attention, that those alluded to are without foundation.

§ 16. Sir Isaac Newton first observed, that a homogeneous globe could not possibly retain many distinct motions, without compounding them all into one, and revolving with a simple and uniform motion about an invariable axis. When 2 forces impress on a globe 2 distinct circular motions, (see *Principia*, lib. I. prop. 67, coroll. 22,) he briefly concludes in his way, from the laws of motion, that it is the same thing as if those 2 forces were at once impressed in the common intersection of the equators of those motions, and on this principle we supposed At , in art. 7, to be the direction of the new equator. In order to remove any doubts that might arise about the justness of this mode of compounding motion, Frisius has given a geometrical demonstration of the principle: but

the thing may be shown much more easily in the following manner. Suppose RB , AB , (fig. 9) to be 2 axes, about which every point in the plane $ABPR$ tends to move with velocities as the respective distances from the axes; let PQ , perpendicular to AB , be to PR perpendicular to RB , as the angular velocity of P about RB , to the angular velocity of the same point about AB ; and let the velocities be in contrary directions: then, I say, every point in the plane will move with a velocity proportional to its distance from the axis PB . First, it is evident, that any point c in the axis RB will move round PB with a velocity proportional to its distance cm : for the point c , lying in the axis RB , has no velocity round RB , and cm is proportional to cn . Draw pc parallel to AB , then any point D in that line will move with a velocity proportional to DV , which is perpendicular to PB , for the following reason: the velocity of D is equal to the difference of its 2 velocities round the respective axes RB , AB , or the difference of D 's velocity round RB , and P 's velocity round AB , since all the points in PC move with the same velocity round AB . Draw DT parallel to CR , then this difference will be proportional to PT , because the velocities of P round RB , AB , are supposed equal to each other, and PT is proportional to DV , and every point in the plane moves round PB with a velocity proportional to its distance; and the same thing may be shown when any point is taken without the plane $ABCPR$.

§ 17. Because any point c in the axis RB moves with the same velocity round PB as it does round AB , the angular velocities round the 2 axes AB , PB , will be to each other inversely as their respective distances cn , cm ; and because $cn : cm :: PB : PC$, and $PR : PQ :: PC : CB$, it follows, that PB , the diagonal of the parallelogram $PGBc$, will represent the angular velocity of the revolving plane, when BG , Bc , are taken to each other as the angular velocities of the same plane round those respective axes.

§ 18. From this it clearly follows, that the reason given by Simpson, in his miscellaneous tracts, pages 44 and 45, of the difference between his own solution and that of Newton in the Principia, cannot possibly be the true one. "It appears further," says he, "by perusing his 39th proposition, that he there assumes it as a principle, that if a ring encompassing the earth at its equator, but detached therefrom, was to tend or begin to move about its diameter with the same accelerative force or angular celerity as that whereby the earth itself tends to move about the same diameter through the action of the sun, that then the motion of the nodes of the ring and of the equator would be exactly the same." The principle is certainly implied in Newton's proof, and is capable of the most rigid demonstration, art. 16, 17.

§ 19. It will be asked then, where is the fault of Newton's reasoning? How comes his conclusion to be too little by above one half? It is acknowledged on all hands that there is an error in his 3d lemma; but then the correction of that

error makes only a very small alteration in the result. It is impossible for any one to form a complete judgment of his method without going through the whole of his calculations, which pre-supposes that the mean motion of the lunar nodes is computed. This motion may be concisely determined, and exactly enough for the purpose, from prop. 30 of the Principia, and thence is inferred the motion of the nodes of a satellite revolving in the plane of the equator at the surface of the earth, with a velocity equal to that of the earth round its axis. We are then to suppose, that the mean motion of the nodes of a satellite, revolving with such a velocity, is the same with the motion of the nodes of a ring of rigid matter surrounding the earth at its equator, and revolving with the same velocity. This last hypothesis is admitted by Simpson, who thinks that his own 2d lemma contains a full demonstration of the point. For my own part, I believe with Frisius, that we are to look here for the material error in Newton's solution of the problem. It is evident, that the true motion of the nodes of the satellite, and the ring of matter, are not the same: and it is by no means obvious that their mean motions are so. The mean motion of the nodes of a ring of hard matter cohering together is very easily computed by the method in art. 4th to the 9th, and turns out nearly double the mean motion of a moon revolving at the surface of the earth with the same velocity.

It is a very interesting inquiry to find out the real cause of the mistake in the Principia, lib. 3, prop. 39; and therefore at a future opportunity I may perhaps consider this particular part of the subject more attentively. I have long been satisfied with the account already given, and should probably have remained so, if M. D'Alembert,* in his Opusc. vol. 5, had not persisted to affirm, that the mean motion of the nodes of the ring of matter and of the satellite were the same. This opinion of so celebrated a mathematician raises scruples in one's mind; and shows, that when we venture to differ from Sir Isaac Newton in these matters, it is with the utmost difficulty that we can arrive at certainty.

XXXIII. An Examination of Various Ores in the Museum of Dr. William Hunter. By George Fordyce, M. D., F. R. S., and Mr. S. Alchorne. p. 527.

The first ore here examined is called gold pyrites. Pyrites is an ore of iron, containing sometimes copper, sometimes arsenic, sometimes other metals; generally, if not always combined with sulphur. This ore, from its yellow colour, has at first sight been often taken for an ore of gold. It is the most

* Il n'y a de parité que dans le mouvement moyen de ces deux anneaux, ou de l'anneau solide et de la lune; les mouvemens instantanés sont très différens de part et d'autre; ainsi la comparaison du mouvement de l'anneau avec celui de la lune, serviroit tout au plus à trouver le mouvement moyen de l'anneau, ou de la précession des équinoxes, mais nullement à déterminer la nutation de l'axe et l'équation de la précession.—Orig.

common of all ores, and on examination is very seldom found to contain gold. Specimens however have been found from which gold has been procured; and particularly there have been found in Fatchobuigna near Zalatna, in Transylvania, masses which contain a large proportion of this metal. Sometimes the gold is in its metallic form, and visible to the naked eye; sometimes it is not: and in these cases the ore has been thought to contain the gold united first with iron, and that compound united with sulphur.

Dr. Fordyce observes on this proposition, that it has not as yet been proved, that a compound metal can be combined with sulphur. If 2 metals are soluble in sulphur, and each be separately combined with it, the 2 compounds may be diffused through one another, as is the case with the compounds of sulphur and iron, and sulphur and copper; which may be diffused through each other: but if we have a compound of 2 metals, of which one is soluble in sulphur, and the other is not, if we apply sulphur to this compound, it will dissolve the one, and leave the other; as, if gold and silver be combined, if we reduce the mass to fine particles, and mix them with sulphur, and throw them into a crucible, heated white hot, and afterwards melt the whole mass, the silver will combine with the sulphur, and the gold will fall to the bottom of the vessel. This being the case, they doubted whether this ore contains the gold in its metallic form, only mechanically mixed with the pyrites; or combined with the sulphur by means of the iron: and therefore subjected a specimen to examination, in which they could not discover, even by the help of a microscope, any particle of native gold.

Exp. 1. They powdered 100 grs. of this ore, and boiled it in nitrous acid diluted with water; a solution took place with effervescence. Having digested them together, till the whole soluble part was taken up, and poured off the solution, and made a precipitation by fixed vegetable alkali, the precipitate appeared to be iron. Having washed the remaining part with water, and exposed in a glass matrass in sand, to nearly a red heat, a very small portion of sulphur sublimed; the remainder was quartzose sand, with particles of gold, which were similar in figure, though small, to the particle of gold found native in veins mixed with various matrixes, and not at all like particles which had been combined with a menstruum, which ought either to have appeared in a powder, whose particles were hardly visible from their smallness; or in crystals similar to one another.

Exp. 2. If any metal be combined with sulphur, mercury will not precipitate the sulphur from it; they therefore took 140 grs. of the same ore, and triturated it for some hours with about 5 times its weight of mercury; the powdered ore was washed off from the mercury, the remainder put into glass vessels, and evaporated by heat: a mass of gold was left, but part of it being accidentally lost, its

weight could not be ascertained; it did not amount to above 2 or 3 grs. at most. The powder, washed off and dried, weighed 134 grs: these, mixed with as much litharge, and 4 times their weight of fixed vegetable alkali, and $\frac{1}{5}$ of wheat flower, and the whole melted, produced a regulus of lead, weighing 80 grs., which, on coppelling with a few grains of silver, and parting in aquafortis, left $\frac{1}{16}$ of a troy grain of gold. It is to be remarked, that in great works, where gold is separated from pieces of crucibles, sand, or other matter, by amalgamation, notwithstanding the process be frequently repeated, and with great care, some small portion of the gold still remains; so that the small quantity left in this case might easily have either escaped the mercury, or have been left unobserved in the powder combined with small particles of it. They therefore concluded that in this ore the gold was in its native form, and not mineralized.

Exp. 3. They examined gold pyrites mixed with quartz, with a deep magnifier, and found evidently native gold interspersed. One hundred grains of this ore, were powdered; and boiled in nitrous acid diluted with water: a solution of iron took place, as in the first experiment. The residuum being exposed to a red heat, there was no appearance of sulphur; the gold was found in such large particles, and so similar to native gold, that they did not think it worth while to apply mercury. The gold being dissolved out by aqua regia, a quantity of quartz and arsenical salt was left.

Exp. 4. When gold and silver are found in their metallic form, it is not uncommon to find them mixed; but it seldom occurs, that they are mixed in so large proportion as in an ore obtained from Norway, and which was given to Dr. Hunter, by Mr. Fabricius. The ore has the appearance of native silver, in scaly particles, intermixed with a hard quartz, tinged brown, in some parts by iron. Twenty-five grains of the mass, where the metal was in the largest proportion, melted with litharge, alkali, and phlogistic matter, and afterwards coppelled, yielded a globule apparently silver weighing 2 grs. which, being boiled in nitrous acid diluted with water, left full $\frac{9}{16}$ of a grain of fine gold. Hence 100 lb. of metal obtained from this ore, consists of 72 lb. of silver, and 28 lb. of gold.

There is an ore of silver which is commonly called vitreous (*minera argenti vitrea*, or *argentum vitreum*). This ore has always been supposed to consist of sulphur and silver; because, if sulphur and silver be melted together; they form a mass which resembles it, especially in colour and malleability; but, as they could find no experiment in any author which authorized this conjecture, they determined to endeavour to ascertain it by analysis, Dr. Hunter not refusing to subject the ore to an assay, though scarce and expensive.

Exp. 5. Fifty grains of this ore broken in pieces, for it was too malleable to be powdered, were boiled in nitrous acid diluted with water; the acid dissolved

the silver with much difficulty, and left a residuum. The solution being poured off, and the residuum washed and exposed in a glass matrass to a red heat, there was no sublimate of sulphur, nor any thing else obtained. The residuum, before it was exposed to heat, did not appear to contain any sulphur; being of a blackish colour after exposure to heat, it had a number of yellow particles mixed, which easily dissolved in aqua regia. Tin being applied to the solution, made no precipitation, which it would have done if those particles had been gold; hence they supposed they were iron.

The silver was precipitated from the acid by copper: washed and dried it weighed 4 grs. which, on coppelling with lead, lost $\frac{1}{4}$. There were particles of powdered quartz apparently mixed with the yellow particles left after the solution.

Exp. 6. Being much disappointed in not finding any sulphur in the former experiment, they took 300 grs. of the same ore. They freed it as well as possible from heterogeneous matter: they mixed it with 4 times its weight of mild fixed vegetable alkali, put them into an earthen body, to which was affixed a glass head, and exposed them to nearly a white heat in a naked fire for $1\frac{1}{2}$ hour; but no sublimation took place, only a few drops of water distilling. They then put the body into a melting furnace, and rendered the whole fluid; as soon as it was melted, it was removed from the fire. The mass, on concreting, was found divided into 2; a black mass at top, and a metallic mass at bottom. The metal being assayed by solution in nitrous acid and precipitation with volatile alkali, showed no sign either of gold or copper; lost nothing by coppellation with lead; it weighed 213 grs., which were pure silver.

The black mass at top, or scorizæ, was boiled repeatedly in water, but did not all dissolve. The insoluble part was unfortunately thrown away; but to the solution they added muriatic acid: on the addition of the acid, there was a strong smell of hepar sulphuris, and a copious precipitate, which, on being examined by a microscope, appeared to consist of pellucid crystals, without the smallest appearance of sulphur. This precipitation, being exposed to heat, did not smell in the least like sulphur; nor was it in the least inflammable. Excepting then the smell of hepar sulphuris, there did not appear any mark of sulphur in this ore, and a very small particle of inflammable matter dropping in by accident would give this smell.

The foregoing experiments occasioning some doubt of sulphur being contained in vitreous silver ore, they endeavoured to investigate it by other means; and after several experiments, they made the following one, which seems conclusive. Half an ounce of silver was precipitated from nitrous acid by copper, in small flaky crystals, as usual; being washed and dried, it was mixed with the same weight of sulphur, and put into a crucible, over which another was placed so as to cover it, and the 2 crucibles were luted together, leaving a small aper-

ture for the escape of vapour, but not so as to admit air for the inflammation of the sulphur. These being put into the fire, as soon as they were heated red-hot, the sulphur in part escaped through the small hole, and as it passed through burnt with a blue flame. The fire was increased to a sufficient degree to melt the mass within; in the mean time the blue flame disappeared, which showed that no more sulphur escaped.

After having applied heat enough, as was supposed, to melt the mass, the crucibles were removed from the fire, and being separated when cold, a regular button-like mass was found in the crucible, of a dark lead colour both without and within, brittle, and streaked on the inside, bearing to be cut with a knife, exactly like the vitreous ore, which it in every way resembled. The whole mass gained 20 grs. or $\frac{1}{12}$ in weight, which may be considered as the true weight of the sulphur in this compound, as silver combines with sulphur in its metallic form, without losing its inflammable air. Fifty troy grains of this compound was powdered with difficulty, from its toughness, and boiled in nitrous acid diluted with water; it dissolved with difficulty, leaving a very small quantity of a light blackish powder, certainly not nearly a grain in weight. It appeared therefore, that the small portion of sulphur contained in the vitreous ore might have been decomposed by the nitrous acid, and the alkali, or the heat, in the former processes; and they concluded, that vitreous silver ore is a compound of silver and sulphur, and when pure, that it contains between 92 or 93 grs. of silver in 100.

XXXIV. On some new Methods of suspending Magnetical Needles. By John Ingenhousz, Body Physician to their Imperial Majesties, and F. R. S. p. 537.

The great utility which navigation derives from the use of compasses, has been the principal reason, that so much labour has been bestowed by many ingenious men in searching the best method of suspending magnetic needles, and that some Academies of Sciences have proposed considerable premiums to be given to the person who should succeed the best in this important object. It has been, and is still, a general complaint, that such needles as were by their size and figure susceptible of the greatest magnetic power, and which were executed with the greatest exactness, and perfectly balanced, were, for this very reason of their superior accuracy, so easily put in motion, that the slightest shaking of the floor, occasioned by the observers walking in the room, communicates to them such a great quivering motion as to drag them out of their direction, and to render a considerable time requisite before they are again fixed in the true magnetical meridian.

Since the late Dr. Knight has improved so greatly the method of communi-

cating a considerable magnetic force to steel bars, needles have been impregnated with a much greater and more permanent polarity than it was possible to give them before, and the construction itself of compasses has been considerably improved. But the subject seems not yet to be exhausted. The degree of magnetic power or polarity to be communicated to steel well hardened, seems to depend in some respect on the proportion which the surface of the piece of steel to be impregnated bears to its bulk; so that a thin lamella of steel, weighing for instance 10 grs., will acquire more magnetic force than a little lump of steel of the same weight. Thus a small magnet, composed of a great many thin lamina of steel pressed together, may be made so strong as to support 150 times its own weight, and even more, which has never yet been done by a compound heavy magnet.

If the too great quivering and horizontal motion, or the too great restlessness of a strong magnetic needle, is in some way or other counteracted by the methods hitherto adopted, the needle is in some danger of stopping near the true magnetic meridian, without always pointing directly in it; because near this very place the power of direction of the needle may be so weak as not to overcome the resistance, which was opposed to its free horizontal circular motion. Dr. I. thought, in the course of last year, that a great part of the abovementioned difficulty might be taken away by different methods, some of which are the following.

Exp. 1. He placed an ordinary magnetic needle, supported on its point, in a china basin, and poured water into it, so as to cover the whole needle. The needle lost a great deal of its restlessness, was influenced by the approach of a magnet at a considerable distance, and seemed to point as well as before to the magnetic meridian, though with a slower motion.

Exp. 2. He took a strong flat magnetic needle, having in its centre a round hole, but no cap. He fixed to this needle as much cork as was necessary to make it just swim on the water in a basin. He fixed a smooth brass pin in a vertical position, so that it passed through the hole in the centre of the needle, to prevent its swimming to the sides of the basin. This did tolerably well: the needle moved to the magnetic meridian with a slow motion.

Exp. 3. He then took the needle used in the first experiment, and fixed on it as much cork as was requisite just to make it sink. He placed a point under it, so that the needle with the cork was kept a little below the surface of the water, bearing on the point with a very inconsiderable weight. This answered nearly as in experiment 2.

Exp. 4. He fixed to the centre of a small, but strong magnetic needle, a silver wire; to the other extremity of this wire he fixed a small steel sewing needle, very much hardened. He stuck the point of this sewing needle to the

lower extremity of a steel magnet, placed in a vertical position, and highly polished. This whole apparatus was placed in a glass cylindrical vessel, filled with water, so far as to cover the point of the working needle. The horizontal magnetic needle, thus suspended, was remarkably quick in directing itself into the magnetic meridian, and was very easily displaced out of its direction by the approach of a magnet at a considerable distance. The greatest difficulty found in this contrivance was, that a little jerk shook the point of the sewing needle from the vertical magnet; and that a heavy magnet could not be suspended from the point of a sewing needle, but required a tolerably good magnet rounded at its extremity, where it was suspended from the vertical magnet, or a thick piece of iron rounded in the same manner, by which means, however, the horizontal magnet did not move so freely.

Exp. 5. He adapted to a flat strong magnetic needle, 2 caps, turned one against another, so that the needle could be supported on either of the caps, and turned with either surface upwards. He fixed on one of the flat surfaces, on each side of the centre, a thin glass tube, hermetically sealed, of such a size, as made the whole together dip in the water so deep that only a small part of the glass tubes remained above the surface of the water. He then depressed the needle entirely under the surface of the water, by thrusting a metallic point in the cap which was uppermost, and fixing this point to the cover of the basin: thus the needle, kept under water, bore against the point only with that small degree of pressure which the very inconsiderable difference of specific gravity, which it had above an equal bulk of the water, could give it, which did not amount to above a few grains of weight. Thus this needle, losing nothing of its polarity, lost very near the whole of the resistance of its weight, and at the same time that quivering motion and restlessness, which it had in the air, moving smoothly in a medium, which could only obstruct its too great vibrations (almost in the same way as astronomers stop the vibrations of their plumb-line by hanging the weight in water or oil) without obstructing (but retarding only) its tendency to the magnetic meridian, liquids pressing equally on all sides.

Having now found, by experience, that a strong magnetical needle pointed to the magnetic meridian near as well under water as in the open air, and that by the resistance of the medium much of its too great versatility was taken away; Dr. I. wanted to try what degree of magnetism could be given to a thin steel cylindrical tube, as a needle made in this shape could be as light as required, without being encumbered with cork or glass tubes; but finding no such tubes ready made, he tried one made of tin, and found it susceptible of a much greater magnetic virtue than he expected, considering that the iron plates of which it is made are neither sufficiently hardened for this purpose, nor approach enough to the nature of steel; besides that, being covered with a pewter coating,

they cannot be exposed to the bare friction of a steel magnet. This experiment however afforded a certain proof, that a magnetic needle, made of a thin steel tube, would be susceptible of a strong polarity, so as to serve for the purpose here intended.

But as the two agate caps, if fixed in the middle of the substance of the tube, would interrupt some part of the continuation of the steel, and thus lessen the magnetic power, the points of suspension would be better soldered or fixed on the surface of the tube itself, and the agate caps fixed on the support beneath and above the tube; so that such a needle should be the reverse, in respect to the suspension, of the common needles. Another advantage would be derived from fixing the points of suspension on the needle itself, viz. that by the motion of the fluid in which it swims, it would be less apt to acquire a too strong waving or undulating motion.

If it should be found difficult to make very thin steel tubes properly hardened, a piece of steel could be scooped out so as to constitute the half of a tube; the other half could be made of another similar piece, or perhaps better of thin brass, or any other metal, and they might be soldered together. It would, perhaps, answer the same purpose if a steel magnet were shut up in a thin tube of glass or some metal.

XXXV. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1778. By Thomas Barker, Esq. p. 547.

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the House.			Abroad.			
					Hig.	Low	Mean	Hig.	Low	Mean	
Jan.	Morn.	29.90	28.16	29.27	42 $\frac{1}{2}$	32	37	41	18 $\frac{1}{2}$	32	1.980
	Aftern.				43 $\frac{1}{2}$	32 $\frac{1}{2}$	38	44 $\frac{1}{2}$	21	37	
Feb.	Morn.	29.85	28.66	29.33	44 $\frac{1}{2}$	34	38	45	24	33	0.949
	Aftern.				45	34	39	51	32	38 $\frac{1}{2}$	
Mar.	Morn.	29.98	28.61	29.32	51	35 $\frac{1}{2}$	41 $\frac{1}{2}$	52	24 $\frac{1}{2}$	36	1.196
	Aftern.				51 $\frac{1}{2}$	37 $\frac{1}{2}$	43	59	36	44	
Apr.	Morn.	29.81	28.92	29.31	56	41 $\frac{1}{2}$	47 $\frac{1}{2}$	51	30	41	1.037
	Aftern.				58	43	49	65 $\frac{1}{2}$	40	51	
May	Morn.	29.79	28.94	29.41	61	48 $\frac{1}{2}$	54	59 $\frac{1}{2}$	45	50	1.322
	Aftern.				62	49	55 $\frac{1}{2}$	70	52	61	
June	Morn.	29.86	29.18	29.58	66	53	60	65 $\frac{1}{2}$	43	55 $\frac{1}{2}$	2.714
	Aftern.				68	55	61 $\frac{1}{2}$	80 $\frac{1}{2}$	57	69	
July	Morn.	29.82	29.02	29.48	70 $\frac{1}{2}$	59	64 $\frac{1}{2}$	66	51 $\frac{1}{2}$	59 $\frac{1}{2}$	4.103
	Aftern.				73 $\frac{1}{2}$	61	66 $\frac{1}{2}$	85	62 $\frac{1}{2}$	72 $\frac{1}{2}$	
Aug.	Morn.	30.00	29.10	29.69	67 $\frac{1}{2}$	56	63	63	42	56	0.391
	Aftern.				70	56	65	76	57	69	
Sept.	Morn.	29.99	28.72	29.54	62 $\frac{1}{2}$	48	56	55 $\frac{1}{2}$	32 $\frac{1}{2}$	46	1.660
	Aftern.				64	50	57	66	44 $\frac{1}{2}$	58 $\frac{1}{2}$	
Oct.	Morn.	29.64	28.62	29.31	55	44	48	52	26	39 $\frac{1}{2}$	4.238
	Aftern.				57	45	49 $\frac{1}{2}$	61	38 $\frac{1}{2}$	50	
Nov.	Morn.	29.72	28.63	29.23	52	42	46 $\frac{1}{2}$	53	30	40 $\frac{1}{2}$	3.845
	Aftern.				53	43	47	56	38 $\frac{1}{2}$	46	
Dec.	Morn.	30.23	28.25	29.34	51	40	44	51 $\frac{1}{2}$	28	40	2.835
	Aftern.				51 $\frac{1}{2}$	41	45	52 $\frac{1}{2}$	33	44	
Whole year,				29.40	50 $\frac{2}{3}$			48 $\frac{3}{4}$			26.270

XXXVI. *Extract of a Meteorological Journal for the Year 1778, kept at Bristol, by Samuel Farr, M. D. p. 551.*

Months.	Barometer.			Thermometer in.			Thermomet. out.		
	Highest.	Lowest.	Mean.	Hig.	Low	Mean.	Hig.	Low	Mean.
January . . .	30.31	28.54	29.37	44	31	36	46	30	36
February . . .	30.34	29.25	29.81	46	35	40	48	32	40 $\frac{6}{10}$
March	30.38	29.10	29.77	52	39	43	56	36	44 $\frac{6}{10}$
April	30.25	29.30	29.78	63	43	51	70	41	53
May	30.28	29.40	29.56	66	50	57	70	48	58
June	30.34	29.65	30.20	74	55	61	78	53	64
July	30.29	29.50	29.94	75	61	67	79	57	68 $\frac{1}{2}$
August	30.48	29.73	30.15	73	59	65	78	61	68
September . .	30.40	29.30	29.80	64	47	57	67	46	58
October . . .	30.10	29.18	29.70	57	41	49	59	38	47
November . .	30.26	29.12	29.69	55	43	49	57	39	46
December . .	30.74	28.79	29.86	54	38	45	53	28	43 $\frac{6}{10}$
Mean of the whole			29.80			51 $\frac{1}{2}$			52

XXXVII. *A Treatise on Rivers and Canals. By Theod. Aug. Mann, Member of the Acad. of Sciences at Brussels. p. 555.*

In the 1st section Mr. Mann enumerates the principal authors who have treated this subject in different ages and countries, in whose works the demonstrations of all the principles he lays down may be found. These are the following: viz.

Sextus Julius Frontinus, de Aquæ-ductibus Urbis Romæ, cum Notis Poleni, impress. 1722.

John Baptist Aleotti, hydrometrician to the duke of Ferrara, and to Pope Clement the 8th.

* Don Benedict Castelli, Benedictine Abbot, de Mensurâ Aquarum Currentium.

J. B. Baratteri, de Architettura d'Acque, lib. 6, Placenza, in folio, 1656.

Alexander Beltingzoli, of Cremona.

Nicolaus Cabeus, in Libris Meteorum.

Galilei Galileo.

* Joh. Bapt. Baliani, de Motu Liquidorum.

* Joh. Bapt. Riccioli, Geographiæ et Hydrographiæ Reform. libro 6, ec. 29 et 30.

* Claude Millet Deschales, de Fontibus et Fluminibus, à prop. 39 usque ad 56. Varenus, General Geography, with Dr. Jurin's and Dr. Shaw's Notes, edit. of 1765, vol. I. from page 295 to page 358.

Dr. Jurin, in the Philos. Trans. N^o 355, page 748 et seq.

Mariotte, Traité du Mouvement des Eaux.

Varignon, Memoires de l'Academie des Sciences de Paris, pour 1699 et 1703.

* Sir Isaac Newton, Princip. Mathem. lib. 2, § 7, p. 318 et seq. edit. 1726.

* Danielis Bernouilli, *Hydrodynamica*, in quarto, Argentorati, 1738.

* Domeniche Guglielmini, *della Natura de Fiumi*, Bononiæ, 1697, in quarto. *Ejusdem de Mensurâ Aquarum fluentium*, Bononiæ, in quarto.

* Joh. Polenus, *de Castellis et de Motu Aquæ mixto*, Patavii, 1697, 1718, 1723.

* *Raccolta d'Autori che trattano del Moto dell' Acque*, Fiorenza, 1723, 3 vols. 4to.

Jac. Hermannus, in *Phoronomia*, cap. 10, page 226 et seq.

Christ. Wolf, *Curs. Mathem. Hydraulicæ*, cap. 6, edit. Genevæ, in quarto, 1740.

M. De Buffon, *sur les Fleuves*, dans son *Histoire Naturelle*, tom. 2, p. 38 to 100.

Several Memoirs upon this subject in the collection of the Royal Academy of Sciences of Paris, particularly those of M. Pitot, in the volumes for 1730 and 1732.

S'Gravesande, in *Elementis Physicæ*, tom. 1, lib. 2, cap. 10.

* R. P. Lecchi S. J. *Hydrostatica*, Mediolani, 1765. In this excellent work are several pieces by Father Boscovich on the same subject.

* *Stattleri Physica*, vol. 3, p. 232—286. *de cursu Fluminum, ejusque Mensuratione et Directione*. Aug. Vindel. 1772, 8 vols. in octavo. This author gives many late observations and experiments on the motion and measure of currents, as those of Zandrini, Himenii, &c.

Two other authors have lately written on rivers and canals, but their works are not yet come to my hands: viz.

Father Frisi, an Italian Barnabite, Professor of Mathematics at Milan, and M. De Lalande, of the Royal Academy of Sciences at Paris, who has just published a history in folio, with plates, of all the navigable canals in the world that have come to his knowledge.

Among the above authors, those marked with an asterisk (*) have treated the subject with the greatest exactness or most extent. After this, in several more sections, the author mentions numerous properties of canals and rivers, in a systematic manner. But these being all of the most common kind, unaccompanied with any new discoveries or new principles; the paper appearing to be out of its place in these Transactions, and rather resembling a book or methodical treatise, of more than 100 quarto pages, of common-place matter and maxims; we shall therefore limit our abridgement of it to little more than a brief account of the chief matters and titles of the sections.

Section 2, is on the theory of rivers and canals, in definitions, and several common maxims; on the motions of bodies on inclined planes; on the nature of rivers and flowing waters; then the application of the laws of the accelera-

tion and retardation of currents to rivers and canals in general, whence are deduced the various means of preventing or remedying the defects and inconveniences which must necessarily happen to them in a series of years : then follow observations on the confluence of rivers, and on the separation of the same rivers into divers branches and mouths, with their effects on the velocity of currents, inundations, &c.

Section 3 treats on the laws of the meeting of opposite currents, with the application of them to sluices. And sect. 4 contains experiments to determine the different velocities, in different depths of water, of the same floating body moved uniformly by an equal force ; which experiments however are on a scale much too small to be of any real practical use. And section 5 treats on the quantity of declivity in rivers. And here, from certain data, obtained from observations and actual mensuration, and from many others of the same nature, he deduces the following table of comparative proportions between the declivities and velocities in different kinds of rivers.

Distinctive attributes of the various kinds of rivers and flowing waters.						
Rates or classes of rivers and flowing waters.	Comparative degrees of the mean velocities of currents.		Seconds of time wherein currents run 50 fathoms.	Fathoms run by the current in 1 minute of time.	Ratios of declivity compared with horizontal length.	Fathoms of length for each one twelfth of an inch of declivity.
1	0	0"	0	$\frac{1}{12000}$	14	Channels where the resistance from the bed, and other obstacles, equal the quantity of current acquired from the declivity ; so that the waters would stagnate, were it not for the compression and impulsion of the upper and back waters.
2	$\frac{2}{3}$	180	$6\frac{2}{3}$	$\frac{1}{7000}$	8	Artificial canals in the Dutch and Austrian Netherlands.
3	1	120	10	$\frac{1}{5200}$	6	Rivers in low and flat countries, full of turns and windings, and of a very slow current, subject to frequent and lasting inundations.
4	$1\frac{1}{2}$	80	15	$\frac{1}{4000}$	$4\frac{2}{3}$	Rivers in most countries that are a mean between flat and hilly, which have a good current, but are subject to overflow : also, the upper parts of rivers in flat countries.
5	$2\frac{1}{2}$	55	$21\frac{2}{3}$	$\frac{1}{3200}$	$3\frac{2}{3}$	Rivers in hilly countries, with a strong current, and seldom subject to inundations : also, all rivers near their sources have this declivity and velocity, and often much more.
6	3	40	30	$\frac{1}{2600}$	3	Rivers in mountainous countries, having a rapid current and straight course, and very rarely overflowing.
7	5	24	50	$\frac{1}{2000}$	$2\frac{1}{3}$	Rivers in their descent from among mountains down into the plains below, in which places they run torrent-wise.
8	8	15	80	$\frac{1}{1760}$	2	Absolute torrents among mountains.

After carefully comparing what has been said in the accounts of travellers, and in the best treatises of geography, on the principal rivers in the known world, Mr. Mann classes them in the following manner : under the 1st rate or class in the above table, he puts that part of the channel of most great rivers which is in extensive plains next the sea ; with regard to the declivity alone, but not at

all with regard to the velocity of the current there, which is often very great from the compression and impulsion of the upper waters. In the 2d rate or class, most artificial canals in flat countries, made for the use of navigation; especially those in the Dutch and Austrian Netherlands. The 3d rate or class, the river Trent; the Scheld and the Lys below Ghent; the Isere and the Iprelee below Fort Knock in Flanders; many rivers in the territories of Bologna and Ferrara in Italy; the river Meander in Natolia; and innumerable others in flat countries. The 4th class, the Thames; the Lys and the Scheld above Ghent in Flanders; the Senne, the Dyle, and the Demmer, in Brabant; the Seine and the Somme in France; the Nile and the Niger in Africa; the rivers of St. Lawrence below Lake Ontario, the Oroonoko, the river of Amazons, and the rivers of Paraguay, in America. The 5th class, the Severn and Ouse in England; the Loire and Garonne in France; the Tagus, the Guadiana, and the Guadalquivir, in Spain; the Po and the Tiber in Italy; the Meuse, the Rhine, and the Elbe, in Germany; the Weissel, the Neister, the Bog, and the Nieper, in Poland; the Don and the Dwina in Russia; the Amur or Saghalien in Tartary; the Yellow and Blue Rivers in China; the rivers of Cambodia, Ava, and Ganges, in India; the Euphrates; the river Zaire in Congo; the Mississippi. The 6th class, the Rhone in France; the Ebro and Douro in Spain; the Danube; the Wolga; the Irtisch and Oby, the Jenesca and Lena, in Siberia; the river Indus; the Tigris; the Malmistra in Cilicia. In the 7th class can only be enumerated those parts of rivers where they descend from among mountains into the plain country below; as also some rivers passing through the midst of mountains. To the 8th class belong all torrents among mountains; such, for example, as the Bourns in the Highlands of Scotland are described to be.

Section 6, professes to give a general and easy method of taking levels through large extents of country where rivers pass; and also of computing the heights of interior parts of continents above the surface of the sea. After which the author concludes with the following table, containing a summary of his researches on this head.

A Table of the elevation of countries above the surface of the sea, at each 100 miles of length up the course of the principal rivers in the world, as far as they extend; by computation from the principles laid down in this treatise.

Feet of eleva. in 100 miles.	Feet of eleva. in 200 miles.	Feet of eleva. in 300 miles.	Feet of eleva. in 400 miles.	Feet of eleva. in 500 miles.	Feet of eleva. in 600 miles.	Feet of eleva. in 700 miles.	Feet of eleva. in 800 miles.	Feet of eleva. in 900 miles.	Feet of eleva. in 1000 miles.	Names of Rivers, and quantities by which the length of their course is to be diminished, to have the distance from their mouths in a direct line.
100	210	330	470	630	820	1040	1300	1600	1950	The river Trent, the Meander, and many others of the same kind, which are seldom of great extent: in these one-third of the course may be allowed for its deviation from a right line.
150	310	480	670	880	1120	1400	1770	2160	2620	The river Thames; the Seine and the Somme in France; the Nile and the Niger in Africa; the river St. Lawrence, the Oroonoko, the river of Amazons, the rivers of Paraguay: in these about one-fourth of the length of course may be allowed for turns and windings in it.
220	450	700	980	1290	1640	2040	2500	3030	3630	The Severn; the Loire and Garonne in France; the Tagus, Guadiana, and Guadalquivir in Spain; the Po and the Tyber in Italy; the Meuse, Rhine, and Elbe in Germany; the Weisell, Neister, Bog, and Nieper in Poland; the Don and Dwina in Russia; the Amur in Tartary; the Hoang-hokeou and Yang-tse Kiang-keou in China; the rivers of Cambodia, Ava, and Ganges in India; the Euphrates; the Zaire in Congo; the Mississippi: in these may be allowed about one-fifth of the length of the course for turns and windings in it.
300	650	1050	1520	2070	2720	3500	4440	5570	6920	The Rhone in France; the Ebro and Douro in Spain; the Danube; the Volga; the Irtisch, Oby, Jenesca, and Lena in Siberia; the Malmistra in Cilicia; the Tigris; the Indus. The course of these rapid rivers is usually very straight, and there cannot be above one-sixth of the length thereof allowed for deviations from a right line.

XXXVIII. Extract of two Meteorological Journals of the Weather, observed at Nain, in 57° North Latitude, and at Okak, in 57° 30', North Latitude, both on the Coast of Labradore. Communicated by Mr. De La Trobe. p. 657.

The thermometer, of Fahrenheit's scale, was observed at 8 in the morning, at noon, at 4 in the afternoon, and at 8 in the evening. The barometer, whose scale is French measure, was observed at 8 in the morning, and at 8 in the evening.

Months.	Thermomet. at Nain.			Barometer at Nain.			Thermomet. at Okak.		
	High	Lowest	Mean.	High.	Lowest	Mean.	High	Lowest	Mean.
1777									
August..	76°.0	36°.0	52°.3	28.3	27.6½	27.10	74°.0	36°.0	52°.0
Septemb.	72.0	27.0	43.6	28.5½	27.6½	28.04	68.0	21.0	43.9
October.	51.0	12.0	30.2	28.7½	26.11½	28.01	48.0	13.0	30.0
Novemb.	41.0	— 8.0	15.4	28.9	26.9	28.00	39.0	— 5.0	17.5
Decemb.	31.0	— 26.0	10.3	28.5½	26.3	28.30	32.0	— 28.0	4.2
1778									
January.	18.0	— 30.0	— 12.2	28.5	27.1½	28.45	16.0	— 27.0	— 11.0
February	32.0	— 27.0	— 10.7	28.6	27.3	28.76	34.0	— 25.0	— 9.2
March..	27.0	— 25.0	4.3	28.10½	27.2½	27.40	32.0	— 18.0	6.5
April...	49.0	— 8.0	27.1	28.6½	27.6	28.43	53.0	— 10.0	28.5
May....	68.0	29.0	39.0	28.5½	27.7	28.75	65.0	27.0	37.5
June....	77.0	33.0	45.0	28.6	27.4	28.00	73.0	31.0	45.7
July....	82.0	34.0	51.0	28.3½	27.8½	28.01	31.0	36.0	51.3
August..	74.0	40.0	52.5	28.5	27.5½	28.00	72.0	41.0	52.0
Septemb.	69.0	34.0	43.5	28.0	27.10	28.20	53.0	32.0	43.0
Mean of all...			32.6			28.2			32.7

XXXIX. Improvements in Electricity. By John Ingenhouz, F. R. S. Body Physician to their Imperial Majesties. Who was nominated by the President and Council to prosecute Discoveries in Natural History, pursuant to the Will of the late Henry Baker, Esq., F. R. S. p. 659.

It is now about 15 years since I began to make use of flat glasses, instead of globes or cylinders, to excite electricity. Finding that a greater quantity of electricity could be excited on a flat piece of glass, when rubbed on both surfaces, than when it was only exposed to friction on one side; I thought it would be an advantage to substitute a round plate, or disc, of glass, for a globe or cylinder. I also thought another material advantage might be derived from a plate of glass, as the form of it admits of placing cushions or rubbers on different parts of it, and taking the electricity, excited by these rubbers, from the interstices between them, which cannot conveniently be done when a globe or cylinder is used. The only inconvenience which I at first conceived would ensue from it was, that the centre on which the plate was to be fixed and whirled round, would always be too near the rubbers, unless these were very short, or the plate of a considerable size; and that these would throw the electricity, collected on the surface of the glass, on that very centre. To obviate this difficulty, I proposed to make the centre also of glass, or some other non-conducting substance, as baked wood.

I began first by making use of one of those glass stands, called a waiter, and which has a glass support fixed at right angles to its centre. I whirled the waiter round as well as I could, rubbing it sometimes on one side, sometimes on both. In this imperfect state I showed it to Dr. Franklin, who approved much of the scheme, and advised me to pursue it. Soon after, I showed it to several of my acquaintance, and in a short time I found such machines ready made at Mr. Ramsden's, and at some other mathematical instrument makers. Since that time great use has been made of these electrical plate machines throughout Europe, as they were thought by many to be more powerful in a little compass than those with globes and cylinders.

In my travels through different countries, I now and then met with considerable improvements made in them. Abbé Fontana had contrived one for the cabinet of the great duke of Tuscany, which consisted of 2 plates, of 18 inches diameter, fixed to the same centre, and each rubbed on both sides on 2 opposite places. The electrical fire excited on these joint plates, when forced on a conductor divided in 2 branches to receive it, was very powerful; so much so, that the conductor, being unable to contain the whole, threw it back upon the brass centre; from which it passed to the hand of the operator, and gave him a very disagreeable shock.

Mr. Cuthbertson, an ingenious mathematical instrument maker at Amsterdam,

contrived an apparatus with a double plate, by which all the electrical fire collected by the 8 cushions was forced upon the conductor, so that none of it could be thrown back upon the centre, though made of brass. His contrivance consisted in placing between the glass plates a strong glass ring, about 2 inches in diameter, so that the brass centre passed through the middle of it. This ring was stuck to the plates with sealing wax or some other non-conducting cement; and the space between the centre and the ring was carefully filled with the same non-conducting substance. The conductor had 2 branches, each of which was placed between the 2 glasses, reaching very near to the glass ring. By this method all, or almost all, the electricity excited by the 8 cushions, was forced to pass upon the conductor, there being no way to reach the brass centre, between which and the conductor all communication was cut off by the above mentioned glass ring being filled up with a non-conducting cement. The power of such a machine, notwithstanding the plates were not above 15 inches diameter, was very astonishing. I saw one made in London with this improvement, the glass plates being 18 inches diameter, by which a coated jar of 2 quarts was fully charged in less than 5 seconds.

Mr. C. Cuypers, an ingenious electrician at Delft, has not a little contributed to the improvement of these machines, by making them less liable to be affected by damp weather. This gentleman, considering that all glasses are not equally fit for electricity, and that J. H. Wartz, and after him Professor Musschenbroek, were of opinion, that glass, in the composition of which there enters a great deal of alkaline salt, is very apt to attract moisture from the air, and therefore less proper for electricity, which defect they thought might be corrected by exposing it to a violent and continued heat, took a proper advantage of this knowledge in the improvement of the machines with flat glasses. He found that glasses which have been many years exposed to the warm air of a room, very old looking-glasses for instance, become by this means much harder, so as better to resist the force of a file; and are then much better for electrical machines: and that such glasses become still incomparably better, if they are exposed to a considerable degree of heat during some months: the heat forcing out of the glass, at least out of its surface, the alkaline salt, not vitrified, which is to be found upon it, and may be known by the taste.

In December 1777 I saw one of these double-plated machines at Mr. Cuyper's house, and found it do admirably well, though the weather was at the time very damp, and the machine kept in a room in which there never was any fire made, and though the cushions had no amalgama upon them: they were made of yellow Turkish leather, stuffed with fine shavings of cork, rammed in them; and had been pressed to give them an equal smooth surface. The same gentleman found glasses, prepared in the above-mentioned way, far superior in strength

to a cake of resin used in the electrophorus. He published his method in a pamphlet, *Exposé d'une Methode, &c.* at the Hague, in 1778.

Those who make use of plate machines, should carefully avoid putting the apparatus near the fire, for the purpose of drying or warming it; because the sudden expansion of the glass by the heat cannot so quickly propagate itself through its whole extent; for the centre being commonly squeezed between 2 flat shives of brass, with a piece of leather between the metal and the glass, does not acquire a similar degree of heat at the same time as the rest, and cannot so easily expand; and therefore the plate is in great danger of breaking. If, in consequence of such a blunder, a flaw should happen, its progress might be stopped by drilling a round hole at the extremity of the flaw. These flat glasses may very safely be rubbed with a dry warm cloth.

As the quantity of electricity excited on glass is nearly in the proportion of the surface exposed to friction; and as glasses of a great size are very precious, and liable to accidents, I conceived, that instead of a disc of flat glass, one might substitute one of paste-board, thoroughly imbibed with copal or amber varnish. To try how this would answer, about seven years since I ordered 3 paste-board discs to be made, of 4 feet in diameter, the distance of 6 inches from the centre being the fittest to give the whole a proper support in whirling it round. When these discs were thoroughly dried and heated, I poured upon them a varnish made of amber dissolved in linseed oil. After they had taken in as much of the varnish as they could imbibe, I covered them with a thick coat of the same varnish, and dried them by the heat of a German stove. When the varnish was very hard, I found, that even a slight friction with a cat's skin or hare's skin excited a strong electricity on them.

I then made a frame to place them in, and to whirl them round; which frame was so contrived, that it could contain about 12 such discs, whirling all round on the same centre. It consisted of 2 square pillars of wood, about 5 feet high, and 3 inches broad; joined together at top and bottom by a transverse piece of wood. In the middle of the 2 pillars was a hole, about an inch and a half diameter, fitted to receive a wooden axis, which could be placed in, and taken out, at pleasure. On this axis were to be stuck the paste-board discs; and a flat board, 3 inches broad, covered on both sides with a flannel, and over this with a hare's skin, was to be placed between each paste-board. The 2 square pillars were also to be wrapped up first with flannel, and over that with hare skin.

The flat boards, to be placed between the paste-board discs, had each a notch in the centre, to give room for the axis to turn round freely. These flat boards could be brought as near each other as was required by 2 wooden male screws, placed at the upper and lower end of the frame, which reached from one square pillar to the other; which screws were to receive a notch cut out at the upper

and under end of each flat board, in order to keep them steady in their vertical situation. A female screw, turning on these horizontal male screws, was placed between each of the flat boards at their upper and under extremities, and served to bring each of the discs as near in contact with the hares' skins as was required to receive a proper friction.

The 3 paste-boards were fixed in the frame, and whirled round. The electricity excited was so strong that I took sparks between 1 and 2 feet long from the front surface of the first disc by approaching my knuckle to it. I then applied a tin conductor to it, about 6 feet long and 6 inches diameter, divided into 2 branches; the extremities of which were furnished with a thick silver lace fringe, instead of points. The sparks from this conductor were about 4 or 5 inches long, appeared to be very thick, were very brilliant, and so strong, that I did not chuse to receive many of them; nor did those who came to see the machine care to receive more than one. As these sparks succeeded each other at short intervals, I think they would have been much longer if every thing had been adapted for that purpose; as I saw afterwards done at Mr. Nairne's, who contrived to obtain sparks of 20 inches long and upwards, from a large glass cylinder.

I considered this paste-board machine rather as a rough sketch of an apparatus, by which I conceived the hope of obtaining an electrical power of almost any degree required, than as a complete machine. My intention was to find out a contrivance by which a very great quantity of electrical fire might be collected without great expence, and without much danger of breaking the apparatus, which two articles cannot be avoided when we make use of uncommon sized glass cylinders or discs.

I saw, two years ago, at the duke of Chaulnes at Paris, an apparatus which had a plate glass 5 feet diameter. This alone cost him 800 French livres. As I had not adapted the tin conductor to receive the electricity from the 3 discs, but only from the front disc, I cannot tell whether the force of electricity would have been proportionably stronger if I had made some metallic communication between each of the discs. I found, that the apparatus, as it was constructed, could not easily have admitted more than 3 such large discs; for the 12 surfaces exposed to friction being each a foot and a half long, and above 3 inches broad, made so much resistance to the working of the machine, that it required a strong arm to work it. Being satisfied with having found that by this, or a contrivance of the same kind, a much greater power could be excited than by the common glass apparatuses; I did not chuse to put myself to more trouble or expence to increase the strength of its frame, or the number of discs.

I must observe, that such a machine may be kept in good order in a heated room; but that it will soon lose its force in countries where it is not the custom

to heat rooms as the Germans do. My paste-board discs kept very good during all the time I left them in a room constantly warmed, which was about 2 months; but when they were placed in a cold room, they soon lost their power, having probably attracted moisture from the air. I cannot be sure that the varnish I got made for the purpose, was of the best kind: I have reason to suspect the contrary, and therefore I should think that much better might be obtained in London. Such paste-boards might be possibly preserved from attracting moisture, by keeping them shut up in a box made on purpose, lined within with tin-foil. The moisture might also be expelled again by placing them a good while in a heated room, or on a baker's oven.

It seems besides not improbable, that a kind of paste-board might be made by sticking together the lamina of paper, first thoroughly dried, with a good oil varnish, instead of common paste, as this last never can be deprived of its watery particles without losing its cohesive quality. A good kind of paste-board might likewise be contrived by sticking together silk cloth instead of paper. I contrived a plated machine, the disc of which was made of baked wood, and boiled in linseed oil; but it did not answer near so well as the paste-board discs. I found the paste-board discs not much less susceptible of electricity before I had varnished them, than they were afterwards; but they lost their force again in a few minutes, and did not recover it till they were dried and heated again. It is well known, that writing and brown packing paper, when warmed, may acquire a considerable electrical power by being rubbed with hares' skin, or a piece of wood, or ivory, nay even (as I found by experience) with a metallic body.

As it seems to be a general law of nature, that all resinous bodies, excited either with a positive or a negative electricity, are more tenacious of keeping it, or seem to part with it, as it were, with more reluctance than glass; it is advisable, that the conductor of such a paper machine be not furnished with metallic points, which being necessarily kept at a distance will not take away all the electricity; but that some flexible, conducting substance, as silver or brass lace fringes, communicating with the conductor, be in close contact with the excited surface. As woollen cloth or Manchester cotton velvet, and such like substances, excite a good deal of electricity on dried paper and resinous substances, and do not wear out so soon as hares' skin, it might, perhaps, be found better to substitute them for hares' skin. I have also excited a very considerable electrical force on strong silk velvet, tied upon the circumference of 2 wooden discs, 2 feet in diameter, and distant about 3 feet from each other, fixed on a wooden axis. The velvet was supported by a strong silk cloth tied under it, in order to give it more strength and steadiness. This machine had the appearance of a drum, and was whirled round, as is usually done with glass cylinders. The rubber was a cushion covered with hares' skin. As silk cloth, and more particularly oiled silk, very

easily receives a strong electricity, I make no doubt but a good use might be made of them, by exposing a great surface of them to friction. I have attempted more than one method of constructing such a machine; but as I tried it only in small, I have not pursued the object far enough, and therefore I think I have no right to throw out hints unsupported by experience.

END OF THE SIXTY-NINTH VOLUME OF THE ORIGINAL.

1. *Calculations to determine at what Point in the Side of a Hill its Attraction will be the Greatest, &c.* By Charles Hutton, LL.D., and F. R. S. Anno 1780, Vol. LXX. p. 1.

1. The great success of the experiment, lately made by the R. S., on the hill Schihallien, to determine the universal attraction of matter, and the important consequences that have resulted from it, may probably give occasion to other experiments of the same kind to be made elsewhere: and as all possible means of accuracy and facility are to be desired in so delicate and laborious an undertaking; it may not be unuseful to add, by way of supplement to the paper of calculations relative to the above-mentioned experiment, in the 68th volume, an investigation of the height above the bottom of a hill, at which its horizontal attraction shall be the greatest; since that is the height at which commonly the observations ought to be made, and since this best point of observation has never been any where yet determined, but has been variously spoken of or guessed at, it being sometimes accounted at $\frac{1}{3}$, and sometimes at $\frac{1}{2}$ of the height of the hill; whereas from these investigations it is found to be generally at about only $\frac{1}{4}$ of the altitude from the bottom.

2. Let ABCEDA be part of a cuneus or pyramid of matter, its sides or faces being the 2 similar right-angled triangles ABC, ADE meeting in the point A, and forming the indefinitely small angle BAD. Then, of any section bced, fig. 10, pl. 6, perpendicular to the planes ABD and ADE, the attraction on a body at A in the direction AB, is equal to the constant quantity ss ; where $s = \sin. < BAC$ and $s = \sin. < BAD$, to the radius 1. For, first, the magnitude of the flowing section being every where as Ab^2 , and the attraction of the particles of matter inversely as the same, or as $\frac{1}{Ab^2}$; therefore their product, $\frac{Ab^2}{Ab^2}$ or 1, a constant quantity, is as the force of attraction of bced, whatever its distance may be from the point A. And to find what the quantity of that attraction really is, the author, by a very simple fluxionary process, determines its actual quantity to be ss , as above stated.*

* As this paper will be found at large, among many others, in the author's collection of his original works, we shall limit our abstract here to an account of the method of process, and of the final results.

3. To find now the attraction of the whole right-angled cuneus on a body at A in the direction AB.—Since the force of each section is ss by what is above said, therefore the force of all the sections, the number of them being AB or a , is $ass = s \cdot AB \cdot \frac{BC}{AC}$, the force of the whole cuneus ABCEDA.

4. To find the attraction of any rectangular part ABCD on A in the direction AB, fig. 11; ABCD being one side of a cuneus, and AD its edge. And here, by a like fluxionary process, it is found that $s \cdot BC \times \text{hyp. log. } \frac{AB + AC}{BC}$ is the attraction of ABCD.

5. To find the attraction of the right-angled part BCD of a cuneus whose edge passes through A the place of the body attracted, fig. 12; he puts $AB = a$, $BC = b$, $BD = c$, $DA = d = a - c$, $DC = e$, $AC = g$, and $DP = x$; and then, by a like process, finds that $\frac{bcs}{ee} \times (g - d - \frac{dc}{e} \times \text{hyp. log. } \frac{ee + eg + dc}{de + dc})$ expresses the force on a body at A in the direction AB.

6. Lastly, to find the attraction of the right-angled part BCD on the point A, fig. 13: using here again the same notation as in the last article, in a similar way, he finds that the attraction in this case will be denoted by

$$\frac{bcs}{ee} \times (g - d + \frac{dc}{e} \times \text{hyp. log. } \frac{ee + eg - dc}{de - dc}).$$

7. To apply now these premises to find the place where the attraction of a hill is greatest, it will be necessary to suppose the hill to have some certain figure. That position is most convenient for observing the attraction, in which the hill is most extended in the east and west direction. Supposing then such a position of a hill, and that it is also of a uniform height and meridional section throughout; the point of observation must evidently be equally distant from the 2 ends. But instead of being only considerably extended, Dr. H. supposes the hill to be indefinitely extended to the east and to the west of the point of observation, in order that the investigation may be nearly mathematically true, and yet sufficiently exact for the beforesaid limited extent also. It will also come nearest to the practical experiment, to suppose the hill to be a long triangular prism, so that all its meridional sections may be similar triangles. Let therefore the triangle ABC, fig. 14, represent its section by a vertical plane passing through the meridian, or one side of an indefinitely thin cuneus whose edge is in PG; or rather PBCGP the side of one cuneus, and PAG the side of another, their common edge being the line PG perpendicular to the base AC; P being the required point in the side AB where the attraction of the indefinitely thin cuneus shall be greatest, in a direction parallel to the horizon AC. And then from the foregoing suppositions, it is evident that in whatever point of AB the attraction of ABC is greatest, there also will the attraction of the whole hill be the greatest, very nearly.

8. Now drawing HPDEF parallel to AC; and AH, PG, BI, CF, perpendicular to the same. Then it is evident that at the point P, in the direction PF, the attrac-

tion of PBCGP is affirmative, and that of PAG negative. But PBCGP is = PBD + BDE + PFCG - EFC; and PAG = PHAG - PHA. Therefore the attractions of PBD, BDE, PFCG, PHA, are affirmative; and those of EFC, PHAG, negative. Putting now BI = a , AI = b , IC = c , AB = d , BC = e , AC = $g = b + c$, and PG = x , the altitude of the point P above the bottom: also s = the sine of the indefinitely small angle of the cuneus to rad. 1; and $q^2 = \sqrt{a^2g^2 - 2abgx + d^2x^2}$. Then by the foregoing articles, these several quantities being collected together with their proper signs, and contracted, there is at length obtained the expression, $s \times \left[\frac{ab}{d} + c \cdot \frac{ad - qq - dx}{ce} + x \times \text{h.l.} \frac{ag + qq - bx}{(b+d)x} + \frac{cgg(a-x)}{e^3} \times \text{h.l.} \frac{(ce + de - cg)(acg + eqq + a^2x - bcx)}{gg(cc - cc) \times (a-x)} \right]$, for the whole attraction in the direction PE.

9. Having now obtained a general formula for the measure of the attraction in any sort of triangle, if the particular values of the letters be substituted which any practical case may require, and the fluxion of this attraction be put = 0, the root of the resulting equation will be the required height from the bottom of the hill. But for a more particular solution in simpler terms, let us suppose the triangle ABC to be isosceles, in which case we shall have $d = e$, and $g = 2b = 2c$, and then the above general formula will become

$s \times \left[\frac{2ad - qq - dx}{dd} b + x \times \text{h.l.} \frac{2ab + qq - bx}{(b+d)x} + \frac{a-x}{d^3} \cdot 2b^3 \times \text{h.l.} \frac{2ab^2 + dq^2 - (b^2 - a^2)x}{2b^2(a-x)} \right]$, for the value of the attraction in the case of the isosceles triangle, where q^2 is = $\sqrt{4a^2b^2 - 4ab^2x + d^2x^2}$. And the fluxion of this expression being equated to 0, the equation will give the relation between a and x for any values of b and d , by a process not very troublesome.

10. Now it is probable that the relation between a and x , when the attraction is greatest, will vary with the various relations between b and d , or between b and a . Let us therefore find the limits of that relation, between which it may always be taken, by using 2 particular extreme cases, the one in which the hill is very steep, and the other in which it is very flat, or a very small in respect of b or d . And first let us suppose the triangular section to be equilateral; in which case the angle of elevation is 60° ; which being a degree of steepness that can scarcely ever happen, this may be accounted the first extreme case. Here then we shall have $d = 2b = \frac{2}{3}a\sqrt{3}$, and the formula in art. 9, will become $s \times \left(\frac{2a - r - x}{2} + x \times \text{h.l.} \frac{2a + 2r - x}{3x} + \frac{a-x}{4} \times \text{h.l.} \frac{a + 2r + x}{a-x} \right)$ for the value of the attraction in the case of the equilateral triangle, in which r is = $\sqrt{a^2 - ax + x^2}$.

Or if we take $x = na$, where n expresses what part of a is denoted by x , the last formula will become $sa \times \left(1 - \frac{1}{2}n - \frac{1}{2}\sqrt{1 - n + n^2} + n \times \text{h.l.} \frac{2 - n + 2\sqrt{1 - n + n^2}}{3n} + \frac{1-n}{4} \times \text{h.l.} \frac{1 + n + 2\sqrt{1 - n + n^2}}{1-n} \right)$ for the case of the equilateral triangle.

11. To find the maximum of the expression in the last article, put its fluxion = 0, and there will result this equation

$1 + \frac{1+n}{\sqrt{(1-n+n^2)}} = 2 \text{ h. l. } \frac{2-n+2\sqrt{(1-n+n^2)}}{3n} - \frac{1}{2} \text{ h. l. } \frac{1+n+2\sqrt{(1-n+n^2)}}{1-n}$; the root of which is $n = .251999$. Which shows that, in the equilateral triangle, the height from the bottom to the point of greatest attraction, is only $\frac{1}{5.06}$ part more than $\frac{1}{4}$ of the whole altitude of the triangle. And this is the limit for the steepest kind of hills.

12. Let us find now the particular values of the measure of attraction arising by taking certain values of n varying by some small difference, in order to discover what part of the greatest attraction is wanting by observing at different altitudes. And first using the value of n , viz. $.251999$, as found in the last article, the general formula in art. 10, gives $sa \times 1.0763700$, for the measure of the greatest attraction.—Again, if $n = \frac{3}{10}$, or $x = \frac{3}{10}a$; the same formula gives $\frac{sa}{20} \times (17 - \sqrt{79} + 6 \text{ h. l. } \frac{17+2\sqrt{79}}{9} + \frac{7}{2} \text{ h. l. } \frac{13+2\sqrt{79}}{7}) = sa \times 1.0702512$, for the attraction at $\frac{3}{10}$ of the altitude; which is something less than the other.—

And if $n = \frac{4}{10} = \frac{2}{5}$; the formula gives

$\frac{sa}{20} \times (16 - \sqrt{76} + 8 \text{ h. l. } \frac{8+\sqrt{76}}{6} + 3 \text{ h. l. } \frac{7+\sqrt{76}}{3}) = sa \times 1.0224232$, for the attraction at $\frac{4}{10}$ or $\frac{2}{5}$ of the altitude; less again than the last was.—Also, if $n = \frac{5}{10} = \frac{1}{2}$; the formula gives $\frac{1}{4}sa \times [3 - \sqrt{3} - 2 \text{ h. l. } 3 + \frac{5}{2} \text{ h. l. } (3 + 2\sqrt{3})] = sa \times .9340963$, for the attraction at half way up the hill; still less again than the last.—Further, if $n = \frac{6}{10} = \frac{3}{5}$; the formula gives

$\frac{sa}{20} \times (14 - \sqrt{76} + 12 \text{ h. l. } \frac{7+\sqrt{76}}{9} + 2 \text{ h. l. } \frac{8+\sqrt{76}}{2}) = sa \times .8109843$, for the attraction at $\frac{6}{10}$ or $\frac{3}{5}$ of the altitude from the bottom; being still less than the last was. And thus the quantity of attraction is continually less and less the higher we ascend up the hill above the $.251999$ part, or in round numbers $.252$ part of the altitude. Let us now descend, by trying the numbers below $.252$.

And 1st, if $n = .25 = \frac{1}{4}$; the same formula in art. 10 gives $\frac{1}{8}sa \times (7 - \sqrt{13} + 2 \text{ h. l. } \frac{7+2\sqrt{13}}{3} + \frac{3}{2} \text{ h. l. } \frac{5+2\sqrt{13}}{3}) = sa \times 1.0763589$, for the attraction at $\frac{1}{4}$ of the altitude; and is very little less than the maximum.—If $n = \frac{2}{10} = \frac{1}{5}$; the formula gives $\frac{1}{10}sa \times (9 - \sqrt{21} + 2 \text{ h. l. } \frac{9+2\sqrt{21}}{3} + 2 \text{ h. l. } \frac{3+\sqrt{21}}{2}) = \frac{1}{10}sa \times (9 - \sqrt{21} + 2 \text{ h. l. } \frac{23+5\sqrt{21}}{2}) = sa \times$

1.0684622 , for the attraction at $\frac{2}{10}$ or $\frac{1}{5}$ of the altitude; and is something less than at $\frac{1}{4}$ of the altitude.—If $n = \frac{1}{10}$; the formula gives $\frac{sa}{20} \times (19 - \sqrt{91} + 2 \text{ h. l. } \frac{19+2\sqrt{91}}{3} + \frac{9}{2} \text{ h. l. } \frac{11+2\sqrt{91}}{9}) = sa \times .9986188$, for the attraction at $\frac{1}{10}$ of the altitude; still less than the last was.—And, lastly, if $n = 0$, or the point be at the bottom of the hill; the formula gives $\frac{1}{4}sa \times (2 +$

h. l. 3) = $sa \times .7746531$, for the attraction at the bottom of the hill; which is between $\frac{2}{3}$ and $\frac{3}{4}$ of the greatest attraction, being something greater than $\frac{2}{3}$ but less than $\frac{3}{4}$ of it.

13. The annexed table exhibits a summary of the calculations made in the preceding cases; where the 1st column shows at what part of the altitude of the hill the observation is made; the 2d column contains the corresponding numbers which are proportional to the attraction; and the 3d column shows what part of the greatest attraction is lost at each respective place of observation, or how much each is less than the greatest.

$\frac{6}{10}$	8109843	$\frac{1}{4}$
$\frac{5}{10}$	9340963	$\frac{2}{13}$
$\frac{4}{10}$	10224232	$\frac{2}{5}$
$\frac{3}{10}$	10702512	$\frac{1}{80}$
$\frac{252}{1000}$	10763700	0
$\frac{1}{4}$	10763589	$\frac{1}{97852}$
$\frac{2}{10}$	10684622	$\frac{1}{34}$
$\frac{1}{10}$	9986188	$\frac{1}{14}$
0	7746531	$\frac{2}{7}$

14. Having now so fully illustrated the case of the first extreme, or limit, let us search what is the limit for the other extreme, that is, when the hill is very low or flat. In this case b is nearly equal to d , and they are both very great in respect of a ; consequently the formula for the attraction in art. 9, will become barely $s \times (x \times \text{h. l. } \frac{2a-x}{x} + 2(a-x) \times \text{h. l. } \frac{2a-x}{a-x})$; the fluxion of which being put = 0, we obtain $0 = \text{h. l.}$

$\frac{2a-x}{x} - 2 \text{ h. l. } \frac{2a-x}{a-x} = \text{h. l. } \frac{2a-x}{x} - \text{h. l. } (\frac{2a-x}{a-x})^2 = \text{h. l. } \frac{(a-x)^2}{x(2a-x)}$; hence therefore $(a-x)^2 = x(2a-x)$, and $x = a \times (1 - \sqrt{\frac{1}{2}}) = .2929a$. Which shows that the other limit is $\frac{2929}{1000}$; that is, when the hill is extremely low, the point of greatest attraction is at $\frac{2929}{1000}$ of the altitude, like as it is at $\frac{295}{1000}$ when the hill is very steep. And between these limits it is always found, it being nearer to the one or the other of them, as the hill is flatter or steeper.

15. Thus then we find, that at $\frac{1}{4}$ of the altitude, or very little more, is the best place for observation, to have the greatest attraction from a hill in the form of a triangular prism of an indefinite length. But when its length is limited, the point of greatest attraction will descend a little lower; and the shorter the hill is, the lower will that point descend. For the same reason, all pyramidal hills have their place of greatest attraction a little below that above determined. But if the hill have a considerable space flat at the top, after the manner of a frustum, then the said point will be a little higher than as above found. Commonly, however, $\frac{1}{4}$ of the altitude may be used for the best place of observation, as the point of greatest attraction will seldom differ sensibly from that place. And when uncommon circumstances may produce a difference too great to be entirely neglected, the observer must exercise his judgment in guessing at the necessary change he ought to make in the place of observation, so as to obtain the best effect which the concomitant circumstances will admit of.

II. Of some New Experiments in Electricity, with the Description and Use of Two New Electrical Instruments. By Mr. Tiberius Cavallo, F.R.S. p. 15.

Professor Lichtenberg, of Gottingen, some time ago made an experiment on the electrophorus, an account of which was first received in London towards the latter end of the year 1777. The phenomena attending the experiment are very entertaining and various, but no person ever offered a satisfactory explanation of them. The author himself, in his paper entitled “*De nova methodo naturam ac motum Fluidi Electrici investigandi Commentatio prior*,” where he gives an account of the experiment, does not attempt any explanation of it; contenting himself with the account only of various particulars attending it. Mr. C. therefore here gives the explanation. But the whole may better be consulted in the 2d volume of his Treatise on Electricity, in 2 vols. 8vo.

III. A New Method of Assaying Copper Ores. By George Fordyce, M.D., F. R. S. p. 30.

Process.—Take 100 grs. of the ore, powder it finely, put it into a small matrass or a glass phial, pour on it $\frac{1}{2}$ oz. of nitrous acid, of the strength commonly sold by the name of aquafortis, that is, the pure acid diluted with about 4 times its weight of water; and $\frac{1}{2}$ oz. of muriatic acid, sold by the name of spirit of salt; place the vessel in a sand heat, or if you have none, an iron pot or fire shovel with sand may be set over a common fire, and the matrass or phial set in it. Raise a moderate heat; an effervescence will take place for the most part; when this ceases increase the heat till it is renewed, and so proceed till the liquor boils, which is also to be done if no effervescence takes place; boil them together for $\frac{1}{4}$ of an hour. Remove the vessel from the fire, and let it cool, then pour into it 2 oz. of water, shake them together, and let them stand till the liquor is clear; pour the clear liquor into a basin where it may be preserved. Add to the residuum a fresh $\frac{1}{2}$ oz. of each of the acids, and proceed again in the same manner, mixing the clear liquor with that procured by the first process. The same operation is to be repeated till the fresh acids acquire no tinge of blue or green.

Dissolve $\frac{1}{2}$ lb. of mild fixed vegetable alkali, commonly sold by the name of salt of tartar, in a quart of water. Purify the solution either by filtration, or letting the impurities subside, and decanting the liquor clear into a glass vessel. Pour the solution of the alkali slowly into the basin containing the fluid procured by the former processes, till the whole matter be precipitated from the acids.

Add, by a little at a time, as much vitriolic acid, commonly sold by the name of oil of vitriol, as will re-dissolve the whole, or only leave a white powder; if there should be any such powder, which is seldom the case, it must be separated

by filtration. Having the liquor in the basin now clear, put into it a piece of iron, bright and free from rust, and at least 1 oz. in weight, and leave them together for 24 hours; the copper will be found precipitated, principally on the surface of the iron, and sometimes in a powder at the bottom of the basin.

Decant the fluid from the copper and iron with great care into another basin, so that as little as possible or none of the copper be carried along with it. Wash the metals in a pint of water; let them subside perfectly, and pour this water into the 2d basin, with the same care, repeat the washing 3 times. If any copper be found in the 2d basin, let the washings stand in it for $\frac{1}{2}$ an hour, so that the metal shall subside; decant the fluid carefully off, and return the copper into the first basin. Pour on the copper and iron 1 oz. of vitriolic acid, and 2 oz. of water; let them stand together for $\frac{1}{4}$ of an hour, or till the copper shall be easily separable from the iron. Separate the copper from the iron, taking great care none be lost; the remaining iron may be laid aside. Pour the acid from the copper, after it has subsided, into the 2d basin, and wash the copper with a pint of water, and repeat the washing 3 times, as before directed.

Great care is to be taken, in decanting both the acid and washings into the 2d basin, that none of the copper goes along with them, and lest any should, they ought to stand for $\frac{1}{2}$ an hour in the 2d basin, and be decanted from it also with care, and if any copper is found at the bottom, it is to be washed and added to the rest. The copper is now to be dried and weighed, and gives the proportion contained in the ore.

Observations on the above process.—It was about 20 years before that Dr. F. contrived some methods of assaying ores, which might avoid tedious and troublesome roastings and fusions in great degrees of heat, which require a dexterity which is only to be acquired by great practice, and which, after all, form a process that is often various in the result, and seldom shows the substances contained in the ore, excepting the metal. The principles on which these processes depend, as far as regards copper ores, are,

1°. Metals are attracted more strongly by acids than by sulphur, with which they are often combined in their ores. In consequence, if a metal be combined with sulphur in an ore, it may be separated by applying an acid, which will unite with the metal, and separate the sulphur. The metal may generally be separated from the acid in its metallic form by means of another metal, which attracts the acid more strongly.

2°. Arsenic unites with vitriolic, nitrous, and muriatic acids, forming a corrosion or compound not soluble in water; whereas most other metals may be united with one of these acids, or a mixture of them, so as to form a compound soluble in water: therefore, if there be arsenic combined with a metal in an ore,

if it be dissolved in such acid diluted with water, the arsenic will fall to the bottom in a white powder or crystals, and the solution being poured off will contain the metal, which may be separated from the acid by another metal as before.

3°. The calces of metals may be dissolved in acids, whether they be pure (of which there are few instances in ores) or combined with gas, respirable air, or other vapours; therefore, if the metal in an ore be in the form of a calx, we may find an acid which will dissolve it, and we may afterwards precipitate it in its metallic form as before.

4°. When an ore is to be assayed, it should be separated from the quartz, spars, and other earthy matters, with which it is often mixed, as perfectly as possible; however, after all our care there will be often a part of them so intimately mixed with the ore, that it cannot be entirely cleared. Many of these earthy matters do not dissolve readily in acids: therefore, if the metal of an ore be dissolved in an acid, so as to form a compound soluble in water, the solution of the metal may be poured off, leaving such earthy matters behind.

5°. If the earthy matter should dissolve in the acid, it is seldom to be precipitated by a metal: therefore if both earth and metal be dissolved, on the application of another metal, which attracts the acid more strongly, that which was combined with the acid will be precipitated, and the earth left in the solution.

6°. Acids attract the metals with different powers: therefore, if two metals be combined with an acid, and we apply to the solution a mass of that which attracts the acid strongest, the other will be precipitated. The mass being weighed before and after the precipitation, the difference will be the quantity of additional metal dissolved; if therefore we pour off the liquid from the precipitate, and apply another metal, which attracts the acid still more strongly, the 2d metal will be precipitated, which being weighed, and the weight lost from the mass deducted, gives the weight of the 2d metal. As this principle is of great use in investigating the elements of mixed metals, we shall give an example. Suppose copper and silver mixed; dissolve the whole in pure nitrous acid, properly diluted with water; apply to the solution a mass of copper, the silver will be precipitated. Pour off the solution, and wash the silver and undissolved copper with water; pour the washings into the solution; weigh the mass of copper left, and mark what it has lost; apply to the solution a mass of iron, the whole copper will be precipitated. Pour off the fluid, and wash the precipitate carefully, dry it, and weigh it, deduct the weight lost from the mass of copper, what remains is the weight of the copper in the mixture; if this weight, together with that of the silver, be the weight of the metal originally exposed to examination, there is no reason to suspect any mixture of another metal.

If the metals mixed are unknown, and we can find an acid which will dissolve

them, we may try to make a precipitation with the metal which is lowest but one in the order of elective attractions, and so proceed to the next above it, till we come to the highest; and by this means we shall obtain all the metals in the mass. There are other principles on which Dr. F. founded various processes for assaying; but these are sufficient for copper ores, all the different known species of which he actually assayed, and therefore ventured to offer this process to the consideration of the R. S.; 1°. as only requiring an apparatus which can be bought at any apothecary's or chemist's, and as capable of being performed by a person totally unacquainted with chemistry, so that any proprietor of an estate, or his servant, may determine if an ore be of copper, and its value; 2°. as affording an assay-master a more perfect manner of determining the value of a copper ore; and lastly, as a process by which the naturalist may investigate not only the copper in an ore, but its various other contents. There is but one known species of copper ore in which the copper is not capable of being combined with aqua regia, which is blue vitriol, which is sometimes found solid, but more frequently in mineral waters; from this the copper may be precipitated by iron immediately.

Many opinions, (Dr. F. remarks) have been published of metals being found in mineral waters combined with various substances. He never examined any mineral water in which he found the metals combined with any other substance but vitriolic acid; and he was certain, many authors had been misled by not knowing this property of metallic salts, viz. that if we dissolve them in a small proportion of water, or if there be superfluous acid, the solution will remain perfect when exposed to the air; but if the acid be perfectly saturated with the metal, and the proportion of water to the metallic salt be very great, on exposure to the air it is decomposed, the metal precipitating in the form of a calx, and the acid being lost. This may easily be tried by taking common green or blue vitriol, dissolving 1 oz. in 3 oz. of water by boiling, letting them stand to cool, and filtering the solution. If this solution be exposed to the air it will remain perfect; but if we drop a drop or 2 of it into a wine glass full of water, in a few minutes the transparency of the water will begin to be disturbed, and the metal in a short time will fall down, in a red powder if it be of iron, in a blue powder if it be copper. A hundred grains of the ore is sufficient to give the copper contained to $\frac{1}{100}$ part; if greater accuracy be required, 1000 grs. may be used.

The mixture of nitrous and muriatic acid is the most proper acid menstruum for copper ores, muriatic acid dissolving most readily the calces of metals, and nitrous acid when they are in their metallic form; a metal in its metallic form being a compound of a pure calx and a substance, which has been called inflammable air, but which is an oil found out by Stahl to exist in metals, and which

we would call the oil of metals. The nitrous acid decomposes this oil, at the same time that it acts on the calx itself, and leaves it also to be acted on by the muriatic acid. When copper is combined with sulphur in an ore, it is in its metallic form; in dissolving in an acid its oil rises in vapour; or vapours produced by the decomposition of this oil occasion an effervescence.

All the calces of copper Dr. F. had tried are combined with gas, respirable air, or other vapours, excepting one, which is of a light green colour, brittle, and which breaks smooth like glass; a specimen of it is contained in Dr. Hunter's museum: this dissolves without effervescence; the others all effervesce. A boiling heat is necessary to render the solution complete, of which great care is to be taken. If there be any sulphur in the ore, it appears quite clear in lumps; a small portion of it however is destroyed by the nitrous acid. Earthy matters insoluble in acids, if any, and arsenic, appear in a powder at the bottom. If there be any silver, it is mixed with this powder, and is to be extracted by melting it with black flux and litharge, and cupelling in the common way. If there be any gold, it may be taken out of the solution by æther.

When the copper is combined with nitrous and muriatic acids, it might be thought sufficient to apply the iron immediately; but it is much more convenient to precipitate it from them, and combine it with vitriolic acid, on account of the convenience of washing the precipitate, which is in a more compacted mass. If there be any calcareous earth dissolved, the vitriolic acid will combine with it, and form a white powder, which will be left after the copper is re-dissolved, and must be separated carefully from the solution. After the precipitation of the copper, it is necessary to get rid of the salts perfectly, before applying the vitriolic acid, otherwise part of the copper would be re-dissolved. Vitriolic acid will not dissolve copper in its metallic form, and may be applied to dissolve any iron that may be mixed with the precipitate, as well as to loosen copper, which sometimes adheres to the iron. The solution of the iron must be carefully washed off from the copper.

There is a criterion by which we may judge certainly if any of the copper be lost. Let all the washings and every thing, except the copper, be put into a vessel together; pour in solution of fixed alkali, till no further precipitation takes place; let the precipitate subside, and pour off the liquor; apply to the precipitate solution of volatile alkali, sold by the name of spirit of sal ammoniac; shake them together, and let them stand for an hour; if the solution of the alkali acquires a purplish blue colour, the process is imperfect, if it does not, it is perfect. If the process be imperfect, which is always for want of care in the decantations, pour in as much vitriolic acid as will dissolve the whole precipitate; apply iron to the solution, and the remaining copper will be procured.

IV. On the Eruption of Mount Vesuvius in August, 1779. In a Letter from Sir Wm. Hamilton, K. B., F. R. S. p. 42.

Since the eruption of 1767, Vesuvius has never been free from smoke, nor ever many months without throwing up red-hot scorïæ, which, increasing to a certain degree, were usually followed by a current of liquid lava, and except in the eruption of 1777, those lavas broke out nearly from the same spot, and ran much in the same direction, as that of the famous eruption of 1767. No less than 9 such eruptions are recorded since that great one, and some of them were considerable. The lavas, when they either boiled over the crater, or broke out from the conical parts of the volcano, constantly formed channels as regular as if they had been cut by art down the steep part of the mountain, and, while in a state of perfect fusion, continued their course in those channels, which were sometimes full to the brim, and at other times more or less so, according to the quantity of matter in motion.

These channels, on examination after an eruption, are found to be in general from 2 to 5 or 6 feet wide, and 7 or 8 feet deep. They are often hid from the sight by a quantity of scorïæ forming a crust over them, and the lava having been conveyed in a covered way for some yards, come out fresh again into an open channel. After an eruption Sir W. had walked in some of those subterraneous or covered galleries, which were exceedingly curious, the sides, top, and bottom, being worn perfectly smooth and even in most parts by the violence of the currents of the red-hot lavas, which they had conveyed for many weeks successively; in others, the lava had incrustated the sides of those channels with some very extraordinary scorïæ: beautifully ramified white salts, in the form of dropping stalactites, were also attached to many parts of the ceiling of those galleries. It is imagined here, that the salts of Vesuvius are chiefly ammoniac, though often tinged with green, deep, or pale yellow, by the vapour of various minerals.

In the month of May last, (1779), there was a considerable eruption of Mount Vesuvius, when Sir W. passed a night on the mountain in the company of Mr. Bowdler, of Bath, as eager as himself in the pursuit of this branch of natural history. They saw the operation of the lava, in the channels as above-mentioned, in the greatest perfection. After the lava had quitted its regular channels, it spread itself in the valley, and, being loaded with scorïæ, ran gently on, like a river that had been frozen, and had masses of ice floating on it: the wind changing when they were close to this gentle stream of lava, which might be about 50 or 60 feet in breadth, incommoded them so much with its heat and smoke, that they must have returned without having satisfied their curiosity, had not the guide proposed the expedient of walking across it.

which he instantly put in execution, and with so little difficulty, that they followed him without hesitation, having felt no other inconveniency than what proceeded from the violence of the heat on their legs and feet; the crust of the lava was so tough, besides being loaded with cinders and scorix, that their weight made not the least impression on it, and its motion was so slow, that they were not in any danger of losing their balance and falling on it. Having thus got rid of the troublesome heat and smoke, they coasted the river of lava and its channels up to its very source, within a quarter of a mile of the crater. The liquid and red-hot matter bubbled up violently, with a hissing and crackling noise, like that which attends the playing off of an artificial firework, and by the continual splashing up of the vitrified matter, a kind of arch or dome was formed over the crevice from which the lava issued. It was cracked in many parts, and appeared red-hot within, like a heated oven: this hollowed hillock might be about 15 feet high, and the lava that ran from under it was received into a regular channel, raised on a sort of wall of scorix and cinders, almost perpendicularly, of about the height of 8 or 10 feet, resembling much an ancient aqueduct. They then went up to the crater of the volcano, in which they found, as usual, a little mountain throwing scorix and red-hot matter with loud explosions; but the smoke and smell of sulphur was so intolerable, that they were under the necessity of quitting that curious spot with the utmost precipitation.

After this slight sketch of the most remarkable events on Vesuvius since the year 1767, Sir W. comes to the account of the late eruption, which affords indeed ample matter for curious speculation. The usual symptoms of an approaching eruption, such as rumbling noises and explosions within the bowels of the volcano, a quantity of smoke issuing with force from its crater, accompanied at times with an emission of red-hot scorix and ashes, were manifest, more or less, during the whole month of July; and towards the end of the month, those symptoms were increased to such a degree, as to exhibit in the night-time the most beautiful fire-works that can be imagined. These kinds of throws of red-hot scorix and other volcanic matter, which at night are so bright and luminous, appear in broad day-light like so many black spots in the midst of the white smoke; and it is this circumstance that occasions the vulgar and false supposition, that volcanoes burn much more violently at night than in the day-time.

On the 5th of August, about 2 o'clock in the afternoon, Sir W. perceived, from his villa at Pausilipo in the bay of Naples, whence he had a full view of Vesuvius, which is just opposite, and at the distance of about 6 miles in a direct line from it, that the volcano was in a most violent agitation: a white and sulphureous smoke issued continually and impetuously from its crater, one puff

impelling another, and by an accumulation of those, clouds of smoke resembling bales of the whitest cotton. Such a mass of them was soon piled over the top of the volcano as exceeded the height and size of the mountain itself at least 4 times. In the midst of this very white smoke, an immense quantity of stones, scorix, and ashes, were shot up to a wonderful height, certainly not less than 2000 feet. He could also perceive, by the help of an excellent refracting telescope, at times, a quantity of liquid lava, seemingly very weighty, just heaved up high enough to clear the rim of the crater, and then take its course impetuously down the steep side of Vesuvius, opposite to Somma. Soon after a lava broke out on the same side from about the middle of the conical part of the volcano, and, having run with violence some hours, ceased suddenly, just before it had arrived at the cultivated parts of the mountain above Portici, near 4 miles from the spot where it issued.

During this day's eruption the heat was intolérable at the towns of Somma and Ottaiano; and was likewise sensibly felt at Palma and Lauro, which are much farther from Vesuvius than the former. Minute ashes, of a reddish hue, fell so thick at Somma and Ottaiano, that they darkened the air in such a manner, as that objects could not be distinguished at the distance of 10 feet. Long filaments of a vitrified matter like spun-glass were mixed and fell with these ashes;* and the sulphureous smoke was so violent, that several birds in cages were suffocated, the leaves of the trees near Somma and Ottaiano were covered with white salts very corrosive. It was generally remarked, that the explosions of the volcano were attended with more noise during this day's eruption, than in any of the succeeding ones; when probably the mouth of Vesuvius was widened, and the volcanic matter had a freer passage.

August 6, the fermentation in the mountain was less violent; but about noon there was a loud report, at which time it was supposed, that a portion of the little mountain within the crater had fallen in. At night the throws from the crater increased, and proceeded evidently from 2 separate mouths, which emitting red-hot scorix, and in different directions, formed a most beautiful and almost continued fire-work.

August 7, the volcano remained much in the same state; but, about midnight, its fermentation increased greatly. The 2d fever-fit of the mountain may be said to have manifested itself at this time. Sir W. was watching its motions from the mole of Naples, which has a full view of the volcano, and had been

* During an eruption of the volcano in the isle of Bourbon in 1766, some miles of country, at the distance of 6 leagues from that volcano, were covered with a flexible, capillary, yellow glass, some of which were 2 or 3 feet long, with small vitreous globules at a little distance one from the other, which perfectly resembled the filaments of vitrified matter which fell at Ottaiano and in other parts on the borders of Vesuvius during this eruption.—Orig.

witness to several glorious picturesque effects produced by the reflection of the deep red fire, which issued from the crater of Vesuvius, and mounted up in the midst of the huge clouds, when a summer storm, called here a *tropea*, came on suddenly, and blended its heavy watry clouds with the sulphureous and mineral ones, which were already like so many other mountains, piled over the summit of the volcano; at this moment a fountain of fire was shot up to an incredible height, casting so bright a light, that the smallest objects could be clearly distinguished at any place within 6 miles or more of Vesuvius. That which followed the next evening was indeed much more formidable and alarming; but this was more beautiful and sublime than even the most lively imagination can paint to itself. This great explosion did not last above 8 or 10 minutes, after which Vesuvius was totally eclipsed by the dark clouds, and there fell a heavy shower of rain. Some scoriæ and small stones fell at Ottaiano during this eruption, and some of a very great size in the valley between Vesuvius and the Hermitage.

August 8, Vesuvius was quiet till towards 6 o'clock in the evening, when a great smoke began to gather again over its crater, and about an hour after, a rumbling subterraneous noise was heard in the neighbourhood of the volcano; the usual throws of red-hot stones and scoriæ began, and increased every instant. At about 9 o'clock there was a loud report, which shook the houses at Portici and its neighbourhood to such a degree as to alarm their inhabitants, and drive them out into the streets; and many windows were broken, and walls cracked, by the concussion of the air from that explosion, though faintly heard at Naples. In an instant a fountain of liquid transparent fire began to rise, and, gradually increasing, arrived at so amazing a height as to strike every beholder with the most awful astonishment. The height of this stupendous column of fire could not be less than 3 times that of Vesuvius itself, which rises perpendicularly near 3700 feet above the level of the sea. Puffs of smoke, as black as can possibly be imagined, succeeded each other hastily, and accompanied the red-hot, transparent, and liquid lava, interrupting its splendid brightness here and there by patches of the darkest hue. Within these puffs of smoke, at the very moment of their emission from the crater, could be perceived a bright, but pale, electrical fire, briskly playing about in zig zag lines.*

The liquid lava, mixed with stones and scoriæ, after having mounted at the least 10 thousand feet, was partly directed by the wind towards Ottaiano, and partly falling almost perpendicularly, still red-hot and liquid, on Vesuvius, covered its whole cone, part of that of the mountain of Somma, and the valley between them. The falling matter being nearly as vivid and inflamed as that

* Sir W. mentions this circumstance to prove, that the electrical matter, so manifest during this eruption, actually proceeded from the bowels of the volcano, and was not attracted from a great height in the air, and conducted into its crater by the vast column of smoke.

which was continually issuing fresh from the crater, formed with it one complete body of fire, which could not be less than 2 miles and a half in breadth, and of the extraordinary height abovementioned, casting a heat to the distance of at least 6 miles around it. The brush wood on the mountain of Somma was soon in a blaze, which flame, being of a different tint from the deep red of the matter thrown out of the volcano, and from the silvery blue of the electrical fire, still added to the contrast of this most extraordinary scene. The black cloud, increasing greatly, once bent towards Naples, and seemed to threaten this fair city with speedy destruction; for it was charged with electric matter, which kept constantly darting about it in strong and bright zig zags, just like those described by Pliny the younger in his letter to Tacitus, and which accompanied the great eruption of Vesuvius that proved fatal to his uncle. This volcanic lightning however very rarely quitted the cloud, but usually returned to the great column of fire towards the crater of the volcano whence it originally came. Once or twice indeed, he saw this lightning (or *ferilli* as it is called here) fall on the top of Somma, and set fire to some dry grass and bushes.

After the column of fire had continued in full force near half an hour, the eruption ceased all at once, and Vesuvius remained sullen and silent. After the dazzling light of the fiery mountain, all seemed dark and dismal, except the cone of Vesuvius, which was covered with glowing cinders and scoriæ, from under which, at times, here and there, small streams of liquid lava escaped, and rolled down the steep sides of the volcano. In the parts of Naples nearest Vesuvius, while the eruption lasted, a mixed smell, like that of sulphur, with the vapours of an iron foundry, was sensible; but nearer to the mountain that smell was very offensive.

August 9, about 9 o'clock in the morning, the 4th fever-fit of the mountain began to manifest itself by the usual symptoms, such as a subterraneous boiling noise, violent explosions of inflamed matter from the crater of the volcano, accompanied with smoke and ashes, which symptoms increased every instant. The smoke was of two sorts; the one as white as snow, the other as black as jet. The white, as described in the former part of this journal, rolled gently mass over mass, resembling bales of the softest cotton; and the black, composed of scoriæ and minute ashes, shot up with force in the midst of the white smoke, which, from the minerals, was also sometimes tinged with yellow, blue, and green. Presently such a tremendous mass of these accumulated clouds stood over Vesuvius as seemed to threaten Naples again, and actually made the mountain itself appear a mole-hill. This day's eruption was similar to that of the 5th, but many degrees more violent. Some stones, thrown near as high as those of last night, fell on the mountain of Somma, and set fire to the brush wood with which it is covered; but there being little wind, and that westerly, the

volcanic matter rose and fell in a more perpendicular direction, and Ottaiano did not suffer by the day's eruption; but most of the inhabitants of the towns, on the borders of Vesuvius, fled to Naples, alarmed by the tremendous clouds and the loud explosions. Sir W. remarked, that several very large stones, after having mounted an immense height, formed a parabola, leaving behind them a trace of white smoke that marked their course: some burst in the air exactly like bombs, and others fell into the valley between Somma and Vesuvius without bursting; others again burst into a thousand pieces soon after their emission from the crater: they might very properly be called volcanic bombs.

On the whole, this day's eruption was very alarming: until the lava broke out, about 2 o'clock, and ran 3 miles between the 2 mountains, they were in continual apprehension of some fatal event. It continued to run about 3 hours, during which time every other symptom of the mountain fever gradually abated, and at 7 at night all was calm. It was universally remarked, that the air this night for many hours after the eruption, was filled with meteors, such as are vulgarly called falling stars; they shot generally in a horizontal direction, leaving a luminous trace behind them, but which quickly disappeared.

August 11, about 6 in the morning, the 5th and last fever-fit of the mountain came on, and gradually increased. About 12 o'clock it was at its height, and very violent indeed, the explosions being louder than those that attended the former eruptions. The same mountains of white cotton-like clouds, piled one over another, rose to such an extraordinary height, and formed such a colossal mass over Vesuvius, as cannot possibly be described, or scarcely imagined. It may have been from a scene of this kind, that the ancient poets took their ideas of the giants waging war with Jupiter. About 5 in the evening the eruption ceased, some rain having fallen this day, which having been greatly impregnated with the corrosive salts of the volcano, did much damage to the vines in its neighbourhood.

August 15, Sir W. went to visit Ottaiano and Caccia-bella, the district which had been most severely treated by the heavy and destructive shower of volcanic matter from the crater of Vesuvius on the 8th. Soon after having passed the town of Somma, he began to perceive, that the heat of the fiery shower, which had fallen in its neighbourhood, had affected the leaves of the trees and vines, which were more parched and shrivelled in proportion as he approached the town of Ottaiano, which may be about 3 miles from Somma. At about the distance of a mile from Somma, he began to perceive fresh cinders or scoriæ of lava, thinly scattered on the road and in the fields. At every step advanced he found them of a larger dimension, and in greater abundance. At the distance of a mile, and a half from Ottaiano, the soil was totally covered by them, and the leaves and fruits were either entirely stripped from the trees, or remained thinly on

them, shrivelled and dried up by the intense heat of the volcanic shower. After having passed through the most fertile country, through villages crowded with chearful inhabitants, to come at once to such a scene of desolation and misery, affording to their view nothing but heaps of black cinders and ashes, blasted trees, ruined houses, with a few of their scattered inhabitants just returned with dismayed countenances, to survey the havock done to their habitations, and from which they had with much difficulty escaped alive on Sunday last, was a most melancholy scene.

The roof of his Sicilian Majesty's sporting seat at Caccia-bella was much damaged by the fall of large stones and heavy scorix, some of which, after having been broken by their fall through the roof, still weighed upwards of 30 pounds. This place, in a direct line, cannot be less than 4 miles from the crater of Vesuvius. The most authentic accounts were received of the fall of small volcanic stones and cinders, some of which weighed 2 ounces, at Benevento, Foggia, and Monte Mileto, upwards of 30 miles from Vesuvius; but what is most extraordinary, as there was but little wind during the eruption of the 8th of August, minute ashes fell thick that very night on the town of Manfredonia, which is at the distance of an hundred miles from Vesuvius. These facts seem to confirm the supposed extreme height of the column of fire that issued from the crater of Vesuvius on the 8th, and are greatly in support of what is recorded in the history of Vesuvius with respect to the fall of its ashes at an amazing distance, and in a short space of time, during its violent eruptions.

Proceeding from Caccia-bella to Ottaiano, which is a mile nearer to Vesuvius, and is reckoned to contain 12,000 inhabitants, nothing could be more dismal than the sight of this town, unroofed, half buried under black scorix and ashes, all the windows towards the mountain broken, and some of the houses themselves burnt, the streets choaked up with these ashes; in some that were narrow, the stratum was not less than 4 feet thick, and a few of the inhabitants just returned were employed in clearing them away, and piling up the ashes in hillocks to get at their ruined houses. Some monks, who were in their convent during the whole of the horrid shower, gave the following particulars, which they related with solemnity and precision.

The mountain of Somma, at the foot of which Ottaiano is situated, hides Vesuvius from its sight, so that till the eruption became considerable it was not visible to them. On the 8th, when the noise increased, and the fire began to appear above the mountain of Somma, many of the inhabitants of this town flew to the churches, and others were preparing to quit the town, when a sudden violent report was heard; soon after which they found themselves involved in a

thick cloud of smoke and minute ashes: a horrid clashing noise was heard in the air, and presently fell a deluge of stones and large scorixæ, some of which scorixæ were of the diameter of 7 or 8 feet, and must have weighed more than 100 pounds before they were broken by their fall, as some of the fragments of them still weighed upwards of 60 pounds. When these large vitrified masses either struck against each other in the air, or fell on the ground, they broke in many pieces, and covered a large space around them with vivid sparks of fire, which communicated their heat to every thing that was combustible.* In an instant the town, and country about it, were on fire in many parts; for in the vineyards there were several straw huts, all of which were burnt. A great magazine of wood in the heart of the town was all in a blaze, and had there been much wind the flames must have spread universally, and all the inhabitants would have infallibly been burnt in their houses; for it was impossible for them to stir out. Some who attempted it with pillows, tables, chairs, the tops of wine casks, &c. on their heads, were either knocked down, or soon driven back to their close quarters under arches, and in the cellars of their houses. Many were wounded; but only 2 persons died of the wounds they received from this dreadful volcanic shower. To add to the horror of the scene, incessant volcanic lightning was whisking about the black cloud that surrounded them, and the sulphureous smell and heat would scarcely allow them to draw their breath.

In this miserable and alarming situation they remained about 25 minutes, when the volcanic storm ceased all at once, and the frightened inhabitants of Ottaiano, apprehending a fresh attack from the turbulent mountain, hastily quitted the country, after having deposited the sick and bed-ridden, at their own desire, in the churches. Had the eruption lasted an hour longer, Ottaiano must have remained exactly in the state of Pompeia, which was buried under the ashes of Vesuvius just 1700 years before, with most of its inhabitants, whose bones are to this day frequently found under arches and in the cellars of the houses of that ancient city. The palace of the Prince of Ottaiano is situated on an eminence above the town, and nearer the mountain, the steps leading up to it, being deeply covered with volcanic matter, resembled the cone of Vesuvius, and the white marble statues on the balustrade made a singular appearance peeping from under the black ashes, which had entirely covered both the balustrade and their pedestals. The roof of the palace was totally destroyed, and the windows were broken; but the house itself, being strongly built, had not suffered much.

They had an opportunity of seeing here exactly the quality of the dreadful

* These masses were formed of the liquid lava, the exterior parts of which had become black and porous by cooling in the long traverse they had made through the air, while the interior parts, less exposed, retained an extreme heat, and were perfectly red.—Orig.

shower, as the volcanic matter, which broke through the roof of the palace, and fell into the garrets, on the balconies, and in the courts, had not been removed. It was composed of the scoriæ of fresh lava much vitrified, great and small, mixed with fragments of ancient solid lavas of different sorts: many pieces were enveloped by the new lava, which formed a crust about them; and others were only slightly varnished by the fresh lava. These kind of stones being very compact, and some weighing 8 or 10 pounds, must have fallen with greater force than the heavier scoriæ, which were very porous, and had the great surface abovementioned. The palace of Ottaiano is built on a thick stratum of ancient lava, which ran from the mountain of Somma when in its active volcanic state. Under this stratum we were shown 3 grottoes, from which issues a constant extreme cold wind, and at times with impetuosity, and a noise like water dashing on rocks. They are shut up with doors like cellars, and are made use of as such, as also to keep provisions fresh, and to cool liquors. He had never seen these ventaroli before.*

Sir Wm. observed that the tract of country, completely covered with a stratum of the volcanic matter abovementioned, was about $2\frac{1}{2}$ miles broad, and as much in length, in which space the vines and fruit trees were totally stripped of their leaves and fruit, and had the appearance of being quite burnt up; but, to his great surprize, having visited that country again 2 days afterwards, he saw those very trees, which were apple, pear, peach, and apricot, in blossom again, and some with the fruit already formed, and of the size of hazel-nuts. The vines there had also put forth fresh leaves, and were in bloom. Many foxes, hares, and other game, were destroyed by the fiery shower in the district of Somma and Ottaiano.

The conical part of Vesuvius was, after the eruption, covered with fragments of lava and scoriæ, which made the ascent much more difficult and troublesome than when it was only covered with minute ashes. The particularity of this last eruption was, that the lava which usually ran out of the flanks of the volcano, forming cascades, rivers, and rivulets of liquid fire, was now chiefly thrown up from its crater in the form of a gigantic fountain of fire, which falling still in some degree of fusion had, in a manner, cased up the conical part of Vesuvius with a stratum of hard scoriæ: on the side next the mountain of Somma, that stratum was more than 100 feet thick, forming a high ridge. The valley between Vesuvius and Somma had received such a prodigious quantity of lava

* At Cesi, in the Roman State, towards the Adriatic, there are many such ventaroli; and the inhabitants of that town, by means of leaden pipes, contain the fresh air from them into the very rooms of their houses, so that by turning a cock they can cool them to any degree. Some, who have refined still more on this luxury, by smaller pipes, bring this cold air under the dining table, so as to cool the bottle of liquor on it.—Orig.

and other volcanic matter during this last eruption, that it was supposed to be raised 250 feet or more. Three such eruptions as the last would completely fill up the valley, and, by uniting Vesuvius and Somma, form them into one mountain, as they most probably were before the great eruption in the reign of Titus. In short, the whole face of Vesuvius was changed. Those curious channels, in which the lava ran in the preceding month of May, were all buried. The volcano appeared to have also increased in height; the form of the crater was changed, a great piece of its rim towards Somma being wanting; and on the side towards the sea it was also broken. There were some very large cracks towards the point of the cone of the volcano, which made it probable, that more of the borders of the crater would fall in. The ridge of fresh volcanic matter on the cone of Vesuvius towards Somma, and the thick stratum in the valley, were likewise full of cracks, from which there issued a constant sulphureous smoke that tinged them and the circumjacent scorix and cinders with a deep yellow, or sometimes a white tint.

The number and size of the stones, or, more properly speaking, of the fragments of lava, which were thrown out of the volcano in the course of this eruption, and which lay scattered thick on the cone of Vesuvius, and at the foot of it, were incredible. The largest they measured was in circumference no less than 108 English feet, and 17 feet high. It was a solid block, and much vitrified: in some parts of it there were large pieces of pure glass, of a brown yellow colour, like that of which common bottles are made, and, throughout, its pores seemed to be filled with perfect vitrifications of the same sort. The spot where it fell, was plainly marked by a deep impression, almost at the foot of the cone of the volcano, and it took 3 bounds before it settled, as was plainly perceived by the marks it has left on the ground, and by the stones which it pounded to atoms under a prodigious weight. Another solid block of ancient lava, 66 feet in circumference, and 19 feet high, being nearly of a spherical shape, was thrown out at the same time, and lay near the former. This stone had the marks of having been rounded, nay almost polished, by continual rolling in torrents, or on the sea-shore; but it had undoubtedly been, in that state, thrown out of the volcano, and may therefore be the subject of curious speculations. Another block of solid lava that was thrown much farther, and which lay in the valley between the cone of Vesuvius and the Hermitage, was 16 feet high, and 92 in circumference, though it appeared, by the large fragments that lay round, and were detached from it by the shock of its fall, that it must have been twice as considerable when in the air. There were thousands of very large fragments of different species of ancient and modern lavas, that lay scattered on the cone of Vesuvius, and in the vallies at its foot; but these 3 were the largest of those they measured. They measured two other stones in the valley between Somma and

Vesuvius; the one was $22\frac{1}{2}$ feet long, $13\frac{1}{2}$ feet broad, and 10 feet high; the other, $11\frac{1}{2}$ feet high, and 72 feet in circumference.

They found also many fragments of those volcanic bombs that were said to have burst in the air; and some entire, having fallen to the ground without bursting. The fresh red-hot and liquid lava having been thrown up with numberless fragments of ancient lavas, the latter were often closely enveloped by the former; and probably when such fragments of lava were porous and full of air bubbles, as is often the case, the extreme outward heat, suddenly rarifying the confined air, caused an explosion. When these fragments were of a more compact lava they did not explode, but were simply inclosed by the fresh lava, and acquired a spherical form by whirling in the air, or rolling down the steep sides of the volcano. The shell or outward coat of the bombs that burst, was always composed of fresh lava, in which many splinters of the more ancient lava that had been inclosed are seen sticking.

The phenomenon of the natural spun-glass, which fell at Ottaiano with the ashes on the 5th of August, was likewise clearly explained here. It has already been mentioned, that the lava thrown up by this eruption was in general more perfectly vitrified than that of any former eruption, which appeared plainly on a nearer examination of the fragments of fresh lava, the pores of which were generally found full of a pure vitrification, and the scorïæ themselves, on a close examination with a magnifying glass, appeared like a confused heap of filaments of a foul vitrification. When a piece of the solid fresh lava had been cracked in its fall without separating entirely, he always saw capillary fibres of perfect glass, reaching from side to side, within the cracks. Sir Wm. explains his idea of this lava by comparing it with a rich Parmesan cheese, which, when broken and gently separated, spins out transparent filaments from the little cells that contained the clammy liquor of which those filaments were composed. The natural spun-glass therefore, that fell at Ottaiano during this eruption, as well as that which fell in the Isle of Bourbon in 1766, must have been formed, most probably, by the operation of such a sort of lava as has been just described, cracking and separating in the air at the time of its emission from the craters of the volcanoes, and by that means spinning out the pure vitrified matter from its pores or cells, the wind at the same time carrying off those filaments of glass as they were produced.

Sir Wm. observed, sticking to some very large fragments of the new lava, which were of a close grain, some pieces of a substance, whose texture very much resembled that of a true pumice stone; and on a close examination, and having separated them from the lava, he perceived that this substance had actually been forced out of the minute pores of the solid stone itself, and was a collection of fine vitreous fibres or filaments, confounded together at the time of their

being pressed out by the contraction of the large fragments of lava in cooling, and which had bent downwards by their own weight. This curious substance has the lightness of a pumice, and resembles it in every respect except being of a darker colour. When the pores of the fresh solid lava were large, and filled with pure vitrified matter, he found that matter sometimes blown into bubbles on its surface, probably by the air which had been forced out at the time the lava contracted itself in cooling: those bubbles, being thin, showed that this volcanic glass has the kind of transparency of our common glass bottles, and is like them of a dirty yellow colour. He detached with a hammer some pieces of this kind of glass, as large as his fist, which adhered to, and was incorporated with, some of the larger fragments of lava, and, though of the same kind, from their thickness they appeared perfectly black, and were opaque.

Another particularity was remarkable in the lava of this eruption: many detached pieces of it were in the shape of a barleycorn or of a plumb-stone, small at each end, and thick in the middle. He picked up several, and saw many more which were too heavy to carry off, weighing more than 60 pounds; some of the smaller ones did not weigh an ounce. He supposed them to be drops from the liquid fountain of fire of the 8th of August, which might very naturally acquire such a form in their fall; but the peasants in the neighbourhood of Vesuvius are well convinced that they are the thunder bolts that fell with the volcanic lightning. He found many of the volcanic bombs, or properly speaking, round balls of fresh lava, large and small; all of which had a nucleus composed of a fragment of more ancient and solid lava. There were also some other curious vitrifications, very different from any he had ever seen before, mixed with the late fallen shower of huge scorixæ and masses of lava.

V. An Appendix to the Paper in the Philos. Trans. for 1778, entitled, "A Method of extending Cardan's Rule for resolving one Case of the Cubic Equation $x^3 - qx = r$ to the other Case of the same Equation, which it is not naturally fitted to solve, and which is therefore called the Irreducible Case." By Francis Maseres, Esq., F. R. S., &c. p. 85.

In the above-mentioned paper in the Philos. Trans. the expression $\sqrt[3]{e} \times$ the infinite series $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} - \&c.$ is shown to be equal to the root of the equation $x^3 - qx = r$, whenever

$\frac{rr}{4}$ is less than $\frac{q^3}{27}$, but greater than $\frac{1}{2}$ of it, or than $\frac{q^3}{54}$. This expression is wholly transcendental, or composed of an infinite number of terms. But Mr. M. thought that it might be convenient on some occasions to divide this expression, if possible, into 2 others, whereof the one should be a mere algebraic expression, or consist of a finite number of terms, and the other should be transcendental,

or involve in it an infinite series. Accordingly, the conclusions obtained in this paper are as follow: If $\frac{1}{4}rr$ is less than $\frac{1}{27}q^3$, but greater than its half, and e be put, as before, $= \frac{1}{2}r$, and $zz = \frac{1}{27}q^3 - \frac{1}{4}rr$, it is shown in the present paper, that the root of the equation $x^3 - qx = r$ will be equal to the mixed expression $\sqrt[3]{(e + z)} + \sqrt[3]{(e - z)} + 4\sqrt[3]{e} \times$ the infinite series $\frac{zz}{9ee} + \frac{154z^6}{6561e^6} + \frac{55913z^{10}}{4782969e^{10}} + \&c.$

As to the 2d branch of the 2d case of the equation $x^3 - qx = r$, or that in which $\frac{1}{4}rr$ is less than half $\frac{1}{27}q^3$, I do not know, says Mr. M. any method of extending Cardan's rule to it. But I have been informed by my learned and ingenious friend Dr. Charles Hutton, professor of mathematics in the Royal Academy at Woolwich, that he has discovered such a method: and I hope he will soon communicate it to this learned Society.

VI. An Account of a Method for the safe Removal of Ships that have been driven on Shore, and damaged in their Bottoms, to Places, however distant, for repairing them. By Mr. Wm. Barnard, Shipbuilder, Grove-street, Deptford. p. 100.

On January the 1st, 1779, in a most dreadful storm, the York East India-man, of 800 tons, homeward bound, with a pepper cargo, parted her cables in Margate Roads, and was driven on shore, within 100 feet of the head, and 30 feet of the side, of Margate Pier, then drawing 22 feet 6 inches water, the flow of a good spring tide being only 14 feet at that place. On the 3d of the same month Mr. B. went down, as a shipbuilder, to assist as much as lay in his power to save the ship. He found her perfectly upright, and her shere, or side appearance, the same as when first built, but sunk to the 12 feet water mark, fore and aft, in a bed of chalk mixed with a stiff blue clay, exactly the shape of her body below that draft of water; and from the rudder being torn from her as she struck coming on shore, and the violent agitation of the sea after being there, her stern was so greatly injured as to admit free access to it, which filled her for 4 days equal to the flow of the tide. Having fully informed himself of her situation and the flow of spring tides, and being clearly of opinion she might be again got off, he recommended, as the first necessary step, the immediate discharge of the cargo; and, in the progress of that business, he found the tide always flowed to the same height on the ship; and when the cargo was half discharged, and he knew the remaining part should not make her draw more than 18 feet water, and while he was observing the water at $22\frac{1}{2}$ feet by the ship's marks, she instantly lifted to 17 feet 8 inches, the water and air being before excluded by her pressure on the clay, and the atmosphere acting on her upper part equal to 600 tons, which is the weight of water displaced at the difference of those two draughts of water.

The moment the ship lifted, he discovered she had received more damage than was at first apprehended, her leaks being such as filled her from 4 to 18 feet water in $1\frac{1}{2}$ hour. As nothing effectual was to be expected from pumping, several scuttles or holes in the ship's side were made, and valves fixed to them, to draw off the water to the lowest ebb of the tide, to facilitate the discharge of the remaining part of the cargo; and, after many attempts, he succeeded in an external application of sheep skins sewed on a sail, and thrust under the bottom, to stop the body of water from rushing so furiously into the ship. This business effected, moderate pumping enabled them to keep the ship to about 6 feet water at low water, and by a vigorous effort they could bring the ship so light as, when the cargo should be all discharged, to be easily removed into deeper water. But as the external application might be disturbed by so doing, or totally removed by the agitation of the ship, it was absolutely necessary to provide some permanent security for the lives of those who were to navigate her to the river Thames. Mr. B. then recommended, as the cheapest, quickest, and most effectual plan, to lay a deck in the hold, as low as the water could be pumped to, framed so solidly and securely, and caulked so tight as to swim the ship independant of her own leaky bottom, which was done in the following manner.

Beams of fir timber, 12 inches square, were placed in the hold under every lower deck beam in the ship, as low as the water would permit; these were in 2 pieces, for the convenience of getting them down, and also for the better fixing them of an exact length, and well bolted together when in their places. Over these were laid long Dantzic deals of $2\frac{1}{2}$ inches thick, well nailed and caulked. Against the ship's side, all fore and aft, was well nailed a piece of fir, 12 inches broad and 6 inches thick on the lower, and 3 inches on the upper edge, to prevent the deck from rising at the side. Over the deck, at every beam, was laid a cross piece of fir timber, 6 inches deep and 12 inches broad, reaching from the pillar of the hold to the ship's side, on which the shores were to be placed to resist the pressure of the water beneath. On each of these, and against the lower deck beam, at equal distance from the side and middle of the ship, was placed an upright shore, 6 inches by 12, the lower end let 2 inches into the cross piece. From the foot of this shore to the ship's side, under the end of every lower deck beam, was placed a diagonal shore, 6 inches by 12, to ease the ship's deck of part of the strain by throwing it on the side. An upright shore, of 3 inches by 12, was placed from the end of every cross piece to the lower deck beams at the side; and one of 3 inches by 12, on the midship end of every cross piece to the lower deck beam, and nailed to the pillars in the hold. Two firm tight bulkheads or partitions were made as near the extremes of the ship as possible. The ceiling, or inside plank, of the ship was very securely caulked up to the lower deck, and the whole formed a complete ship with a flat bottom.

within side to swim the outside leaky one; and that bottom being depressed 6 feet below the external water, resisted the ship's weight above it, equal to 581 tons, and safely conveyed her to the dry dock at Deptford.

After writing the above account, Mr. B. was desired to use the same method on a Swedish ship, stranded near Margate on the same day as the York East India-man, and swim her to London. As this ship was about 250 tons, and the execution of the business something different from what was practised with regard to the large ship, it may not be improper to describe it. As this ship's bottom was so much injured, having lost 8 feet of her stern-post and all her keel, several floor timbers being broken, and some of the planks off her bottom, so as to leave a hole large enough for a man to come through, several lower deck beams being likewise broken, and all the pillars in the hold broken and washed away; he thought it necessary to connect, in some degree, the shattered bottom with the ship's decks, not only to support the temporary deck by which she was to swim up, but to prevent the bottom being crushed by the weight of the ship when she should be put on blocks in the dry dock: to effect which, after he had put across 12 beams of fir, 6 inches by 12, edgeways, one under every lower deck beam of the ship, and well fastened them to the ship's side, he placed 2 upright pieces to each beam of 6 inches by 12, securely bolted to the sides of the keelson, and scored 6 inches under the ship's lower deck beams, and 3 inches about the beams of the temporary deck, and well fastened to each: the deck was then laid with long 2 inch Dantzic deals, and well nailed and caulked; the ship's inside plank being well caulked up to the lower deck. A piece of fir, of 12 inches broad and 2 inches thick on the upper, and 4 inches on the lower edge, was well nailed to the ship's side all fore and aft, and well caulked on both edges, to prevent the side of the deck from leaking, or being forced up by the pressure of the water against the deck, a 2 inch deal or cross piece was laid over every beam, from the ship's side to the uprights at the middle line; then, at equal distance from the side and middle line, pieces of 6 inches square, as long as could be got down, were put all fore and aft on both sides, scored 2 inches over every cross piece, and well bolted through the cross piece and deck, and into the fir beams. From this fore and aft piece or ribband, to the ship's side, and from it to the uprights in the middle, were placed 2 rows of diagonal shores, 6 inches square, the heels of which were securely wedged against the fore and aft piece or ribband, which afforded sufficient support to the temporary deck without any other shores. Two bulkheads or partitions were built, as far as the fore-mast forward, and mizen-mast aft, well planked, shored, and caulked, to resist the water. As decks laid in this manner, and in so much hurry as the time of low water requires, will of consequence leak in some degree, and as that leakage, washing from side to side, will cause the ship to lay along, he fixed a 2 inch

deal, 12 inches broad, edgeways, all fore and aft at the middle line, and well caulked it, to stop half the water on the weather or upper side, when the ship would incline either way, which not only made her stiffer under sail, but facilitated the pumping out the water made by leaks in the deck.

This deck was 63 feet long and 23 feet broad, and was laid at 5 feet 5 inches above the bottom of the keel, or 4 feet above the top of the floor timbers, and swam the ship at 12 feet 5 inches water, resisting 216 tons, and containing under it 124 tons of water, which pressing against the under side of the temporary deck, acted as ballast, and brought her safely into the dry dock at Deptford, from the most dangerous situation possible, being partly within and partly without Margate Pier, where she had been left by some Ramsgate men, who had undertaken to remove her from the place where she was stranded to a safer one within Margate Harbour.

VIII. Account of a Woman who had the Small-pox during Pregnancy, and who seemed to have communicated the same Disease to the Foetus. By John Hunter, Esq., F. R. S. p. 128.

Mr. Grant's Account.—On the 5th of Dec. 1776, Mrs. Ford had been seized with shivering and the other common symptoms of fever, to which were added great difficulty of breathing and a very hard cough. Mr. Grant saw her on the 7th; and he took from her 8 oz. of blood, and gave her a composition of the saline mixture with spermaceti and magnesia every 6 hours.

This had operated by the 8th 2 or 3 times very gently, when most of the complaints were relieved; but the cough still shaking her violently, bleeding seemed necessary to be repeated, more particularly as she considered herself to be in the 6th month of her pregnancy. The medicine was continued without the magnesia.

In the evening, viz. the 8th, the small-pox appeared, which proved of a mild kind, and moderate in quantity. Its progress was rather slower than might have been expected; but the woman passed through the disease in great spirits, sitting up the greatest part of the day during the whole time, and taking only a paregoric at night, and, as occasion required, a little magnesia: thus the symptoms were mitigated, and the cough at last became very little troublesome. On the 25th she complained of a pain in her side: 8 oz. of blood were taken away. The next day she was quite free from pain, and thought herself as well on the 27th as her particular situation would admit of; after which she was not visited by Mr. Grant till the 31st, when she was in labour.

Mr. Wastall's letter on the same subject.—Dec. 30, 1776, I was sent for to Mrs. Ford, a healthy woman, about 22 years of age, who was pregnant with her first child. She had come out of the country about 3 months before. Soon

after her arrival in town she was seized with the small-pox, and had been under the care of Messrs. Hawkins and Grant, who have favoured me with the particulars here annexed. I called on her in the afternoon; she complained of violent griping pains in her bowels, darting down to the pubes. On examining I found the os tincæ a little dilated, with other symptoms of approaching labour. I sent her an anodyne spermaceti emulsion, and desired to be called if her pains increased. I was sent for. The labour advanced very slowly; her pains were long and severe; she was delivered of a dead child, with some difficulty. Observing an eruption all over the body of the child, and several of the pustules filled with matter, I examined them more particularly; and recollecting that Dr. Leake, in his Introductory Lecture to the Practice of Midwifery, had observed, that it might be necessary to inquire, whether those adults who are said totally to escape the small-pox have not been previously affected with it in the womb, I sent a note to Dr. Leake, and likewise to Dr. Hunter, in hopes of ascertaining a fact hitherto much doubted. Dr. Leake came the same evening, and saw the child. Dr. Hunter came afterwards, with Mr. Cruickshanks, and examined it; also Mr. John Hunter and Mr. Falconer; who all concurred with me, that the eruption on the child was the small-pox. Dr. Hunter thought the eruption so like the small-pox that he could hardly doubt; but said, that in all other cases of the same kind, that he had met with, the child in utero had escaped the contagion.

From Mr. Grant's Notes.—The eruption appeared on Mrs. Ford in the evening of the 8th of Dec. and she was delivered the 31st, that is, 23 days after the appearance of the eruptions.

Reflections by Mr. John Hunter.—The singularity of the above case, with all its circumstances, has inclined me to consider it with some attention.

1. There can be no doubt but that the mother had the small-pox, and that the eruption began to appear on the 8th of Dec. also, that it went through its regular stages, and that on the 31st, viz. 23 days after the first appearance of the eruption, the woman was delivered of the child, who is the subject of this paper.

2dly. The distance of time when she had the small-pox before delivery, joined with the stage of the disease in the child when born, which probably was about the 6th or 7th day of the eruption, viz. about 15 or 16 days after the beginning of the eruption on the mother, perfectly agrees with the possibility of the infection being caught from the mother.

3dly. The external appearance of the pustules in the child was perfectly that of the small-pox, as must have appeared from the relation given in Mr. Wastall's letter. Most of the pustules were distinct, but some were blended or united at

their base. The face had the greatest number ; and these were in general the most indistinct. They were somewhat flattened with a dent in the middle.*

So far were the leading circumstances and external appearances in favour of their being the variolous eruption ; but though these leading circumstances and external appearances were incontrovertible, yet they were not an absolute proof of this being the genuine small-pox ; therefore I must be allowed to consider this subject a little further, and see how far all the circumstances correspond, or are similar to the true small pox. In the small-pox we have a previous fever, instead of which, in the present case, we have no information but that of the mother having had the small-pox within such a limited time as may favour the possibility of infection in the womb ; yet we may presume, that the child must have had considerable fever preceding such an eruption, of whatever kind it was.

In the small pox the eruption goes through pretty regular stages in its progress and declension, which circumstances we know nothing of in the present case ; but even this fever, the eruptions, and their progress, are not absolutely proofs that the disorder is the small-pox when it is caught in the common and natural way : and in proof of this assertion it may be observed, that practitioners every now and then are mistaken.

It may be asked, what is the true characteristic of the small-pox ? That by which it differs from all other eruptions that we are acquainted with ? The most certain character of the small-pox, that I know, is the formation of a slough, or a part becoming dead by the variolous inflammation ; a circumstance which hitherto, I believe, has not been taken notice of. This was very evident in the arms of those who were inoculated in the old way, where the wounds were considerable, and were dressed every day ; which mode of treatment kept them from scabbing, by which means this process was easily observed ; but in the present method of inoculation it is hardly observable : the sore being allowed to scab, the slough and scab unite and drop off together. The same indistinctness attends the eruptions on the skin ; and in those patients who die of, or die while in, the disease, where we have an opportunity of examining them while the part is distinct, this slough is very evident. This slough is the cause of the pit after all is cicatrized ; for it is a real loss of substance of the surface of the cutis : and in proportion to this slough is the remaining depression.

The chicken-pox comes the nearest in external appearance to the small-pox ; but it does not commonly produce a slough. As there is generally no loss of substance in this case, there can be no pit. But it sometimes happens, though

* I endeavoured to take some matter upon the point of 2 lancets ; but not having an opportunity of making an experiment myself, I gave them to two gentlemen, who, I imagine, were afraid of inoculating with them.—Orig.

but rarely, that there is a pit in consequence of a chicken pock ; then ulceration has taken place on the surface of the cutis, a common thing in sores. In the present case, besides the leading circumstances mentioned in the case of the mother, corresponding with the appearances on the child, and the external appearances themselves, we have in the fullest sense the third and real or principal character of the small-pox, viz. the slough in every pustule ; from all which, I think, we may conclude, that the child had caught the small-pox in the womb ; or at least a disease, the effects of which were similar to no other known disease.

In opening the bodies of those who had either died of, or died while under the small-pox, I always examined carefully to see whether any internal cavity, such as the œsophagus, trachea, stomach, intestines, pleura, peritoneum, &c. had eruptions on them or not, and never finding such in any of those cavities, I began to suspect, that either the skin itself was the only part of the body susceptible of such a stimulus ; or that the skin was subject to some influence to which the other parts of the body were not subject, and which made it alone susceptible of the variolous stimulus. If from the first cause, I then concluded it must be an original principle in the animal economy. If from the second, I then suspected, that external exposure was the cause ; and I was the more led into this idea, from finding that these eruptions often attack the mouth and throat, two exposed parts ; add to which, that we generally find the eruptions most on the exposed parts of the body, as the face, &c.

With these ideas in my mind, I thought I saw the most favourable opportunity of clearing up this point. I therefore very attentively examined most of the internal cavities of this child ; such as the peritoneum, pleura, trachea, inside of the œsophagus, stomach, intestines, &c. but observed nothing uncommon. I have already observed, that in this child the face and extremities were the fullest, similar to what happens in common ; from all which I may be allowed to draw this conclusion, that the skin is the principal part which is susceptible of the variolous stimulus, and is not affected by any external influence whatever.

The communication of the small-pox to the child in the womb may be supposed to happen in two ways ; one by infection from the mother, as is supposed in the above case ; the other by the mother's having absorbed the small-pox matter from some other person, and the matter being carried to the child from the connection between the two, which we may suppose done with or without first affecting the mother.

Testimonies and opinions are various with respect to these two facts. Boerhaave seems to have been led by his experience to think that such infection was not communicable : for we find that he attended a lady, who having, in the 6th month of her pregnancy, had the confluent small-pox, brought forth at the regular period a child, who showed not the least vestige of his mother's disease.

His commentator, however, Van Swieten, supports a different opinion, in his Comment, vol. 5. He quotes a case from the Phil. Trans., vol. 28, of a woman, who, having just gone through a mild sort of small-pox, was, by means of a strong dose of purging physic thrown into a miscarriage, and brought forth a dead female child, whose whole body was covered with variolous pustules full of ripe matter; but this history is founded only on the relation of a midwife to a clergyman, and therefore not absolutely to be depended on as accurately stated: however, it is more than probable, that there was a case as described; and that there were really eruptions on the skin of the child similar to the small-pox. Van Swieten likewise mentions what Mauriceau relates of himself. This author testifies, that he had often heard his father and mother say, that the latter, when big with him, and very near her time of delivery, had a painful attendance on one of her children, who died of the small-pox on the 7th day of the eruption; and that on the day following the death of this child, Mauriceau came into the world, bringing with him 5 or 6 true pustules of the small-pox.

It does not appear however, from this recital, whether Mauriceau passed through life free from any posterior infection; but admitting that this eruption of Mauriceau's was truly the small-pox, yet I should very much doubt of his having caught it from the child who died of it: as it should seem that the pustules of Mauriceau were of the same date with those of the child who died. Van Swieten appeals to a more recent case, which had been reported to him by persons of great credit, and is recorded in the Phil. Trans. vol. 46, p. 235. "A woman, big with child, having herself long ago had the small-pox, very assiduously nursed a maid servant during the whole process of this disease. At the proper time she brought forth a healthy female child, in whose skin Dr. Watson asserted, that he discovered evident marks of the small-pox, which she must have gone through in the womb; and the same physician pronounced, that this child would be free from future infection. After 4 years her brother was inoculated; and Dr. Watson obtained permission of the parents to try the same experiment on the girl. The operation was performed on both children in the same manner; and the pus used in both cases was taken from the same patient. The event, however, was different; for the boy had the regular eruption, and got well; but the girl's arm did not inflame nor suppurate. On the 10th day from the insertion of the matter, she turned pale suddenly, was languid for 2 days, and afterwards was very well. In the neighbourhood of the incision there appeared a pustule like those pustules that we sometimes observe in persons who, having had the disease, attend patients ill of the small-pox."

In the epistles of T. Bartholinus, cent. 2, p. 682, is the following history. "A poor woman, aged 38 years, pregnant, and now near the time of delivery,

was seized with the symptoms of the small-pox, and had a very numerous eruption. In this state she was delivered of a child, as full of variolous pustules as herself. The child died soon after birth; the mother 3 days afterwards." Van Swieten infers, that the mother and the child were in this case infected at the same time; therefore the child not infected by the mother.

Dr. Mead asserts, that when a woman in the small-pox suffers an abortion, the foetus is generally full of the contagion; but that this does not happen always. This variety, he says, depends on the state of the mother's pustules when the child is born; that is, whether they are or are not in a state of purulence. Whence he has observed it sometimes to happen, that on the 2d day from the birth, or the 3d, or any day before the 8th, the disease caught from the mother shows itself in eruptions on the child. Dr. Mead here relates the history of a lady of quality, of which this is the substance. A lady, in the 7th month of her pregnancy, had the confluent small-pox, and on the 11th day of the disease brought forth a son, having no signs of the disease on his body; and she died on the 14th day. The infant having lived 4 days, was seized with convulsions, and, the small-pox appearing, died. The doctor hence infers, that the suppuration being in some measure completed on the 11th day, the mother's disease was communicated then to the foetus, and made its appearance on the child after 8 days.

If there be no abortion, Dr. Mead pronounces, that the child will ever be free from the disease, unless the birth should happen before the maturation of the pustules. He brings a case to prove, that the foetus in the womb may be infected by the contagion of which the mother does not partake. "A woman, who had long before suffered the small-pox, nursed her husband, under that disease, towards the end of her pregnancy; and was brought to bed at the due time. The child was dead, and covered all over with variolous pustules."

With respect to the case quoted from Mauriceau, it has been proved by Sir George Baker (Med. Trans. vol. 2, p. 275,) that Dr. Mead drew a conclusion from it directly contrary to the author's meaning. The negative opinion appears evidently to be supported by that history. Sir George Baker mentions, in the same paper, the case of 2 pregnant women who were inoculated at Hertford. They both had the small-pox favourably, and afterwards brought forth their children perfectly healthy at the usual time. Both these children, at the age of 3 years, were inoculated with effect. Sir George Baker also mentions a case which fell under the observation of Dr. Clarke of Epsom. "A woman towards the end of her pregnancy had the small-pox, from which she narrowly escaped. Five weeks after the crisis she was delivered of a healthy female child, whose having numerous marks on her skin, was judged by all who saw her to have undergone the same distemper before her birth. However, at the end of 12

months she had the small-pox in a very severe manner. Both the mother and child were lately living at Epsom."

Since we see then that it is very probable, that the small-pox may be caught from the mother when she is infected, it may be asked, why does not this happen oftener? In answer to this we may suppose, that this is not so ready a way as when the child is exposed to catch it after the birth, as we find too that a difference can be produced after birth; viz. inoculation is a much readier way of catching it than what is called the natural way. It may also be said, that many women who are with child, and have the small-pox during pregnancy, do not recover; therefore both mother and child die before the disease can have time to produce eruptions on the child. Finally, in many of those cases, where the mother recovers, there is sometimes produced a miscarriage, which also hinders the infection from taking place in the child. However, many women go through the whole disease, and the child shows no marks of the small-pox.

Thus have I stated facts relative to the present subject, with some of the best authorities on both sides of the question; and shall now leave the reader to form his own judgment.

IX. A Short Extract from a Journal kept by C. P. Thunberg, M. D., during his Voyage to, and Residence in, the Empire of Japan. From the Swedish. p. 143.

Dr. T. was sent out by the directors of the Botanic Gardens at Amsterdam, first to the Cape of Good Hope, and thence to Japan: in order to investigate the natural history of those countries, and to send home seeds and living plants of unknown kinds, for the use of their collections in Holland. At the first of these places he resided 3 years; and during that time had the good fortune to observe and describe many new species both of animals and vegetables. In the year 1775 he sailed from thence for Batavia, and after a short stay there, embarked on board a Dutch ship, called *Stavenisse*, bound for Japan, in company with the *Blyenburg*. On the 21st of June, they passed *Pulo Sapatoo*, the coast of China, and the island *Formosa*. Aug. 13th they made the land of Japan, and the day after were off the harbour of *Nagasacci*, the only one in that empire where foreign ships are allowed to anchor.

They sailed into the harbour of *Nagasacci* with colours flying, and saluted the *Papenburg*, the emperor's and empress's guard, and the town itself. During this time there came on board 2 *Over Banjoses*, several interpreters, and inferior officers, and some people belonging to the Dutch Factory. These *Over Banjoses* may be compared to the Mandarins of China: a place is prepared for them on the ship's deck, and some of them (for they are frequently changed) must be present when any thing is taken out of, or received into her. They inspect

every thing, muster the people, give passports to such as go on shore, and every day report to the governor of Nagasacci the proceedings on board.

The attention and care with which these gentlemen execute the orders issued by the Imperial Court in 1775, is well worthy of relation. The most minute articles which are carried out of a ship undergo a jealous inspection, both when they are put into the boats, and when they are landed from them; and the same caution is used in embarking goods from the shore. Bedding is ripped open, and the very feathers examined; chests are not only emptied of their contents, but the boards of which they are made are searched, lest contraband goods should be concealed in their substance. Pots of sweetmeats and of butter are stirred round with an iron skewer. The cheeses had a more narrow inspection; a large hole was cut into the middle of each, and a knife thrust into the sides of it in every direction: even the eggs were not exempted from suspicion; many of them were broken, lest they should conceal contraband goods within them. The people themselves, from the highest to the lowest, underwent the same suspicious scrutiny, whenever they went from or returned on board the ship. Their backs were first stroked down by the hand of the inspector; their sides, bellies, and thighs, were then in like manner examined; so that it was next to impossible that any thing could be concealed.

Formerly they were less exact in this visitation; the chief of the factory and captain of the vessel were even exempted from it. This privilege they used in its utmost extent: each dressed himself in a great coat, in which were 2 large pockets, or rather sacks, for the reception of contraband goods, and they generally passed backwards and forwards 3 times a day. Abuses of this nature irritated the Japan government so much, that they resolved to make new regulations. For some time they found, that the more dexterity they used in detecting the tricks of the Europeans, the more dexterously they contrived to evade them: at last however, by repeated trials, they have so completely abridged their liberties, that it is now almost, if not absolutely impossible, to smuggle any thing.

The complexions of the Japanese are in general yellowish, though some few, generally women, are almost white. Their narrow eyes and high eye-brows are like those of the Chinese and Tartars. Their noses, though not flat, are shorter and thicker than ours. Their hair is universally black; and such a sameness of fashion reigns through this whole empire, that the head-dress is the same from the emperor to the peasant. The mode of the men's head-dress is singular; the middle part of their heads, from the forehead very far back, is close shaven; the hair remaining round the temples and nape of the neck is turned up, and tied on the top of the head into a kind of brush about as long as a finger; this

brush is again lapped round with white thread, and bent a little backwards. The women preserve all their hair, and, drawing it together on the top of the head, roll it round a loop, and fastening it down with pins, to which ornaments are affixed, draw out the sides till they appear like little wings; behind this a comb is stuck in. Physicians and priests are the only exception to the general fashion; they shave their heads entirely, and are by that means distinguished from the rest of the people.

The fashion of their clothes has also remained the same from the highest antiquity. They consist of one or more loose gowns, tied about the middle with a sash; the women wear them much longer than the men, and dragging on the ground. In summer they are very thin; but in winter quilted with silk or cotton wadding. People of rank have them made of silk; the lower class of cotton stuffs. Women generally wear a greater number of them than men, and have them more ornamented, often with gold or silver flowers woven into the stuff. These gowns are generally left open at the breast; their sleeves are very wide, but partly sewed up in front, so as to make a kind of pocket, into which they can easily put their hands, and in this they generally carry papers, or such like light things.

Men of consequence are distinguished from those of inferior rank by a short jacket of thin black stuff, which is worn over their gowns, and trowsers open on the sides, but sewed together near the bottom, which take in their skirts. Some use drawers, but all have their legs naked. They wear sandals of straw, fastened to their feet by a bow passing over the instep, and a string which passes between the great toe and that next to it, fixing to the bow. In winter they have socks of linen, and in rainy or dirty weather wooden shoes. They never cover their heads but on a journey, when they use a conical cap made of straw; at other times they defend themselves from the sun or the rain by fans or umbrellas. In their sash they fasten the sabre, fan, and tobacco-pipe; the sabre always on the left side, and, contrary to our European custom, with the sharp edge uppermost. Those who are in public employments wear two, the one considerably longer than the other.

Their houses are built with upright posts, crossed and wattled with bamboo, plastered both without and within, and white-washed. They generally have 2 stories; but the uppermost is low, and seldom inhabited. The roofs are covered with pantiles, large and heavy, but neatly made. The floors are elevated 2 feet from the ground, and covered with planks. On these are laid mats which are double, and filled with straw 3 or 4 inches thick. The whole house consists of one large room; but may be divided at pleasure into several smaller, by partitions made with frames of wood, filled up with painted paper, that fix into

grooves made for that purpose in the floor and ceiling. The windows are also frames of wood, divided into squares, filled up with very thin white paper, transparent enough to answer tolerably well the purpose of glass. They have no furniture in their rooms; neither tables, chairs, stools, benches, cupboards, nor even beds. Their custom is to sit down on their heels on the mats, which are always soft and clean. Their victuals are served up to them on a low board, but a few inches from the floor, and one dish only at a time. They have mirrors, but never fix them up in their houses as ornamental furniture; they are made of a compound metal, and used only at their toilets.

Notwithstanding the severity of their winters, which oblige them to warm their houses from November to March, they have neither fire-places nor stoves: instead of these, they use large copper pots standing on legs; these are lined on the inside with loam, on which ashes are laid to some depth, and charcoal lighted on them, which seems to be prepared in some manner which renders its fumes not at all dangerous.

The Portuguese, in all probability, first introduced the use of tobacco into Japan: however be that as it may, they use it now with great frugality, though both sexes, old and young, continually smoke it, blowing out the smoke through their nostrils. The first compliment offered to a stranger in their houses, is a dish of tea and a pipe of tobacco. Their pipes have mouth-pieces and bowls of brass or white copper. The hollow of the bowl is so small as hardly to contain an ordinary pea. The tobacco is cut as fine as a hair about a finger's length, and is rolled up in small balls like pills, to fit the small hollow in the bowl of the pipe; which pills, as they can last but for a few whiffs, must be very frequently renewed.

Fans are used by both sexes equally, and are, within or without doors, their inseparable companions. The whole nation are naturally cleanly; every house, whether public or private, has a bath, of which constant and daily use is made by the whole family. You seldom meet a man who has not his mark imprinted on the sleeves and back of his clothes, in the same colour in which the pattern is printed: white spots are left in manufacturing them, for the purpose of inserting these marks.

Obedience to parents and respect to superiors is the characteristic of this nation. It is pleasing to see the respect with which inferiors treat those of high rank: if they meet them abroad, they stop till they have passed by; if in a house, they keep at a distance, bowing their heads to the ground. Their salutations and conversations between equals abound also with civility and politeness; to this children are early accustomed by the example of their parents. Their penal laws are very severe; but punishments are seldom inflicted. Perhaps there is no country where fewer crimes against society are committed.

Their usage of names differs from that of all other nations. The family name is never made use of but in signing solemn contracts, and the particular name by which individuals are distinguished in conversation, varies according to the age or situation of the person who makes use of it; so that sometimes the same person is, in his life-time, known by 5 or 6 different names. They reckon their age by even years, not regarding whether they were born at the beginning or the end of a year, so that a child is said to be a year old on the new year's day next after his birth, even though he has not been born many days.

Commerce and manufactures flourish here, though, as these people have few wants, they are not carried to the extent which we see in Europe. Agriculture is so well understood, that the whole country, even to the tops of the hills, is cultivated. They trade with no foreigners but the Dutch and Chinese, and in both cases with companies of privileged merchants. The Dutch export copper and raw camphor, for which they give in return sugar, ripe cloves, sappan wood, ivory, tin, lead, tortoise-shell, chintzes, and a few trifles more. As the Dutch company do not pay duty in Japan, either on their exports or imports, they send an annual present to the court, consisting of cloth, chintzes, succotas, cottons, stuffs, and trinkets. Dr. T. attended the ambassador, who was intrusted with these presents, on his journey to Jeddo, the capital of this vast empire, situated at an immense distance from Nagasacci, a journey on which 3 Europeans only are permitted to go, attended by 200 Japanese at least.

They left their little island of Dezima, and the town of Nagasacci, on the 4th of March, 1776, and travelled through Cocora to Simonoseki, where they arrived on the 12th, and found a vessel prepared for them; they embarked on board her, and coasted along to Fiogo. They thence travelled by land to Osacca, one of the principal commercial towns in the empire. At this place they remained the 8th and 9th of April, and on the 10th arrived at Miaco, the residence of the Dairi, or ecclesiastical emperor. Here they also stayed 2 days; but after that made the best of their way to Jeddo, where they arrived on the 1st of May. They were carried by men in a kind of palankins, called norimons, covered, and provided with windows. The presents also and their provisions were carried on men's shoulders, except a few articles, which were loaded on pack-horses. The Japanese officers who attended them provided every thing, so that their journey was by no means troublesome.

On the 18th they had an audience of the Cubo, or temporal emperor, of the heir-apparent, and of the 12 senators; the day following, of the ecclesiastical governors, the governors of the town, and other high officers. On the 23d they had their audience of leave. They left Jeddo on the 26th of May, and arrived at Miaco on the 7th of June. Here they had an audience of the emperor's viceroy, to whom they also made presents, as they were not allowed to

see the Dairi, or ecclesiastical emperor. On the 11th they procured leave to walk about the town, and visit the temples and principal buildings. In the evening they set out for Osacca, which town they were also permitted to view, which they did on the 13th. They saw temples, theatres, and many curious buildings; but, above all, the manufactory of copper, which is melted here, and no where else in the empire. On the 14th they had an audience of the governors of this town; after which they resumed their journey to Fiogo, where they again embarked on the 18th, and proceeded by sea to Simonoseki, whence they arrived on the 23d at Cocota, and thence were carried in norimons to Nagasacci, and arrived at their little island Dezima on the 30th of June, after an absence of 118 days.

X. Of an Extraordinary Appearance in a Mist. By Mr. William Cockin, of Lancaster. p. 157.

January 13, 1768, between 9 and 10 in the morning, being on an eminence that overlooked some low meadow ground, Mr. C. observed, in a direction opposite to that of the sun, which shone very bright, and in a mist which covered the said inclosures, an unusual meteor, which, without attempting to name it, he describes by help of fig. 15, pl. 6. At about the distance of half a mile, and incurvated towards each other, like the lower ends of the common rainbow, there appeared in the mist 2 places of a peculiar brightness as represented at AA. They seemed, as is common, to rest on the ground, were continued as high as the mist, and in breadth near half as much more as that of the iris. In the middle, between these 2 places, on the same horizontal line, was a coloured appearance like dcb, a, bcd, whose base could not at most subtend an angle of above 10 or 12 degrees, and whose interior parts were thus variegated. The centre a was dark and irregularly terminated, as if made by the shadow of some object not larger than an ordinary sheaf of corn. Next this centre was a curved space bb, of a yellowish flame-colour. To this succeeded another curved space of nearly the same dark cast as the centre, seemingly tinged with a faint hue of green, and very evenly bounded on each side, as at cc. After these came on the terminating ring, which was coloured very much in the manner of the common rainbow, except that the tints were not quite so vivid (as if owing to the effect of a yellowish tinge, which seemingly entered into the composition of all the colours) nor their boundaries so well defined. The centre of the image appeared to be exactly in the line of aspect, as it is called, or one conceived to be drawn from the sun through the eye of the spectator; and it may be observed from the figure, that these curve spaces were not segments of perfect circles, but formed like the ends of concentric ellipses, whose transverse axes were perpendicular to the horizon.

To the above description of the image it may be necessary to add the following particulars which attended it. The mist was very thick near the surface of the meadows, though rarer upwards, and chiefly, if not solely, on the side of the hill opposite to the sun. The place where Mr. C. stood was just on its confines; and as he advanced into it, the object became gradually fainter and fainter. As the sun dispersed the vapour, the appearance faded proportionably; and about half an hour after he first saw it, it was scarcely visible. The evening before was wet; but the drops on the hedges were congealed by frost. Where the sun shone the bushes were each invested with a mist, as if owing to the vapours exhaled from them by the sun's warmth; and, on a nearer inspection, he could clearly discern the little humid particles which occasioned it, and which were floating around the bushes at about half an inch distance from each other.

Such were the most material circumstances of this beautiful and remarkable appearance. We have only 2 instances of a like kind mentioned in Dr. Priestley's *History of Light and Colours*. The first is given by M. Bouguer as seen on the Andes; and the other by Dr. Macfai as seen in Scotland. A 3d however may be met with as observed at Pambamarca, in Ulloa's *Voyage to South America*.

With regard to the elliptical form of the curve spaces, as it cannot be accounted for from refraction, it is probably owing to the oblong figure of the observer's shadow, which is very evidently the dark part in the middle, and to which the coloured marginal rings are in some sort obliged to conform. The bright places AA correspond to an appearance once observed by Dr. Smith, and which he very plausibly attributes to a confused mixture of the principal reflected beams that exhibit the ordinary bow. In his *Treatise of Optics* there is an account of Dr. Pemberton's theory for the slender rings of colours, which are sometimes seen within the rainbow, which Dr. Langwith first described in the *Philos. Trans.*, N^o 375, (Abrid. vol. 6, p. 623) and from which some idea may be formed of the cause of the coloured part of the image. It is in substance this: if the drops of rain, &c. which the sun shines on be exceedingly small, from the irregular reflection of all surfaces, and the fits of easy transmission, which the dissipated rays may undergo in their passage through those little globules, there may naturally be formed other coloured arches within the common bow for a number of successions. Hence, with regard to the instance in question, since its rings were so very small in diameter, it appears, that on some account or other the refracted, coloured, and dissipated rays alluded to, have in their return to the eye nearly made the smallest angles possible with the lines of incidence.

XI. On the American Poison called Ticunas. By the Abbé Fontana. Translated from the Italian. p. 163.*

The experiments which I made, says the Abbé Fontana, at Paris during 2 years on the poison of the viper, and which are the sequel of many others on the same subject, published in Italy 10 years before, have enabled me to pronounce with safety on the nature and properties of that poison. The unexpected and important effects which I observed on the application of the poison of that animal to the bodies of living creatures, have led me to new discoveries in animal physics; and these discoveries have gradually led me to doubt of some certain medical theories, either not sufficiently proved, or too generally applied by practitioners.

From that time I have been desirous of extending my researches to other poisons; and, if it had been possible, I could have wished to examine some of the most active vegetable poisons. I had imagined that the animal poisons were like the poison of the viper, which freely diffuses itself through the body of an animal when applied to a wound, but is not increased in the manner in which the poison which produces the small-pox, or the canine madness, is augmented: I say, I conceived, that these poisons might have much analogy to each other, and that they might act in the same manner, and on the same parts of animals. On the other hand, I did not dare to conjecture any thing concerning the operation of vegetable poisons, which I had not yet examined; nor did I think that any thing could safely be advanced concerning their action, even after the instructions derived from the best writers on them. Their manner of experimenting was very different from that which I had used in examining the poison of the viper, and their inferences appeared too vague and uncertain. Being arrived at London however, I had it easily in my power to satisfy my desires on this head. Dr. Heberden, an eminent physician there, and F.R.S., procured me a great number of American arrows which had been carefully preserved, and were well impregnated with poison. He was also so obliging as to supply me with a good quantity of poison, inclosed and sealed up in an earthen vessel inclosed in a tin case. Within the tin case was a paper containing the following words: "Indian poison, brought from the banks of the river of the Amazons, by Don Pedro Maldonado; it is one of the sorts mentioned in the Philos. Trans. vol. 47, N^o 12." In the vol. of the Trans. here quoted, mention is made of 2 poisons little different in their activity; the one called the poison of Lamas, and the other of Ticunas. The poison contained in the earthen vessel which I used is that of the Ticunas. It is not well known to which of the 2 the poison on the arrows be-

* Of the experiments related in this memoir an account is to be found in the English translation of Fontana's Treatise on Poisons, published in 2 vols. 8vo. 1787; but as in that translation the words of the author are not rendered with so much exactness as in the present translation, by one of the secretaries of the R. S., this memoir has, for that reason, been reprinted in this Abridgement.

longed; but my experiments proved it was of the same strength with that of the Ticunas, so that I do not think it at all material to distinguish the one from the other.

Much has been written by authors concerning the activity of these American poisons: so that I thought it proper to make my experiments by degrees, and with all possible precaution. The very smell of them was thought to be noxious, on the bare opening of the vessel; and if the least of their particles was suffered to diffuse itself through the air, some grievous disorder, and even death itself, was apprehended; so, at least, we read in the best authors. I began therefore, as soon as the vessel which contained the poison was open, by making a young pigeon breathe the air of it; for which purpose I held its head within the vessel for a few minutes. On taking it out I found it as well as at first. I loosened with a pen-knife many pieces of the poison, in order to have a little dust in the vessel, and then again immersed the head of the pigeon; but I found that in this 2d experiment also the animal suffered nothing.

From that moment I made no more difficulty of exposing myself to that vapour, and of smelling the effluvia, which seemed disagreeable and nauseous. Many of the particles entered my mouth with the air, and I found that they had a taste something like liquorice. The smell therefore of this poison, when dry, is perfectly innocent; as are also the particles which enter with the air into the mouth and nose, and thence go to the lungs.

But the case in which it seems that this poison is most dreaded, though its application be still external, is, when it is reduced to vapour or smoke by burning on the coals, or when, after boiling a considerable time, it rises in dense fumes. I wished to try it in both these ways, and therefore threw many pieces of the dry poison on burning coals, and caused the pigeon to breathe the fumes, by holding its head in the middle of them; but it never showed any signs of suffering pain. I went still further: I took a glass tube 6 inches long, and 4 wide, and filled it full of this dense and white fume, and then introduced the same pigeon into it; but it showed no more signs of suffering than if it had been held in the fumes of burning sugar. I afterwards boiled a good quantity of it in an earthen vessel. I exposed the pigeon to the vapours both when the poison began to have some consistence, and when, by more boiling, it began to burn to the sides of the vessel, and to be reduced into very dense vapours, and to a coal; but still the animal suffered nothing from these trials. I then made no scruple of freely smelling it and exposing myself to the fumes of it. The odour of the dry poison, when burnt on the coals, is very disgusting, and smells like burnt excrement.

From all these experiments I draw this conclusion, to wit, that the vapours or fumes of the American poison, when smelled or breathed, are innocent. Mr.

de la Condamine was certainly deceived when he wrote that this poison is prepared by women condemned to die; and that it is known to be come to its perfection when the vapours, which it emits in boiling, kill the person who is obliged to be present.

This poison dissolves easily and very well in water, even when it is cold, and so it does also in the mineral and vegetable acids. But it dissolves in oil of vitriol much more slowly than in the other acids, and becomes as black as ink by the operation, which it does not do with any of the other acids. It does not effervesce with either acids or alkalis; neither does it alter milk, nor tinge it, except with its natural colour; nor does it tinge the vegetable juices, either red or green. When examined with the microscope, there is no appearance of regularity or of crystallization; but it is for the most part made up of very small irregular roundish bodies, like vegetable juices. It dries without making any noise, in which it differs from the poison of the viper, and it has an extremely bitter taste when put on the tongue. From all which I deduce, that it is neither an acid nor an alkali, nor composed of salts that are visible even with the microscope.

It was not so much through curiosity as on account of the order which I had prescribed to myself in making my experiments, that I was led to examine if this poison is fatal to life, when applied immediately to the eyes, or if it excites any disease or irritation of the parts. I had before found, that the poison of the viper was as innocent when put on the eyes, as it is in the mouth and in the stomach; whence I was curious to see the analogy between these 2 poisons, both so active, and yet of such different origins.

I began therefore, by putting a small quantity of it, dissolved in water, on the eye of a Guinea-pig. The animal showed no signs of suffering, neither at the time nor afterwards, nor was the eye in the least inflamed. Two hours after, I repeated the experiment on both the eyes of the same animal, and with a greater quantity of the poison; but the pig did not feel the least inconvenience, and the eyes remained in the natural state. I tried the experiments on the eyes of 2 other Guinea-pigs with the same success; which constantly attended all the experiments I afterwards made on the eyes of many other animals, and especially on those of rabbits. I could never observe that it made any alteration in their eyes, any more than if I had bathed them with pure water: whence I think it may be concluded, that the American poison is not in the least hurtful when applied to the eyes, and that it exerts no extraordinary action on them.

But will it be innocent when taken in by the mouth and swallowed? Mr. de la Condamine, and all others who have treated of this poison, believe it to be quite innocent when received by the mouth; and this is the common opinion of all the Americans. The reason they give for it is, that they can eat with impunity the animals killed with this poison, or rather with the poisoned arrows.

This reason is more specious than convincing; since it may be a poison when introduced into the blood, even in the smallest quantity, and yet not be such when taken in by the mouth, except the dose be very considerably increased. The following are the experiments which I have made, the result of which is to render us cautious of pronouncing, even after we have consulted experience itself.

I made a young rabbit swallow 2 grs. of the poison, dissolved in water, and then forced it to drink a tea-spoonful of water, to wash all the poison out of its mouth into its stomach. This animal showed no signs of suffering either then or afterwards. In like manner I made another young rabbit drink 3 grs. of the poison, and it suffered nothing, any more than the former had done. I made another young rabbit drink 4 grs. of the poison, and it likewise suffered nothing. I repeated the same experiments on 3 other young rabbits, to the last of which I gave 6 grs. of the poison, but still without any effect. I then concluded that these experiments were sufficient to assure me, that the American poison is innocent when taken by the mouth, as the poison of the viper is; but I was deceived. I had the curiosity to try it on a young pigeon, to which I gave 6 grs. to drink, and it died in less than 20 minutes. I repeated the experiment on 2 other pigeons, and they both died within the $\frac{1}{2}$ hour.

These last experiments being contradictory to the former, obliged me to try several over again on the rabbits and on the Guinea-pigs. I gave therefore to a small Guinea-pig 5 grs. of the poison to drink, and I found it dead after 25 minutes. I then made a young rabbit drink 8 grs. of the poison: at the end of the half hour it did not seem affected; but in half an hour more it tottered; 4 minutes after it fell down as if it were dead; and in 4 minutes more it was quite dead. I made 2 other young rabbits, and 2 other small Guinea-pigs, drink 9 grs. of the poison: the 2 pigs died in 20 minutes, and 1 of the rabbits died in less than 45 minutes. These experiments induced me to believe, that a greater dose of the poison may prove still more certain death; and that the same quantity of poison produced different effects in the same animals, according to the state their stomachs happened to be in at the time. I had generally observed, in making the experiments, that after swallowing the poison, those animals which had their stomachs pretty full of meat either did not suffer any thing, or else died with much difficulty. I was desirous of making this clearer, by experiments on 3 rabbits and 2 pigeons, which I therefore first kept for a long time without food. Three grs. of poison only killed each of them in less than 35 minutes. I repeated the experiment on 5 other animals with full stomachs, and only 1 of them died.

Hence I deduce this certain fact, that the American poison, when taken in by the mouth, is a poison; but that it requires a pretty large quantity of it to kill even a small animal. The facts above related concerning the American poi-

son, which is noxious when taken in large doses, make me think that the poison of the viper, though it is innocent when taken by the mouth in a small quantity, may yet be mortal when taken in a greater quantity. That torpor which it excites on the tongue, and which continues so long, is enough to convince us, that it is not quite inactive, and that it may really be fatal when taken in a large quantity. I intend to try this experiment on some future occasion, when I propose to give the collected poison of 18 or 20 vipers to a small animal when its stomach is empty, and I dare venture to prophecy, that it will die; for since a very small dose can take away motion and sensation from the tongue, or, in other words, deprive that organ of its principles of life, a greater quantity ought to destroy those of the organs more essential to life itself. If we consider that poison taken in by the mouth must extend itself over a very large surface which is always moist, and mix itself with the food in the stomach, and that the absorbing vessels are extremely small, it will no longer seem strange that it is not noxious when taken in a small quantity, which we have just seen to be the case with the American poison.

I began my experiments on the activity of this poison by wounding different parts of animals with a lancet wetted in the poison dissolved in water. I wounded a small Guinea-pig with it in the thigh 3 times at different intervals. The lancet was full of poison, yet the animal suffered no harm. I made the same trial on 3 other little pigs and a rabbit, but none of them either died or suffered any injury. In all these cases the blood flowed evidently from the wounds: from which I suspected that the poison could not diffuse itself, but that it was driven back, as I had observed in the case of the poison of the viper, which, for this reason, is frequently harmless.

My suspicion was soon confirmed by the following experiments. I soaked a single thread in the poison, and passed it through the skin of a Guinea-pig near one of the nipples, but yet no disorder followed. I then soaked another thread thrice doubled, and let it first dry a little, for fear the poison should remain behind on the skin, in drawing the thread through it. I passed it through the skin of the thighs of a small rabbit near the belly; in 6 minutes the rabbit began to shake and show signs of weakness. In another minute it fell down motionless, appeared convulsed at intervals, and was quite dead in 6 minutes more. I repeated this same experiment, of the soaked double thread, on 2 other rabbits, and on 3 Guinea-pigs; all of which fell down, and were convulsed in 6 or 7 minutes, and died within the half hour.

I had the curiosity to try if the American poison could communicate itself to animals, and kill them, when applied to the skin barely scratched, or scarcely wounded with the point of a lancet. I had observed at Paris, that the poison of the viper communicated a local disorder in such cases, and that it affected and

disordered the skin, but did not prove fatal. The American poison, on the contrary, never produces any local disease, as I observed in making the experiments related above, but leaves the wounded parts as it found them. This constitutes an essential difference between these 2 poisons.

I clipped off the hair, with a pair of scissars, from a part of the thigh of a Guinea-pig, and scratched the skin lightly with a file. There was no visible discharge of blood; but certain small red spots and a moistness appeared on the skin. I bathed the part with a little drop of the poison dissolved in water. In 10 minutes the animal gave signs of convulsions; a little after it fell down motionless, except convulsions, which it had now and then more or less strong, and it died in 20 minutes. The part of the skin where the poison had been applied was not at all altered. This experiment was attended with the same success on 2 other Guinea-pigs, and on a small rabbit; all the 3 dying in less than 27 minutes with very evident signs of convulsions. I wished to try if the larger animals could resist this poison, when only applied to the scratched skin; and therefore, with the point of a lancet, I wounded, very slightly, in many places, the skin of a large rabbit, having first cleared the part of the hair, and then I bathed the wounded places with several drops of the poison. After 15 minutes the rabbit became less brisk than before, and shook its head now and then, as if it could not hold it up without difficulty; but in 20 minutes more it became as lively as at first. I repeated the experiment on another somewhat smaller rabbit: in 10 minutes it gave the same kind of shakes with its head, and could hardly go or support itself on its feet; but after 20 minutes it was as lively as ever.

I shaved off about an inch of the skin of a pretty large rabbit; a little blood appeared though the flesh did not seem to be cut; I put about 3 drops of the poison on the place: in 6 minutes the rabbit seemed very faint; after another minute it fell down as if dead; it scarcely breathed, and was at times convulsed; but in less than 46 minutes it recovered the use of its feet, and a little after began to eat, and remained without any more signs of being disordered. I scratched the skin on the thigh of a hen, and applied the poison to it; but it continued well, though I repeated the experiments twice on other parts of the skin. I slightly scarified the skin of a pigeon's thigh, and applied to it the poison dissolved in water. After 25 minutes it was so weak that it could not stand, and was convulsed at intervals. It fell down a little after, as if it were dead, and remained in that state of apparent death above 3 hours. By degrees however it began to recover, and in $\frac{1}{2}$ an hour more it was quite well.

This experiment on pigeons was repeated 5 times. Three of them died in less than 20 minutes; and the other 2 were seized with convulsions, but afterwards they recovered. From other experiments made afterwards, both on birds

and beasts, it may be concluded, that the American poison applied to the skin slightly scratched may be fatal, though it is not so always, nor in all circumstances. The larger animals more easily resist the action of the poison; and when the more feeble animals did not die, in a little time they were as well as ever.

I was desirous to know what quantity of the poison was necessary to kill an animal. I had made a like inquiry concerning the poison of the viper, and had determined the quantity of that poison requisite to kill the different animals. I might indeed have safely concluded, that a very small quantity of the American poison is sufficient to kill a small animal, since 1 or 2 small drops applied to the skin just scratched, had proved fatal to more than one; but I wished for something more positive. I steeped a very small bit of cotton in about one-fiftieth of a little drop of poison dissolved, which could scarcely be the 50th part of the whole drop. This I introduced into a muscle of the thigh of a pigeon, but the animal was not affected by it. Two hours after, I put into another muscle an atom of the dry poison, scarcely perceptible to the eyes: this likewise did not affect the pigeon. I repeated the experiment with the dry poison on 3 other pigeons; but none of them died or was sick, though in one of the cases the bit of dry poison was very perceptible. I made the same experiment on 3 Guinea-pigs, and on 2 small rabbits, still with the same success, none of them being at all affected. It must be observed, that the poison was not dissolved by the humours of the wound, and I found the bits of it quite entire.

I put to the muscle of another pigeon a bit of cotton much larger than before, impregnated with about 8 times as much poison as in the former case: in 6 minutes time the pigeon fell down, and died soon after. To the muscles of 2 Guinea-pigs I applied bits of cotton steeped in much the same quantity of poison as above: one of them died in 12 minutes, and the other fell down, as if dead, in 6 minutes, but it recovered a little time after. From these experiments I conclude, that above $\frac{1}{1000}$ part of a grain is requisite to kill a small animal; and that it is necessary that the poison be dissolved, for it to prove fatal, or to cause any alteration in the animal economy.

I have made various experiments to determine whether the American poison be fatal or hurtful when applied to the wounded combs of poultry, or to the ears of quadrupeds slightly wounded. The poison of the viper is not commonly fatal in those parts, nor is there any visible disorder in the poisoned comb, though there is in the wattles, which swell horribly, so as sometimes to kill the animal. I wounded the comb of a fowl in many parts, and twice applied to it the American poison by means of cotton well soaked in it; but without being able to produce any disorder. But the experiment succeeded better when tried on the ears. After having made many fruitless trials to communicate the poison by

scratching or wounding the ears of many rabbits, all which showed no signs of injury: I at last succeeded in killing 2 in less than 30 minutes by the application of a great quantity of the poison to the more fleshy parts of the ears which I had wounded in many places with the point of the lancet. The experiments on the ears have evinced to me, that where there are few blood vessels, either no disorder is produced, or else it is not mortal. In this respect the American poison has much analogy to that of the viper. As the poison of the viper is quite innocent when applied to the tendons and ligaments, especially if they are without blood vessels, so the American poison is equally innocent when applied to the same parts. It would be superfluous to relate the sequel of these experiments.

I was desirous of knowing whether the American poison were more surely fatal when introduced into the muscles than when applied to the skin, though drawn through the latter from side to side. A large Guinea-pig, which 2 days before had twice undergone the operation of the skin cut, without suffering any disorder, and a 3d time with but little signs of being affected, died in less than 12 minutes after I had applied the poison to the wounded fibres of a muscle of its thigh. It fell down motionless after the first 3 minutes. I repeated the experiment 10 times on Guinea-pigs, pigeons, and middle-sized rabbits, and all the animals died; so that there can be no doubt but that poisoned wounds in the muscles are more fatal than those in the skin, or in the combs of fowls. The more certain method however of succeeding is, to soak well a piece of porous wood, cut very sharp, in the poison, and so introduce it into the substance of the muscle laid bare for that purpose. But even this method failed 3 times that I tried it on the combs of fowls: nor did I ever observe any appearance of disorder, though the wood was well soaked, and though I left it for several hours in the combs.

On this occasion I made use of the arrows; many of which I employed in perforating the skin of animals, and many others in piercing the muscles. All the animals, especially the larger rabbits, which were wounded in the skin, did not die, though the greater part of them did; but none of those recovered which were pierced in the muscles. In general, I found that the arrows are more dangerous, and oftener fatal, than the poison dissolved in water, and then simply applied to the wounded parts. I found the poison on the arrows more active after steeping them in warm water, as they then operated both more speedily and more surely; and their activity is still more increased by soaking them in the poison, boiled in water to the consistence of julep. Various large animals, such as rabbits, have fallen down motionless in this manner in less than 2 minutes; and some of the smaller sort have been visibly affected in less than 1 minute. I introduced 1 of the arrows, that had been well soaked in the boiled poison, into the comb of a fowl, and left it there a whole day, without any

appearance of injury to the animal. The next day I perforated the comb and the wattles with 2 arrows prepared as before, and left them there for 10 hours; but still without any effect. I then perforated one of the muscles of the thigh with an arrow, and the animal died in 42 minutes.

I had proposed to myself among other things, to examine what alteration this poison may undergo by uniting it with acids and with alkalis. This I had tried with the poison of the viper, the noxious qualities of which neither the most powerful mineral acids, nor the most active alkalis, could take away. For this purpose then I dissolved the poison in 3 mineral acids, as also in distilled vinegar, and in rum; and about an hour after I made the following experiments. I made some small gashes in the skin of a small Guinea-pig, and covered it over several times with the poison dissolved in the nitrous acid: the animal appeared to suffer nothing but the mechanical inconvenience of the wound and the acid; within an hour after it was as lively as ever. Two hours after I repeated the experiment on another part of the skin prepared as above, which I covered with the poison dissolved in rum; the animal died in less than 4 minutes. I slightly wounded the skin of a young rabbit, and applied to it many drops of the poison dissolved in oil of vitriol: it seemed to suffer nothing, and was as lively as before. Four hours after I prepared another part of the skin as above, and applied to it a few drops of the poison dissolved in distilled vinegar: the animal fell down after 4 minutes, and was quite dead in 6. I prepared likewise the skin of another small rabbit, and covered it with the poison dissolved in the marine acid; but the animal was not affected. Six hours after I applied the poison, dissolved in rum, to another part of the skin, and in 45 minutes it fell, and was convulsed; but it recovered in less than 1 hour.

From these first experiments it should seem, that mineral acids render the poison innocent; and that, on the contrary, vinegar and rum make no alteration in it. I continued my experiments with the poison dissolved in vinegar and in rum, but the results were somewhat various. Of 6 animals, to which was given the poison dissolved in vinegar, only 2 died; 2 others were very sick, and the remaining 2 were not at all affected. Of 6 others, treated in like manner, with the poison dissolved in rum, 5 died; and the 6th was very sick. Hence it appears, that the poison dissolved in these 2 fluids preserves its noxious quality.

On the other hand, I repeated the experiments with the poison dissolved in mineral acids on 6 animals; and not 1 of them was in the least affected. I began to suspect, that the poison might fail in its effect, not because it had lost its noxious quality, but because it could not insinuate itself into the wounded parts, on account of the too great action of the mineral acids on the skin and vessels, which they shrivel and burn up. To clear up this doubt, I evaporated by heat the poison dissolved in the mineral acids, and, when it was quite dry, applied

it many times to several animals in various parts of their skin; but none of them were hurt by it. It appears therefore, that the mineral acid destroys the noxious quality of the American poison: I only say, it appears, because it may perhaps be suspected, that, as there remained a little of the acid mixed with the poison after evaporation, it was this that produced the usual alteration in the vessels of the skin. I ought to have made some other experiments with it washed several times in water, and rendered quite insipid; but at that time I was in want of animals to ascertain the truth of this suspicion, and I have not had time since to resume the subject. With respect to the alkaline salts, I may say that I have not discovered that they alter the poison, or render it in the least less noxious. It is true indeed that I have not so often repeated the experiments, nor so much varied them, as I ought to, and as I should have done, if I had not found a great difficulty in procuring animals, and if I had not had in view other experiments much more important.

It was natural for me to think, that as the acids hinder the action of that poison on animals, they might also be a remedy against that poison. I prepared therefore, in the usual way, the skin of a Guinea-pig, and covered it with the poison; and about 40 seconds after I washed it with the nitrous acid, and afterwards with pure water: the animal suffered nothing. Two hours after I introduced the poison into a muscle, and immediately applied the nitrous acid to the part; but the animal fell in a moment convulsed, and was quite dead in 2 minutes. I repeated this experiment on the muscles of another Guinea-pig, and as soon as I had applied the poison, washed it with the nitrous acid, and a little after with water. It fell convulsed in 2 minutes, and was quite dead in 4.

I poisoned, in like manner, the muscles of 4 pigeons, and the moment after I washed them with the nitrous acid: they died in 1 minute. Suspecting that it might be owing to the nitrous acid, rather than to the poison, I next made use of nitrous acid, much diluted, on 4 other pigeons; but they all 4 died, though much more slowly. Being desirous of knowing whether the simple application of the nitrous acid to the muscles could kill pigeons and small Guinea-pigs, I made the trial on 2 of each sort: the pigeons both died soon after; but neither of the pigs, though one of them was very sick.

It appears, therefore, that acids are not only useless as a remedy; but sometimes dangerous, when applied to the poisoned muscles of animals. I shall not say any thing of any other remedy which I have tried, because I have found by experience that they are all useless, whether they are applied soon or late, or whether externally or internally. When the poison is introduced deeply, and has already insinuated itself into the humours, every remedy comes too late and is useless.

There yet remained to be made a further very nice inquiry, and which might in some cases too have turned out a very useful one. My experiments on the

poison of the viper led me to make an observation of the same sort on the American poison. I had determined from them the time which the poison of the viper employs in diffusing itself into the body of the animals; and when it might be useful to cut away the poisoned part, or to make a ligature about it, to hinder the poison from communicating itself to the animal by the blood. I introduced into the muscle of a pigeon's leg an American arrow steeped in warm water, and left it there; and 4 minutes after I made a ligature moderately tight, immediately above the wound. In 26 hours the animal seemed to suffer no inconvenience but from the ligature. I then withdrew the arrow, and loosed the ligature. The part was a little swelled and livid; but the animal did not die of it, though it could make no use of its leg for many days, and afterwards used it with some difficulty. I pierced the muscles of another pigeon with a fresh arrow, as above, and applied the ligature 6 minutes afterwards, leaving the arrow in the wound. In 4 minutes the pigeon had not strength to hold up its head, or scarcely to stand; presently after it fell down to all appearance dead, and was quite dead in 6 minutes more. I repeated the experiment on another pigeon, leaving the arrow in the muscles again, and 8 minutes after I tied the bandage about the leg. Three minutes after it was visibly sick, but recovered again a little afterwards. It was still living 26 hours after, though the muscles were livid. I then loosed the bandage, and it died in 2 hours. I subjected a 4th pigeon to the same experiments, leaving the arrow in the muscles, and applying the bandage 5 minutes after: it died in 2 hours.

I performed the same experiments on 4 other pigeons, to all of which I applied the bandage in 2 minutes. Ten hours after they were all living; I then took off the bandage, after which 3 of them died, and the 4th recovered perfectly. I again repeated the same experiments in like manner on other 4 pigeons, but left the bandage on 30 hours: only one of them died, and that was not till 2 days after, and certainly from the effect of the bandage being too tight, which had produced a gangrene in the muscles. These same experiments I have also repeated on much younger pigeons, whose legs may be cut off below their thighs without their dying. None of those died whose legs were cut off 2 minutes after they were poisoned; and only 2 out of 10 died of those whose legs were cut off after 3 minutes. Fewer pigeons died by this method than by the bandage, when they were both applied after equal times. The reason of this is, that the amputation is not attended with death or any remarkable disorder in the animals; whereas the bandage often produces gangrene in the parts wounded by the arrows, and the pigeons frequently die of the gangrene. I made also the same experiments, on some small Guinea-pigs, and on many young rabbits, sometimes cutting off the part, and sometimes applying the bandage. The results were in general similar to those observed in the pigeons, though not quite so regular or certain.

In general I found, that it required a certain time for the American poison to communicate itself to animals; that this time is much greater than that which the poison of the viper requires to communicate itself; and that the effects of the American poison on animals are more vague and more various; and that, finally, animals may be cured of the effects of both these poisons by cutting away the part in time, when it can be done without endangering the life of the animal by the operation itself.

In the course of my experiments on the poison of the viper, I found that it is not poisonous to all animals; and that there are some cold-blooded animals to whom it is quite innocent. I was curious to know if the case was the same with the American poison. All the writers on the American poison tell us, that it is poisonous to all animals; but assertions may be very different from facts. Many experiments are necessary to evince this general conclusion, and it does not appear that enough have been made to warrant it. I began by impregnating the muscles of frogs with the poison, and they died in a little time. I then had recourse to eels, into which I introduced the arrows, towards the lower or tail-parts of them; and they all died, though very slowly.

I had found that the poison of the viper is quite innocent to the viper itself, and to those serpents which in Tuscany are called *binchi*, and by the French, *couleuvres*. Of these last I could procure no more than 2; for which reason I could make but few experiments on them; but those I have made I think quite decisive. I wounded one of them towards the tail with an arrow well covered with poison of the thickness of syrup, and left the arrow in the muscles. I had previously made an incision in the place where I introduced the arrow, that the dissolved poison on the arrow might easily enter the muscles with the arrow. I also applied some more poison to the same part by means of small incisions in the muscles. The serpent did not seem affected by the poison; but for many hours was as well as ever. I locked it up in a box, which having opened 6 hours after, I found that the serpent was gone, nor could I ever find it again. I repeated the experiment many times, at different intervals, on another rather smaller. The last time I introduced 2 poisoned arrows into the muscles of the tail, and left them there for 24 hours. I frequently applied the poison thickened to a syrup to the wounds, and introduced a great quantity into them with a tooth-pick, yet, so far from dying, the animal was not sensibly hurt. I have often made this same experiment on vipers, without any one of them dying by the poison, though some were wounded in the muscles towards the tail with many arrows well impregnated with poison thickened to a syrup. I have left the arrows for 20 or 30 hours together in the muscles, and yet none of them have died. It is true indeed, that some few, after being operated on, appeared less lively than before; and it seemed that the wounded parts, or the lower half of the body, had sensibly lost some of its natural motion, and this

torpor continued in some for many hours; but then again others continued as lively as before.

After all these experiments it may be safely asserted, that the American poison is entirely innoxious to cold-blooded animals, as is the poison of the viper; in which respect these 2 poisons have a great analogy, though the one be no more than an animal gum, as I have shown elsewhere, and the other a mere vegetable juice.

It remained to examine the action of this poison on living animals; and also which are the particular parts of animals that are affected by it when it proves fatal. Every thing tended to make us think, that it excites one of those disorders which modern physicians call nervous. The symptoms are precisely and decisively the symptoms of those diseases. Convulsions, faintness, a total loss of strength and motion, a diminution or entire want of sensation, are the ordinary symptoms of the poison, in animals. It has often been observed, that very lively animals become in a moment senseless and motionless, and seem at the point of death. I have generally observed a symptom which seems to be a real demonstration that the disorder produced by this poison is purely nervous. If the animals do not die in a few minutes, they perfectly recover again, though they have been thrown into a state of lethargy often for hours, and have not given any certain and evident signs of life. Now this is the very case with the disorders which are called nervous. They frequently come on at once; they sometimes excite convulsive motions, and sometimes they deprive the patient of all strength; but as soon as the effects of the disorder cease, the patient becomes perfectly well, and is hardly sensible that he has been ill. Notwithstanding all this, these signs could not impose on me after my experiments on the poison of the viper: for the disorder produced by that poison has also some symptoms of the nervous disorders, and it appears that the nerves are chiefly affected, though experiments have determined the contrary; therefore we ought here also to have recourse to experiments, and not suffer ourselves to be seduced by an unfounded theory, and by specious reasonings.

To proceed methodically in this important question, I thought it would be proper to begin by examining whether the American poison produces any sensible alteration in the blood drawn from the veins of animals, when it is mixed with it. I cut off the head of a pigeon, and received its warm blood into 2 warm conical glasses, to the amount of about 80 drops into each. Into the 1 glass I put 4 drops of water, and into the other 4 drops of the poison, dissolved in water as usual; the quantity of poison in these 4 drops scarcely amounting to 1 gr. in weight when dry. I stirred round equally the contents of the 2 glasses for a few seconds, in order to mix the materials well together. In 2 minutes the blood which was mixed with the pure water was coagulated; but the other blood which was united with the American poison never coagulated, but became darker

and blacker than the former, which remained red as usual. Three hours after it was still as fluid as at first, while in the other glass the serum appeared to be already separated from the red part. I examined the blood of both glasses with a microscope, both at that time and afterwards, and found that the red globules still preserved their original figure, and that there was no difference between the 2 in this respect.

I repeated this experiment many times with the same success; so that it is evident, that the American poison does not sensibly alter the red globules of the blood in the circumstance abovementioned. It is however worthy of observation, that this poison is so far from coagulating the blood, that it absolutely prevents the coagulation which happens in the blood after it is drawn from animals; yet it cannot be said to attenuate or dissolve the blood, since nothing of that kind is observed when it is examined with a microscope, the red part remaining figured as in its natural state, and nothing more subtle or more thin being observable in that fluid.

A circumstance perfectly similar I also observed to happen with the poison of the viper; so that the effects or alterations caused by these 2 poisons in the blood drawn from the vessels, appear to be perfectly similar, both of them hindering the blood from coagulating, yet neither of them dissolving or altering the globules of it: the only difference between them is, that the poison of the viper tinges the blood much blacker than the American poison does. The poison of the viper does not alter the globules of the blood even when it is given to the living animal, and that the animal in consequence dies. I have observed the same thing with respect to those animals which are killed by the American poison; so that the 2 poisons agree in a wonderful manner in all these cases. But as the poison of the viper produces in general a sensible alteration in the mass of the blood of those animals that are bitten by it; I thought that the same attention ought to be paid to the examination of the blood of those animals which have died of the American poison.

I have observed in general, that the muscles of animals killed by the American poison were paler than before; the blood vessels near the heart appeared more turgid than usual; the blood a little darker coloured than ordinary, though not much, nor coagulated; the viscera of the abdomen not sensibly affected; the heart and the auricles in the natural state, though the heart had sometimes its external vessels more visible, and they appeared as if they were injected. But I have observed a great alteration in one of the viscera, the most essential to life, to wit, the lungs, which always appeared greatly affected. I have generally found them spotted more or less; often with large and livid spots, and sometimes they seemed to be quite putrified. This effect on a viscus so essential to life deserves the greatest attention: it appeared to me, that it was the greater the longer the animal had lived after being poisoned. I have observed the lungs of

some animals to be here and there transparent, especially towards the edges. The pulmonary air was very visible through the external membrane: I have examined it with a microscope, and have been able to distinguish very well the small pulmonary bladders, streaked with vessels for the most part without blood. Great as this alteration is in so important a viscus, I could not persuade myself, that it could alone produce so great and instantaneous a disorder, and that the whole force of the poison exerted itself only on the blood and lungs. There was indeed the instance of the poison of the viper, which produces something similar; but then this poison produces a kind of general coagulum in the blood itself, which certainly is not observable from American poison.

In an inquiry so important, and at the same time so obscure, I thought I ought to have recourse to experiment itself, and to examine the effects of the American poison when introduced immediately into the blood. For this purpose I made use of the same method which I had before employed for introducing the poison of the viper into the blood of the jugular. I used a turn-cock of glass, bent at the point, instead of a small syringe. With this cock I took up the American poison dissolved in water, and then opening the jugular vein thrust the point into it. As the method of making this sort of experiments has been before described in my treatise on the poison of the viper, I think it unnecessary to give a particular description of it here. The experiment is so conducted, that the poison enters the blood by the jugular without touching any cut part of the vessels, not even those of the jugular itself.

For the first experiment I put into the glass syringe 4 drops of the poison dissolved in water; the quantity of poison in the 4 drops could hardly amount to $\frac{1}{2}$ a grain. I introduced the crooked end of the syringe into the jugular of a large rabbit; but in the act of pushing in the piston, which was not close enough fitted to the bore of the syringe, the poison returned backward, which made me say to the persons who were present, that the experiment had failed; but I was surprized when they answered me, that the animal was already dead. I believe there were not 10 seconds between the moment in which I saw the poison turned back, and that of my being told that the animal was dead, as in fact it was. I cannot say what quantity of poison was introduced into the blood; there must have entered a sufficient quantity, as the animal was killed; but had it not been for that circumstance, I should have judged, from the quantity returned back, that none at all had entered the jugular. The animal was so dead, that there appeared no signs of respiration, and the whole body was more pendent and flaccid in all its parts than is usual with animals that have been long dead. The death of this animal followed so close on the introduction of the poison, that the interval of time seemed quite insensible: it appeared to take place much quicker than in the case of the poison of the viper, introduced into the blood in the same manner.

Having repaired my syringe, I put into it only 2 drops of the water in which I had mixed $\frac{1}{4}$ of a drop of the poison dissolved in water as above. I had scarcely introduced the poison into the jugular, when the rabbit fell down as dead as if it had been struck with lightning. I do not believe that half a drop of the liquor in the syringe was introduced when the animal fell motionless and dead. In general, from other experiments which I made afterwards, I think I may venture to say, that this poison, introduced immediately into the blood by the jugular, kills sooner, and requires a less quantity of it to kill, than the poison of the viper does. Death follows the introduction of the poison into the blood so speedily, that it prevents the usual convulsions of the animal. If a smaller quantity of the poison be taken, we then perceive the ordinary convulsions and palpitations, and death does not happen so suddenly. It is indeed true, that the blood is not coagulated, nor so much altered in its colour, as when the poison of the viper is introduced into the jugular; but death is not therefore more tardy, nor less certain, as both the poisons, when introduced immediately into the blood, kill animals in the same manner.

This then is a truth gained from experiment, to which nothing can be objected, however dark or little understood the cause of death may be in such cases: viz. that the American poison, introduced into the blood, kills the animal instantaneously; whence also it indubitably appears, that when it is externally applied to a wounded part of a living animal, it can and ought to communicate, by means of the blood, a great disorder into the animal economy, and occasion death itself. The death of the animal, which follows the instant that the poison is introduced into the blood by the jugular, proves to a demonstration, that in such cases all the action of the poison is against the blood itself, and that the nervous system is not at all affected or altered. This however is still no proof that the nerves may not be more or less affected by that poison, when death happens much slower, and when it is applied externally to wounded parts. In this case we perceive the convulsions, and all the signs of a nervous disorder. The nerves may therefore probably be affected by the poison, and be the chief cause of the death of the animal.

We must here, however, still have recourse to direct experiment, as was done with respect to the poison of the viper, and see what disorders and diseases are produced by the American poison when applied immediately to the nerves without touching the vessels. My experiments have been made on the sciatic nerves of large rabbits, which I prepared in the same manner as I had done at Paris, when I was making experiments with the poison of the viper, so that I shall not here give any particulars concerning the method of preparing these nerves. I shall notice however a small number of the principal experiments made on the nerves, by which may be seen the variety of successes I met with, especially in the first trials, which would have deceived me if I had not persisted

in multiplying my experiments, and varied them in proportion as I found less agreement in the results. To this perseverance, or obstinacy as I may call it, I chiefly owe the new discoveries which, I believe, I have made, concerning the 2 poisons of the viper and the Ticunas.

Having laid bare the sciatic nerve of a rabbit, I passed under it a fine rag several times doubled, and put on the nerve a little cotton well soaked in the American poison, thickened to a syrup. I covered the nerve with the same rag, that the poison might not run over the opened muscles of the animal, and sewed up the skin as usual. After 10 minutes the rabbit began to have convulsions, and to totter; it then fell, with all the signs accompanying the effects of the poison, and died soon after. I repeated the same experiment on another rabbit, and took care to wrap up the poisoned nerve with rags still better than before. This 2d rabbit showed no signs of being affected for 10 hours, during which I observed it; but looking at it after 2 hours more, I found it had been dead a little while, as it was still warm.

I suspected that the poison applied to the nerve, which was considerable in quantity, might at length have penetrated through the rags, and, uniting with the humours of the parts cut, have extended its action to the muscles, and the adjacent parts. I was under the necessity therefore, of either diminishing the quantity of poison, or increasing the rags, to prevent any diffusion of the poison through them: I adopted the latter as the more secure way. I detached the sciatic nerve of a rabbit as usual, and introduced below it a very fine rag, often doubled. I put the bit of cotton, well soaked in the poison, on the nerve, and covered every thing well with the lappets of the rag. This rabbit lived 24 hours, and only showed signs of being ill in the last hour, nor was there any reason to think that it died of the effects of the poison.

I prepared the sciatic nerve of another rabbit in the same manner, covering it with the poison and rags as before. This rabbit died 40 hours after, without any symptoms of being poisoned. I made the same experiment on the sciatic nerve of 3 other rabbits, having taken all possible care that the poisoned nerve should be well covered with rags, that there might not be any reason to suspect the poison might penetrate through them. One of them died 3 days after, but the other 2 were still living at the end of 8 days. I also prepared the nerves of 2 other rabbits exactly as above, but without the poison, to serve as a kind of comparative experiment to the former. One of these rabbits died in 36 hours, and the other was still living 8 days after.

These experiments seemed sufficient ground to determine, whether the American poison, when applied externally to the nerves, is capable of producing any malady or disorder in animals. But it remained to be tried, whether it be equally inactive when applied to wounded nerves, as also to the pulp of the

nerves. I prepared therefore the sciatic nerve of a rabbit as above, and I pierced it several times through with a lancet, before I applied the poison to it, and then put the poison exactly on the wounded part of the nerve. The rabbit lived 5 days, and then died without any apparent illness. I repeated the experiment on another rabbit with the same circumstances, and it was still living 8 days after.

I varied the experiment a little on the nerves of 3 other rabbits. Instead of wounding the nerve in many places, I made an incision, above $\frac{5}{12}$ of an inch long, into it, and introduced into the slit some threads, well soaked in the poison, and covered the whole up very well. One of these died in 60 hours, but seemingly not from the poison; the other 2 were living 8 days after. I thought it necessary to vary also this 2d kind of experiment, and to cut the nerve as I had done in examining the poison of the viper. In consequence I cut the sciatic nerve as far as I could from the top, to be able easily to wrap the rags about it. The detached part of the nerve in the largest rabbits might be about an inch and a half. Having placed the nerve on the rags I covered it well with poison in the part where it was cut, and covered the whole up with the rags as usual. I performed this experiment on 6 rabbits. Two of them died in 40 hours; 2 others in 3 days; but the remaining 2 were living 4 days after. For a comparative experiment I prepared, as above, the nerves of 2 other rabbits, which I cut, but did not apply the poison to: one of them died in 36 hours, but the other was living the 3d day after.

The uniformity of the results of these experiments on the nerves induced me to think it quite superfluous to repeat them; and I am persuaded they will leave no doubt with any one who is accustomed to experiments, and not prejudiced in favour of ill-grounded hypotheses. Hence it follows, that the American poison is not poisonous when applied to the nerves; and that in such cases it produces no sensible disorder in the economy of living animals: this is what the experiment directly establishes. But to suppose what has not been observed; to believe what is contradicted by experiments, is dreaming in philosophy, running after error instead of truth, and adopting mere fancies for facts. The American poison, similar in this respect to the poison of the viper, is not poisonous, but quite innoxious, applied in any manner whatever, to the nerves. But it kills in a moment, and with the smallest quantity, when introduced immediately into the blood by the jugular, as does likewise the poison of the viper. Its action is therefore all upon the blood, and not in the least on the nerves, whatever may be the principle or the mechanism by which death is produced. The effects and alterations caused in the blood by the poison of the viper are more determinate and more evident. Here a coagulation undeniably happens, which is not observable in the blood of animals killed by the American poison; but the latter produces a great change in the lungs, which are greatly disordered by it.

It is true, that death happens so suddenly after introducing the American poison into the vessels, that we cannot well comprehend how it can take place in so short a time ; for it may be said, that the poison is hardly arrived at the heart before the animal is dead : nor is it well understood, how cold-blooded animals can be killed by it (frogs, for instance, which live so long with the circulation stopped) though it be true that they die much slower by these poisons than other animals, whose blood is warm. A humour, or the blood's being affected by a poison, may by degrees produce, in cold-blooded animals, disorders still greater than those which may be caused by stopping the circulation. By death taking place immediately on introducing the poison into the blood, we may be induced to suspect, that there exists in that fluid a very active, subtle, and volatile principle, which eludes the acutest sight, and even the microscope itself. This principle may on this hypothesis appear necessary to life, and against this principle the poison may be supposed chiefly to direct its operation.

That there really exists in the blood a very active and volatile principle, may very well be suspected from seeing that the poison of the viper prevents the coagulation of the blood when it is drawn from the vessels, while, on the contrary, it produces a coagulation of it within the vessels. It may be supposed that something evaporates from the blood when it is drawn out, which exists in it while within the vessels. On this hypothesis this active and vital principle may be considered as the result of the whole animal economy, not excluding the nerves, which may mostly contribute to it. But all this is only mere conjecture, more or less probable, and unsupported by experiment. We ought to abide by sure facts, let the mode of explaining them be what it may. Now these facts are, that the American poison does not act on the nerves, and that its action is entirely on the blood.

Before my experiments, no person would have doubted but that the action of the American poison was immediately on the nerves. All the external signs declared it to be so. These signs then are equivocal, and they are falsely adopted by physicians for the certain proofs that a disease is purely nervous. All these symptoms may exist without the nerves being in the least affected : the alteration of the blood alone is sufficient to produce them in a moment. The principal physicians have attributed the disease produced by the poison of the viper, and by the American poison, to an alteration in the nerves ; it belongs to them now to examine, whether other diseases, supposed generally to be nervous, be not rather diseases of the fluids, than diseases of the blood. The suspicion is great, the signs equivocal ; the principle is shown not to be general. I would not here assert, that no disorder could ever be derived from the nerves ; this would be running into one extreme in order to avoid another. There is no doubt but that many diseases are nervous in their origin, and that many others become so from

disorders which have begun in other parts, and those merely fluid. The illnesses which arise from mental uneasiness show us the power of the nerves on living bodies. But all this does not prove that all the diseases attributed to the nerves are nervous; and that the ordinary signs of this disorder are not equivocal. And it is certain, that the poisons we have examined have no immediate action on the nerves, as has been commonly believed hitherto. Some may object to this, that the poison of the viper and the American poison may act on the extremities of the nerves, and that for this reason they are innocent when applied to the trunk of the nerves. But this would be to object merely for the sake of objection, and to fancy unnecessary difficulties. The smallest variation of circumstance would then be sufficient grounds for objection; and who may not find a variety in things when it is so difficult to meet with 2 things alike? As for myself, I observe that the internal substance of the trunks of the nerves does not appear different from that which forms the extremities of those nerves; that the trunk is subject to pain the same as the extremities; and that I am no inventor of hypotheses which are not confirmed by facts.

In the universality of the consequences which I have deduced from my experiments, I may be deceived; and I may even be deceived in some of the experiments themselves, notwithstanding that they have been very carefully conducted, and that I have sought after truth without prejudice. I do not doubt, but that those who may apply themselves to such researches after me, may find some things to add, and perhaps some to correct also. It is sufficient for me to have opened a channel to new truths, and that the principal facts which I have advanced are true. The greatest part of these experiments were made in the presence of Dr. Ingenhousz, physician to the emperor, my particular friend; a man who has manifested in several works his possessing the talents of a true observer. Mr. Cavallo was also present at many of the more important ones. I thought that the authority of 2 gentlemen, so well known to the learned, would procure the more credit to my experiments.

After having finished my experiments on the American poison, a friend in London procured me a great number of East Indian arrows. I wished to make some experiments on them also; but those I have made are neither many nor sufficiently varied. It appears however, that this poison differs not from the other, except in its being less active, in killing of animals: which lesser activity is probably to be attributed, either to these arrows having been less carefully preserved than those from the West Indies, as really appeared to be the case, or else to the poisons having been prepared many years since. I have never succeeded in killing any rabbit, even the smallest-sized one, with it, by applying it to the skin scratched or slightly wounded, though I have used it in greater quantities, and on more extensive parts of the skin, than the poison of Ticunas;

even in rabbits of scarcely a pound weight it did not produce any sensible alteration.

I pierced the skin of several animals with the arrows, and let them remain in it several whole days, without being able to perceive that the animal was affected with the poison; but when I perforated the muscles with the arrows, and left them there, its effects were very observable. Several animals died in this manner, and that with all the visible signs of the poison, and with all the signs or symptoms with which animals die who are killed by the American poison: it is true however, that none of them died, or were even sensibly affected, till after several hours; so that this poison seems not to differ essentially from the other. It perfectly agrees with it when examined with the microscope, when mixed with turnsol, when thrown into the eyes of animals, when tasted with the tongue, and when chewed between the teeth; on the other hand, it does not dissolve so well in water as the other poison, for indeed a great part of it remains insoluble in that fluid. The only consequence which can be deduced from the facts above-mentioned, is, that the poison is much more noxious when applied to the muscles, than when applied to the skin, in which respect also it agrees very well with the other poisons. This still more convinces us, that the immediate action of these poisons is not against the nerves, since it is certain, that the skin is more sensible than the muscles, and that it is all intersected with nerves.

I have also made a few experiments on the oil of tobacco, the results of which I thought it might not be improper briefly to relate in this place.

Experiments made with Oil of Tobacco.—I made a small incision in the right thigh of a pigeon, and applied to it 1 drop of the oil of tobacco, and in 2 minutes it lost the use of the right foot. I repeated the same experiment on another pigeon, with exactly the same effect. I made a slight wound in the muscles of the breast of a pigeon, and applied the oil of tobacco to it; and in 3 minutes the animal could not stand on its left foot. And this same experiment was repeated on another pigeon, with the same success. Into the muscles of the breast of a pigeon I introduced a tooth-pick steeped in the oil of tobacco, and a few seconds after the pigeon fell down seemingly dead. Having applied the oil of tobacco to 2 other pigeons, they threw up several times all the food they had eaten. Two others, treated in the same manner, but with empty stomachs, made many efforts to vomit. In general, I found the vomiting to be a constant effect of this poison; but the loss of motion in the part to which the poison is applied, is only accidental. None of the animals however died by the application of the oil of tobacco.

Experiments with the Water of the Lauro-Cerasus.—I shall conclude my experiments on poisons with an account of some that I have made on a poison, which has for some years become remarkable in Europe. This poison is the

water of the Lauro-Cerasus, which is not inferior to any of the other very active poisons, if considered with respect to the great disorder which it introduces into the animal economy, and the short time which it takes to act when given to animals by the mouth. It not only produces the strongest convulsions, and death itself, even in animals of a middle size; but even when given in small doses, the animal writhes so that the head joins the tail, and the vertebræ arch out in such a manner as to fill every one who sees it with horror. The convulsions and motions of the whole body are extremely violent, and the struggles kill the animal in a short time. If it be given to the animal as a glister, it equally produces the convulsions and death. With only 2 tea-spoonfuls of the water given by the mouth, I have seen middle-sized rabbits fall down convulsed in 30 seconds, and die within a minute. When it is given in great quantities to animals, they die almost in an instant, and without convulsions, the parts of the body being relaxed and pendent. When it is given in small quantities, the convulsions are more or less strong; the hind feet first lose their motion, and afterwards the fore, which die slower. When the animal can no longer move its feet, or the rest of the body, it still moves very well the neck and the head, which it continues to hold up strongly, and to turn round every way. In this state the animal hears sounds, and sees objects; and though it has ceased to move its feet spontaneously, yet it still moves them backwards and forwards when they are much pricked or squeezed; a sign it can still move them, though it does so only on account of the great pain. The water of the Lauro-Cerasus is therefore a strong poison when given by the mouth, or introduced into the body in the manner of a glister. Its action is so violent, and so quick, that it may be said to begin to act the moment an animal takes it into its mouth; certain it is, that it has scarce entered the stomach by the throat before the animal suffers. It is however true, that very small doses have no effect, that is, a little drop given to a small animal produces no sensible disorder, though the same quantity of the poison of Ticunas would be fatal. But that makes no essential difference between this poison and the other well-known poisons.

I have found, that a certain quantity of water put on the leaves of the Lauro-Cerasus produces a liquor which is quite innocent, unless the leaves be in a great, and the water in a very small quantity. When the water is put several times on the same leaves, and drawn off successively, its activity becomes greater indeed, but still is not sufficient to take away life; but if, instead of a bare mixture with water, the infusion be distilled in balneo mariæ, the distilled liquor becomes then a most powerful poison, and proves fatal in a very short time. I have chiefly used it in this way; but I have no doubt but that it might be raised to such a degree of activity, as to prove mortal even when given in as small doses as the American poison. It might be sufficient to distil the liquor

first obtained several times over again with new leaves well chopped and dried. I believe it would at last be obtained in the form of a concrete oily substance, which, evaporated by fire, would not only be equal in force to any known poison, but far exceed them all. But I reserve this experiment for some other occasion, when I shall also speak of the bitter almonds, and of the degree of poison to which their water can be raised by distilling it till it be dry.

The water of the *Lauro-Cerasus* then kills animals when introduced into the cavity of the body: but what effect does it produce when applied to wounds? It may suffice here to relate one only of the various experiments which I have made. I opened the skin of the lower belly of a pretty large rabbit, and made a wound in it of about an inch long, and having slightly wounded the muscles under it in many places, I applied to the part 2 or 3 tea-spoons full of the water: in less than 3 minutes the animal fell down convulsed, and died soon after. This experiment shows us, that the water of the *Lauro-Cerasus* is a poison similar to the others, and that it operates when insinuated into the body by means of wounds made in it. This experiment has been attended with similar results in other warm-blooded animals; but I have always found, that the water of the *Lauro-Cerasus*, when given by the mouth, acts more powerfully and quicker than in the other way, even when the quantity given is smaller; a circumstance, in my opinion, that deserves the greatest attention, since it is matter of fact, that a large wound offers many more vessels to absorb that poison immediately than the mouth and stomach; besides that the nervous parts ought to be more affected from the very state in which they are put by the wound. It is not the warm-blooded animals alone which are suddenly killed by this water when they are made to drink it, but the cold-blooded animals also die of the effects of it; and what appears to me very singular is, that they die in an extremely short time, and perhaps more quickly than the others; which is quite contrary to what happens from the other poisons. It may suffice for the present to mention eels, which are very difficult animals to kill, and still continue to move their parts when dead. These animals die in a few seconds after having drank the water, and have scarcely swallowed it when they begin to contract themselves; but death suddenly seizes them, and renders them immoveable in a moment, without leaving even the motion of the parts they usually have on being handled. The heart indeed continues to beat, though faintly, but it ceases to move much sooner than when they are killed by cutting off the head. Here it cannot be denied but that the muscular irritability is extremely affected, and in a particular manner. I know not if there be any cold-blooded animal that resists this poison. Those which I tried it on all died; and I doubt whether there be any to which it is not fatal: if so, it deserves a particular distinction, on account of its being the most terrible of all known poisons, as well as for its universality in

proving fatal to all sorts of animals. But how is it always mortal in so short a time, when taken by the mouth, and admitted into the stomach, where we do not suppose there are any vessels capable of receiving it? This difficulty requires some further experiments. We ought to examine what effects it produces when applied immediately to the nerves, and when introduced into the blood, without touching the parts which are cut. For this purpose, I made use of large rabbits, and performed the experiments on their sciatic nerves, in the same manner as I had done with the poison of the viper and the American poison. I shall here transcribe only one experiment, omitting the rest for the sake of brevity, not thinking them necessary after the great number which I have already related on the nerves.

Having detached the sciatic nerve of a large rabbit for above an inch and a half, I introduced under it a wrapper of very fine linen, 16 times doubled, that the parts below it might not be penetrated by the water of the *Lauro-Cerasus*. I then wounded the nerve with many strokes of the lancet in the longitudinal direction, and covered all this wounded part, which extended above 8 lines in length, with a roll of cotton of 3 lines in thickness, well steeped in the laurel-water. More than 15 drops of the water were wanted to moisten the cotton, and this water communicated itself directly by the wounds to the medullary substance of the sciatic nerve. I covered the whole about a minute after with new rags, so that it was impossible for the laurel-water to communicate with the parts below it or near it. Having sewed up the external skin, and left the animal at liberty, it seemed not to be in the least affected, neither then nor afterwards. It ran about, and eat, and was as lively as ever. In short, the animal was not sensibly affected in this way by this poison, which kills so quickly when it is taken in by the mouth. This case, as well as many others, is very similar to those of the poison of the viper and of the American poison; and it shows, that the water of the *Lauro-Cerasus* applied immediately on the nerves, and so insinuated into the medullary substance of them, is not at all poisonous; consequently, that it does not act on the nerves, however applied externally.

After so many experiments, as have been related in the course of this essay, on the poison of the viper, and on the American poison, a still more powerful one than the former; and after having seen that neither of these 2 poisons act on the nerves, when applied immediately to them, while they instantly kill very strong animals when introduced into the blood; nothing was more natural than to conclude, that the poison of the *Lauro-Cerasus*, which is equally innocent with the others when applied to the nerves, would also prove mortal when introduced into the blood: experience however shows quite the contrary; so true is it that we ought to mistrust analogy, even when it appears most uniform. I introduced the water of the *Lauro-Cerasus* into the jugulars of a large rabbit, to the

amount of 5 drops or upwards, in the same manner as I had done the poison of the viper and the American poison, yet the animal showed no signs of suffering. I suspected I had not performed the operation right; that I had not introduced any of the water into the vessels; and that the syringe had insinuated itself into the cellular membrane. I therefore repeated the experiment, and again introduced into the jugular a quantity of poison, perhaps 3 or 4 times greater, and I was very careful to make the point of the syringe enter the jugular properly before I introduced the poison, that the poison should not by any means turn back again; yet still the animal was not affected by it, but continued as lively as ever. I was more surprized than satisfied with all this. I could not persuade myself, that the water of the *Lauro-Cerasus* was not a poison, and a very powerful one too, when introduced into the blood, since it was poisonous when applied to wounds in the flesh, and taken by the mouth, though inactive and harmless when applied to the nerves. I therefore again repeated the experiment, and this time I introduced by the jugular a whole tea-spoonful of the *Laurel-water*; and yet still the animal was not affected. I also tried the same experiment on another rabbit, into the jugulars of which I introduced a large tea-spoonful of the same poison; yet neither did this rabbit show any signs of suffering, either then or afterwards.

The unexpected result of these experiments threw me into a very great uncertainty concerning the action of this poison: I could neither comprehend the mode of its operation, nor even on what parts it acted when taken by the mouth or applied to wounds. Here all was confusion: it was found neither to act on the nerves nor on the blood, and yet it killed, and that in an instant, when introduced into the stomach by the mouth.

Is there then a new way of destroying the life of animals different from that of the blood and of the nerves? The loss of motion, and that too in a few seconds, in such animals as eels, which in other cases continue to move for hours after the head is cut off, and even after they are cut in pieces, would make one believe, that the irritability of the muscular fibres was affected by this poison. It is true indeed, that the heart continues to move in those animals; but that motion is very much lessened, and lasts but for a very short time. In the warm-blooded animals, just killed by this poison, there still exists some motion, but it is very little; and though their heart continues to beat for some time, it beats much less than when they are killed by other means. The irritability is certainly diminished very much in many animals, and in many others entirely destroyed; by whatever means the poison kills in so short a time, and however obscure the mechanism may be by which the muscular fibres lose their irritability. We must confess our ignorance in our researches into nature. When we think we have accomplished every thing, we suddenly find ourselves just where we

began. Experiment is the only guide which we have to conduct us in our researches: experiment is indeed a secure way of avoiding error, but experiment does not always lead us to the more remote truths, nor always guide us to the knowledge of the secret arcana of nature, nor yet always conduct us whither we have proposed to go.

But though we know not how the Laurel-water operates, or, more properly speaking, on what part that poison exerts its action, when it kills animals; we know however, that it is quite innocent when applied immediately to the nerves, and even to their medullary substance: and it is equally true, that all the experiments above related clearly show, that the poison of the viper and the American poison are both harmless any how applied to the nerves; but that they are both poisonous when introduced into the blood. These are facts which were before unknown; they are truths now laid open; nor can they be brought into doubt again by any one. These facts destroy all the systems that have been invented by the writers on the action of those poisons; and from these facts we ought to set out, to arrive at the knowledge of those poisons, and of the manner in which they operate.

Some light might probably have been thrown on the action of the poison of the Lauro-Cerasus, by applying it to different parts of the brain of living animals; but I reserve this experiment for a more convenient occasion than the present, and till I shall have reduced the Laurel-water to the consistence of syrup. In that state, the poison being rendered much more active, will probably offer new and more important facts, and may perhaps give a clearer insight into its operation; as also enable us to judge on what parts of living animals it acts so as to kill them. I shall also reserve for that time the trial of whether that poison acts on the lymphatic vessels, or, to speak more properly, on the lymph itself. This is a mere suspicion which I have lately taken up, and which my present circumstances do not permit me to examine at present. I am therefore forced to give my experiments on this subject, in some measure, defective and unconnected.

VII. Experiments relating to a New Animal Acid.† By F. L. F. Crell, M. D., Professor of Chemistry at Helmstadt. p. 109. An Abstract from the Latin.*

Professor Segner‡ of Gottingen having by distillation extracted from beef suet an acid liquor, which by combination with the vegetable fixed alkali, and with

* This paper should have been inserted at p. 628 of this Abridgment; immediately before the Account of a Woman who had the Small-pox during Pregnancy.

† At first called acid of fat, but afterwards denominated sebacic acid.

‡ Diss. Inaug. de Acido Pinguedinis Animalis; præside Io. A. Segnero. Respond. D. H. Knappe, Gott. 1754.

the volatile alkali, yielded neutral salts possessing peculiar properties; Dr. Crell was induced to make the following experiments, with the view of ascertaining more exactly the constituent parts of fat, and the nature and properties of the acid extracted from it.

Exper. 1. He took lbij. of beef suet, previously melted and strained. With this he filled a glass retort half full, and having applied a receiver and well luted the vessels together, he subjected the suet to distillation in a sand bath. At first there came over a thin oil, which remained fluid, next an acid which settled at the bottom of the receiver, and at the same time an oil which congealed immediately as it fell by drops into the receiver. It required 16 hours, and a very strong heat towards the end, before the distillation of all the fluid matter was effected. When the vessels had become cold, most of the oil was found congealed, and on loosening the receiver, there came forth an intolerable stench, which irritated the eyes and nose to such a degree as nearly to occasion suffocation. The fluid part being poured off, it weighed $3\frac{1}{2}$ oz. and $\frac{1}{2}$ scr. and consisted of 2 different kinds of liquid, (separated by means of a funnel) namely, a greenish oil, of the colour of oil of wormwood, weighing 1 oz. 7 dr. 2 scr. and a heavier acid liquor of a gold colour, extremely pungent, and weighing 1 oz. $3\frac{1}{2}$ dr. The congealed or concrete oil which formed the other part of the product, and which resembled hog's lard, had still a pungent smell, which was not proper to it, but was owing to the acid from which it could not be totally freed. There remained in the retort a coally matter, which weighed 1 oz. $4\frac{1}{2}$ dr.

Exper. 2, 3, 4, 5, 6, 7, 8, 9. In these experiments the coagulable oil was rectified by repeated distillations, during which the oily and acid products acquired different shades of yellow, orange, and red; and in one experiment there escaped through the luted joinings of the vessels, an inflammable vapour.

Exper. 10. The whole of the fluid matter extracted by distillation from the suet was found to consist of 3 oz. 5 dr. of a bright gold coloured acid, and $21\frac{1}{2}$ oz. of reddish brown oil. If to this weight, viz. 25 oz. 1 dr., of the fluid products, be added the weight of the coally residue, viz. 5 oz. 2 scr., collected in the different processes, the loss from the whole quantity of suet operated on, amounts to 1 oz. 6 dr. 1 scr.

Exper. 11. It not being possible, as Professor Segner had remarked, to separate by mechanical means, the whole of the acid from the oil; Dr. C. for this purpose added to the whole quantity of oil he had obtained an equal quantity of water, and then subjected the mixture to a digesting heat, shaking the vessel frequently; after which he found that the water had acquired an acid taste, and effervesced with an alkali. This he repeated until the water ceased to taste sour, or to effervesce on the addition of an alkaline salt.

But it was necessary to determine more exactly the quantity of acid retained by the oil: for this purpose

Exper. 12. To $\frac{1}{2}$ dr. of pure salt of tartar he added so much of the acid obtained in exper. 10, as was sufficient to saturate it completely; $6\frac{1}{2}$ dr. of the acid were required for this purpose.

Exper. 13. Now 2 dr. of salt of tartar were found barely sufficient for the complete saturation of the acid contained in the water impregnated with this acid in exper. 11. Therefore, calculating according to the results obtained in exper. 12, the quantity of acid contained in this water amounted to 3 oz. 2 dr.

Exper. 14. Three oz. of the reddish-brown oil that had beenedulcorated with water, exper. 11, were mixed with an equal quantity of distilled water, and placed over a lamp. Before the water rose up, there came over a limpid oil, resembling ethereal oil both in smell and taste; next along with the water there passed over a portion of white oil. When the whole had distilled over, the lamp was removed, and 3 oz. of oil were separated from the water, which had acquired an acid taste.

Exper. 15. The whole quantity of oil, exper. 11, being distilled per se, by the heat of a lamp, a limpid oil was obtained, similar to that in exper. 14; and a small quantity of red acid remained at the bottom.

Exper. 16. This oil, exper. 15, being digested with a strong heat, in circulatory vessels, it acquired a darker colour, till at length it became of a dark bay.*

Exper. 17. The residuum after the distillation in exper. 15 was exposed to a strong heat, till nothing more could be distilled from it. On breaking the retort there was found a coally matter, which weighed 3 oz. 18 dr. exactly similar to that obtained in exper. 1, 4, 7, 9.†

Exper. 18, 19. Three dr. of the oil, exper. 14, 15, were mixed with 2 oz. of sp. of wine. Of this mixture $\frac{1}{2}$ was subjected to digestion in circulatory vessels, and the other half was distilled in a retort. The first portion was not dissolved by the sp. of wine, but the latter was.

Exper. 20. On the addition of water both liquors turned milky; but they afterwards became clear, the oil rising up to the surface.

Exper. 21, 22. By adding caustic salt of tartar made hot to the rectified oil, exper. 14, boiling hot, a soap was produced. The result was nearly the same with the oil of exper. 11.

* Dr. C. remarks that on subjecting Dippel's animal oil to the same treatment, it underwent a similar change.

† On comparing the results obtained in exper. 10, 13, 17, with each other, it will appear, says Dr. C., that lbij. of suet consist of pure oil 14 oz., acid 7 oz. 2 scr., carbonaceous matter 10 oz. 6 dr. 1 scr.

Exper. 23, 24. A saponaceous compound was also obtained by adding to the rectified oil spirit of sal ammoniac, prepared with quick lime. The result was the same when the oil of *exper. 11* was employed.

Exper. 25, 26. But this rectified oil would not form a soap with the crystallized volatile alkali. The attempt was equally unsuccessful when made with the salt previously dissolved in a very small quantity of water.

Exper. 27, 28, 29, 30, 31, 32, 33, 34, 35, 36, 37. In these experiments the rectified oil was subjected to the action of the vitriolic, nitrous, muriatic, and acetic (*Sp. Veneris*) acids. The action of the vitriolic, muriatic, and acetic acids did not exhibit any thing that requires particular notice; but when the nitrous acid, *exp. 31, 32, 33*, was added to the oil obtained in *exp. 10*, a smoke and coagulation followed, with a precipitation of some particles, which appeared to be of a carbonaceous nature. The liquor being poured off, this precipitate was found to be soluble in water, to which it communicated a straw-colour, and a bitter taste. On evaporating this solution, a yellow concrete was obtained, of a lamellated saline appearance. It was soluble in *sp. of wine*, to which it imparted a gold-colour. On adding salt of tartar to the aqueous solution, no separation of the oil ensued.

Exper. 38. Dr. C. next proceeded to examine the coally residuum collected after the distillation of the oil. This he found it extremely difficult to incinerate; but by subjecting it to a very strong heat in a wind-furnace, he at length succeeded in reducing it to ashes. The weight of the coally residuum operated on was 2 oz., that of the ashes obtained from it 3 dr.

Exper. 39. To the ashes obtained in *exper. 38*, which were of a reddish colour, Dr. C. added 2 oz. of distilled water. The mixture was then digested in a gentle heat, and afterwards filtrated. The water had a saline taste; but on evaporating it till a cuticle formed on the surface, no crystals appeared; but continuing the evaporation to dryness, he obtained 41 gr. of a salt, which was of no determinate figure, but had a very peculiar taste; it did not deliquesce on exposure to the air.

Exper. 40. Vitriolic acid being added to the aqueous solution of this salt, a precipitation of white grains or molecules took place.

Exper. 41. Having separated this precipitate from the liquor by filtration, and having dried it, Dr. C. found it to be a white salt of an acid taste, which being subjected to the blow-pipe melted into a transparent glass. This he afterwards converted into phosphorus by the usual process.

Exper. 42. To ascertain whether the red colour of the ashes was owing to the presence of iron, Dr. C. digested the ashes deprived of their salt, *exper. 39*, in aquafortis, for 24 hours; but on adding some drops of the aquafortis so digested, to an infusion of gall-nuts, no purple or black colour was produced. Hence he infers that the red colour of the ashes is not derived from iron.

Exper. 43. To the aquafortis which had been digested with the ashes, some vitriolic acid being added, the liquor immediately became turbid, and selenitic particles were precipitated: whence it appears that the ashes contained some uncombined calcareous earth. The selenitic particles being separated by filtration, and the liquor being afterwards evaporated, it left no solid matter behind.

Exper. 44. The portion of ashes which was not dissolved by the nitrous acid, after beingedulcorated and dried, weighed 2 dr. On this 1½ oz. of oil of vitriol was poured; it was then subjected to a strong heat until all the fluid was driven off, and was afterwards boiled in distilled water. This liquor being filtrated and evaporated, it yielded small crystals, tasting like alum, and letting fall a precipitate on the addition of an alkali.

Exper. 45. Theedulcorated and dried residuum being mixed with an equal quantity of mineral alkali, and being afterwards melted in a crucible by a strong heat, it yielded a very good transparent glass. Dr. C. next proceeded to examine the nature of the acid and its action on other bodies.

Exper. 46. He distilled with a gentle heat the acid of exper 10. What came over was of a whitish colour inclining to yellow; there remained at the bottom of the retort an unctuous coally residuum. The attempt to dephlegmate the acid by distillation in a gentle heat, proved ineffectual; the liquor in the receiver was found equally acid with that in the retort.

Exper. 47. The salt formed by the combination of this acid with the vegetable fixed alkali, has been already mentioned under exp. 12. That formed with the mineral alkali is here described. It took 5 oz. of the acid of exper. 46 to saturate 3 dr. of the crystallized mineral alkali. This solution being evaporated to a cuticle, it yielded brown-coloured crystals, which were afterwards put into a crucible and fused in a moderate degree of heat, until they ceased to emit any vapour, or smoke. The saline mass was then dissolved in water, and the solution being evaporated to the point of crystallization, it afforded cubic crystals, terminating for the most part in triangular pyramids. On exposure to the air these crystals became covered over with a white mealy powder.

Exper. 48. The salt formed by the union of this acid with the volatile alkali sublimed, in the form of white flowers, in the same degree of heat at which common sal ammoniac sublimes.

Exper. 49. This acid dissolved calcareous earth with much effervescence. Two dr. of the acid were sufficient for the saturation of 11 dr. of this earth. The same salt was prepared by mixing suet with pulverized quick lime, then melting the mixture in a gentle heat, and afterwards boiling it in water. The filtrated and evaporated liquor concreted into a saline mass. The experiment succeeded better when a mixture of suet and quick lime was subjected to distillation. The lixivium being evaporated to a due consistence, it afforded crystals

of a brown colour; these were subjected to fusion, as in exper. 47, and afterwards dissolved in distilled water. This solution being evaporated, it yielded transparent hexagonal crystals, terminated by a plane surface. This salt has a sharp taste, but is not so pungent as sal ammoniac; it dissolves readily in water, but does not deliquesce in the air; it is not soluble in sp. of wine. Dr. C. thinks it might be called sal calcarium animale.

Exper. 50. This acid dissolves magnesia alba with effervescence. Two dr. of the acid are sufficient for saturating 9 dr. of this earth. The compound is not crystallizable.

Exper. 51. The earth of alum, though not without difficulty, combined with this acid. Two dr. of the acid were added to $\frac{1}{2}$ dr.* of the precipitate, in a moist state, obtained from alum by means of a fixed alkaline salt. At first the whole appeared to be dissolved; but in a short time a considerable part, seemingly about half, settled at the bottom. The filtrated liquor could not be brought to crystallize; it had an astringent, not sweetish, but rather austere taste. On adding an alkali, the dissolved earth was immediately precipitated. It may be called alumen animale.

Exper. 52. Siliceous earth did not appear to be acted on by this acid.

Exper. 53. To obtain this acid in a concentrated state, which was a primary object in these experiments, Dr. C. added to 12 dr. of the salt compounded of the aforesaid acid and the vegetable alkali, $\frac{1}{2}$ oz. of oil of vitriol, and afterwards distilled off the (sebacic) acid in a gentle heat. It came over in the form of grey vapours. The distilled liquor was as limpid as water, was strongly acid, and weighed $\frac{1}{2}$ oz.

Exper. 54. Half an oz. of the concentrated acid, exp. 53, was mixed with an equal quantity of sp. of wine, and then digested and distilled. The liquor which passed over into the receiver had the same smell as the ol. vini. On adding water to it, the liquor turned milky; but it soon afterwards became clear, the oil rising up to the surface. It had an aromatic taste, but in a less degree than the ol. vini.

Exper. 55. A solution of the oil in sp. of wine being subjected to distillation, some ether (naphtha) passed over into the receiver. In this operation, the acid uniting more intimately with the sp. of wine, and both being more volatile than the oil, they were separated from it.

XII. A Conjecture concerning the Method by which Cardan's Rules for resolving the Cubic Equation $x^3 + qx = r$ in all cases (or in all magnitudes of the known quantities q and r) and the Cubic Equation $x^3 - qx = r$ in the first Case of it (or when r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{9^3}{27}$) were pro-

* It does not seem to act on the exsiccated aluminous earth; and $\frac{1}{2}$ dr. after all the humidity was expelled from it, weighed only 4 gr.

ably discovered by Scipio Ferreus, of Bononia, or whoever else was the first Inventor of them. By F. Maseres, Esq., F. R. S. p. 221.

Art. 1. There is nothing more amusing, or more grateful to an inquisitive mind, in the study of the sciences of Geometry and Algebra (for if we banish from it the ridiculous mysteries arising from the supposition of negative quantities, or quantities less than nothing, the latter may deserve the name of a science as well as the former) than to contemplate the methods by which the several ingenious and surprizing truths that are delivered in the books that treat of them were first discovered. This we are sometimes enabled to do by the authors themselves to whom we are indebted for these discoveries, who have candidly informed their readers of the several steps, and sometimes of the accidents, by which they have been led to them: but it also often happens, that the authors of these discoveries have neglected to give their readers this satisfaction, and have contented themselves with either barely delivering the propositions they have found out, without any demonstrations, or with giving formal and positive demonstrations of them, which command indeed the assent of the understanding to their truth, but afford no clue, to discover the train of reasoning by which they were first found out; and consequently contribute but little to enable the reader to make similar discoveries himself on the like subjects. This seems to be the case with those ingenious rules for the resolution of certain cubic equations, which are usually known by the name of Cardan's rules. We are told to make certain substitutions of some quantities for others, in these equations $x^3 + qx = r$ and $x^3 - qx = r$, which are the objects of those rules, and certain suppositions concerning the quantities so substituted; by doing which we find, that those equations will be transformed into other equations which will involve the 6th power of the unknown quantity contained in them, but which (though of double the dimensions of the original equation $x^3 + qx = r$ and $x^3 - qx = r$, from which they were derived) will be more easy to resolve than those equations, because they will contain only the 6th power and the cube of the unknown quantity which is their root, and consequently will be of the same form as quadratic equations; so that, by resolving them as quadratic equations, we may obtain the value of the cube of the unknown quantity which is their root, and afterwards, by extracting the cube-root of the said value, we may obtain the value of the said root, or unknown quantity itself; and then at last, by the relation of this last root to x , or the root of the original equation, (which relation is derived from the suppositions that have been made in the preceding transformations) we may determine the value of x . And if we please to examine the several steps of this process with sufficient attention, we may perceive, as we go along, that all these substitutions are legitimate and practicable, or are founded on possible suppositions; though I cannot but observe, that the writers on

algebra, for the most part, have not been so kind as to show us that they are so. But still the question recurs, "How came Scipio Ferreus, of Bononia, who, as Cardan tells us, was the first inventor of these rules, or the other person, whoever he was, that invented them, to think of making these lucky substitutions, which thus transform the original cubic equations into equations of the 6th power, which contain only the 6th and 3d powers of the unknown quantities which are their roots, and consequently are of the form of quadratic equations?" To answer this question as well as I can by conjecture (for I know of no historical account of this matter in any book of algebra) and in a manner that appears to me to be probable, is the design of the following pages.

2. The most probable conjecture concerning the invention of these rules, called Cardan's rules, by Scipio Ferreus, of Bononia, or whoever else was the inventor of them, seems to be this: that the said inventor tried a great variety of methods of reducing the 3 cubic equations of the 3d class, to wit, $x^3 + qx = r$ and $x^3 - qx = r$, and $qx - x^3 = r$ (to some one of which all other cubic equations may, by proper substitutions, be reduced) to a lower degree, or to a more simple form, by substituting various quantities in the stead of x , in hopes that some of the terms arising by such substitutions might be equal to others of them, and, having contrary signs prefixed to them, might destroy them, and so render the new equation more simple and manageable than the old one. And, among other trials, it seems natural to imagine, that he would substitute the sum or difference of 2 other quantities instead of x , as the most simple and obvious substitutions, that could be made. And by making these substitutions, the abovementioned rules would of course come to be discovered, as well as the aforesaid limitation of them in the resolution of the equation $x^3 - qx = r$, which restrains the rule to those cases only in which r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ greater than $\frac{q^3}{27}$, and their utter inutility in all the cases of the equation $qx - x^3 = r$. This will appear by examining each of these equations separately in the following manner.

Art. 3. In the 1st equation $x^3 + qx = r$, the investigator of these rules would naturally be inclined to substitute the difference of 2 quantities, y and z , instead of x , rather than their sum, or would suppose x to be equal to $y - z$, rather than to $y + z$; because, if he was to suppose x to be equal to the sum of the 2 quantities y and z , and was to substitute that sum, or the binomial quantity $y + z$, instead of x in the equation $x^3 + qx = r$, it is evident, that (as the signs x^3 and qx are both affirmative) the terms of the new equation, arising from such substitution, would be all likewise affirmative; and consequently none of them, though they should happen to be exactly equal to each other, could exterminate each other, and thus render the new equation more simple than the old one; which was the only view with which the substitution would have

been made. He would therefore suppose x to be equal to $y - z$; and by substituting this quantity instead of x in the original equation $x^3 + qx = r$, he would transform that equation into the following one, to wit, $y^3 - 3yz \times (y - z) - z^3 + q \times (y - z) = r$. Now in this equation it is evident, that the terms $3yz \times (y - z)$ and $q \times (y - z)$ have contrary signs; and therefore, if their co-efficients $3yz$ and q can be supposed to be equal to each other, those terms will mutually destroy each other, and the equation will be reduced to the following short one, $y^3 - z^3 = r$. And if in this equation we substitute, instead of z , its value $\frac{q}{3y}$, derived from the same supposition of the equality of q and $3yz$, the equation will be $y^3 - \frac{q^3}{27y^3} = r$; and, by multiplying both sides by y^3 , it will be $y^6 - \frac{q^3}{27} = ry^3$; which equation, though it rises to the 6th power of the unknown quantity y , is evidently of the form of a quadratic equation, and may therefore be resolved, so far as to find the value of the cube of y , in the same manner as a quadratic equation; after which it will be possible to find the value of y itself by the mere extraction of the cube root; and then at last, from the relation of y to x (derived from the foregoing suppositions that $y - z$ was equal to x , and that $3yz$ was equal to q , and consequently z equal to $\frac{q}{3y}$) we shall be able to determine the value of x .

Art. 4. It would therefore remain for the investigator of this method to inquire, whether the supposition of $3yz$ being equal to q , was a possible supposition; that is, whether it was possible (whatever might be the magnitude of q and r) for 2 quantities, y and z , to exist, whose nature would be such that their difference $y - z$ should be equal to the unknown quantity x in the equation $x^3 + qx = r$, and that 3 times their product should at the same time be equal to q . And this supposition he would soon find to be always possible, whatever may be the magnitudes of q and r ; because, if the lesser quantity z be supposed to increase from 0 ad infinitum, and the greater quantity y be likewise supposed to increase with equal swiftness, or to receive equal increments in the same times, and thus to preserve their difference $y - z$ always of the same magnitude, or equal to x , it is evident that the product or rectangle yz will increase continually at the same times from 0 ad infinitum, and consequently will pass successively through all degrees of magnitude, and therefore must at one point of time during its increase become equal to $\frac{1}{3}q$.

And having thus found this supposition of the equality of yz and $\frac{1}{3}q$, or of $3yz$ and q , to be always possible, whatever might be the magnitudes of q and r , our investigator would justly consider his solution of the equation $x^3 + qx = r$, which was founded on that supposition, as legitimate and complete. And thus we see in what manner it seems probable, that Cardan's rule for resolving the cubic equation $x^3 + qx = r$ may have been discovered.

Art. 5. In the 2d equation $x^3 - qx = r$, in which the 2d term qx is subtracted from the first, or marked with the sign $-$, it seems to have been natural for the person who invented these rules to substitute the sum as well as the difference of 2 other quantities, y and z , instead of x , in the terms x^3 and qx , in hopes of such an extermination of equal terms, and consequential reduction of the equation to one of a simpler and more manageable form, as was found to be so useful in the case of the former equation $x^3 + qx = r$. We will therefore try both these substitutions, and, as that of the difference $y - z$ has in the former case proved so successful, we will begin by that.

Art. 6. Now, by substituting the difference $y - z$ instead of x in the equation $x^3 - qx = r$, we shall transform it into the following equation, to wit, $y^3 - 3yz \times (y - z) - z^3 - q \times (y - z) = r$; in which the terms $3yz \times (y - z)$ and $q \times (y - z)$ have both of them the same sign $-$ prefixed to them, and consequently can never exterminate each other, whether $3yz$ be equal or unequal to q . This substitution therefore is in this case of no use.

Art. 7. We will now therefore try the substitution of the sum of y and z , instead of their difference, in the equation $x^3 - qx = r$. Now, if x be supposed to be equal to $y + z$, and $y + z$ be substituted instead of it in the equation $x^3 - qx = r$, that equation will be thus transformed into the following one, to wit, $y^3 + 3yz \times (y + z) + z^3 - q \times (y + z) = r$. Now in this equation, $3yz \times (y + z)$ and $q \times (y + z)$ have contrary signs. Consequently, if they can be supposed to be equal to each other, they will destroy each other, and the equation will be thus reduced to the following short one, $y^3 + z^3 = r$; that is, if $3yz$ and q can be supposed to be equal to each other, or if yz can be supposed to be equal to $\frac{1}{3}q$, the equation will be reduced to the short equation $y^3 + z^3 = r$. And if in this short equation we substitute, instead of z its value $\frac{q}{3y}$, derived from the same supposition of the equality of $3yz$ and q , the equation thence resulting will be $y^3 + \frac{q^3}{27y^3} = r$; and by multiplying both sides by y^3 , it will be $y^6 + \frac{q^3}{27} = ry^3$; and, by subtracting y^6 from both sides, it will be $ry^3 - y^6 = \frac{q^3}{27}$; which, though it rises to the 6th power of y , is evidently of the form of a quadratic equation, and consequently may be resolved in the same manner as a quadratic equation, so far as to find the value of y^3 , or the cube of the root y ; after which it will be possible to find the value of y itself by the mere extraction of the cube root; and lastly, from the relation of y to x (contained in the 2 suppositions, that $y + z$ is equal to x , and that $3yz$ is equal to q , and consequently that z is equal to $\frac{q}{3y}$) we may determine the value of x .

Art. 8. The only thing therefore, that would remain for the investigator of these rules to do, in order to know whether the foregoing method of resolving

the equation $x^3 - qx = r$ was practicable or not, would be to inquire, whether it was possible in all cases, that is, in all magnitudes of the known quantities q and r , for $3yz$ to be equal to q , or for yz (or the product or rectangle of the 2 quantities y and z , whose sum is equal to x) to be equal to $\frac{1}{3}q$, and, if it was not possible in all cases, but only in some, to determine in what cases it was possible, or what must be the relation between q and r to make it possible.

Art. 9. Now, in order to determine this question, it would be proper and natural to observe, that the quantity yz , or the product of the 2 quantities y and z , whose sum is supposed to be equal to x , can never be greater than the square of half that sum, that is, than the square of $\frac{1}{2}x$, or than $\frac{1}{4}xx$, by El. 2, 5, but may be of any magnitude that does not exceed that square. Therefore, if $\frac{1}{3}q$ is greater than $\frac{1}{4}xx$, it will be impossible for yz to be equal to it; but, if $\frac{1}{3}q$ is either equal to, or less than $\frac{1}{4}xx$, it will be possible for yx to be equal to it, and if $\frac{1}{3}q$ is exactly equal to $\frac{1}{4}xx$, z will be exactly equal to y , and each of them equal to one half of x . We must therefore inquire, what is the magnitude of x when $\frac{1}{3}q$ is equal to $\frac{1}{4}xx$. Now when $\frac{1}{4}xx$ is $= \frac{1}{3}q$, then xx will be $= \frac{4q}{3}$, and $x = \frac{2\sqrt{q}}{\sqrt{3}}$: therefore, when x is less than $\frac{2\sqrt{q}}{\sqrt{3}}$, it will be impossible for yz to be equal to $\frac{1}{3}q$; but when x is greater than $\frac{2\sqrt{q}}{\sqrt{3}}$, it will be possible for yz to be equal to $\frac{1}{3}q$.

But when x is $= \frac{2\sqrt{q}}{\sqrt{3}}$, x^3 will be $= \frac{8q\sqrt{q}}{3\sqrt{3}}$ and qx will be $= \frac{2q\sqrt{q}}{\sqrt{3}}$ or $\frac{6q\sqrt{q}}{3\sqrt{3}}$, and consequently $x^3 - qx$ will be $= \frac{8q\sqrt{q}}{3\sqrt{3}} - \frac{6q\sqrt{q}}{3\sqrt{3}} = \frac{2q\sqrt{q}}{3\sqrt{3}}$.

Therefore, if it be true, as we shall presently see that it is, that while x increases from being equal to \sqrt{q} (which is evidently its least possible magnitude) to any other magnitude, the compound quantity $x^3 - qx$, or the excess of x^3 above qx , will also continually increase from 0 (to which it is equal when x is $= \sqrt{q}$, or xx is $= q$) to some correspondent magnitude, without ever decreasing; it will follow that, when x is less than $\frac{2\sqrt{q}}{\sqrt{3}}$, the compound quantity $x^3 - qx$ will be less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$; and when x is greater than $\frac{2\sqrt{q}}{\sqrt{3}}$, the compound quantity $x^3 - qx$ will be greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$; and, è converso, if the compound quantity $x^3 - qx$ is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, x will be less than $\frac{2\sqrt{q}}{\sqrt{3}}$; and if the compound quantity $x^3 - qx$ is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, x will be greater than $\frac{2\sqrt{q}}{\sqrt{3}}$. Consequently, if the compound quantity $x^3 - qx$, or its equal, the absolute term r in the equation $x^3 - qx = r$, is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, it will be impossible for yz to be equal to $\frac{1}{3}q$; but, if $x^3 - qx$, or r , is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is

greater than $\frac{q^3}{27}$, it will be possible for yz to be equal to $\frac{1}{3}q$. Therefore, if $x^3 - qx$, or r , is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, the foregoing method of resolving the cubic equation $x^3 - qx = r$ will be impracticable; but if $x^3 - qx = r$, or r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, it will be practicable.

Art. 10. It now only remains to be proved, that while x increases, from being equal to \sqrt{q} , ad infinitum, the compound quantity $x^3 - qx$ will likewise increase from 0 ad infinitum, without ever decreasing. Now this may be demonstrated as follows.

Art. 11. It is evident, that while x increases from being equal to \sqrt{q} ad infinitum, both the quantities x^3 and qx will increase ad infinitum likewise. But it does not therefore follow, that the excess of x^3 above qx will continually increase at the same time. This will depend on the relation of the contemporary increments of x^3 and qx : if the increment of x^3 in any given time be equal to the contemporary increment of qx , the compound quantity $x^3 - qx$ will neither increase nor decrease, but continue always of the same magnitude during the said time, notwithstanding the increase of x ; if the former increment be less than the latter, the said compound quantity will decrease; and if it be greater, it will increase. We must therefore inquire, whether the increment of x^3 in any given time be greater or less than the contemporary increment of qx .

Art. 12. Now if \dot{x} be put for the increment which x receives in any given time, the increment of x^3 in the same time will be the excess of $(x + \dot{x})^3$ above x^3 , that is, the excess of $x^3 + 3x^2\dot{x} + 3x\dot{x}^2 + \dot{x}^3$ above x^3 ; and the increment of qx in the same time will be the excess of $q \times (x + \dot{x})$, or $qx + q\dot{x}$, above qx ; that is, the increment of x^3 will be $3x^2\dot{x} + 3x\dot{x}^2 + \dot{x}^3$, and that of qx will be $q\dot{x}$. Now, in the equation $x^3 - qx = r$, it is evident that xx must be greater than q ; for otherwise x^3 would not be greater than qx , as it is supposed to be. Consequently $xx \times \dot{x}$ must be greater than $q\dot{x}$; and, à fortiori, $3x^2\dot{x} + 3x\dot{x}^2 + \dot{x}^3$, which is more than triple of $x^2\dot{x}$, must be greater than $q\dot{x}$; that is, the increment of x^3 will be greater than the contemporary increment of qx . Therefore the excess of x^3 above qx , or the compound quantity $x^3 - qx$, will increase continually, without decreasing, while x increases from \sqrt{q} ad infinitum. Q. E. D.

Art. 13. It follows therefore, on the whole of these inquiries, that if the compound quantity $x^3 - qx$, or its equal, the absolute term r , be less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ less than $\frac{q^3}{27}$, it will be impossible for yz to be equal to $\frac{1}{3}q$, and consequently the foregoing method of resolving the equation $x^3 - qx = r$ will be impracticable; but if $x^3 - qx$ or r be greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ greater than $\frac{q^3}{27}$ it will be possible for yz to be equal to $\frac{1}{3}q$, and consequently, the foregoing me-

thod of resolving the equation $x^3 - qx = r$ will be practicable. And thus we see in what manner it is probable that Cardan's rule for resolving the cubic equation $x^3 - qx = r$, in the first case of it, or when r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, together with the restriction of it to that first case, may have been discovered.

Art. 14. In the 3d equation $qx - x^3 = r$, the terms x^3 and qx have different signs, as well as in the 2d equation $x^3 - qx = r$; and therefore it seems to have been natural for the inventor of Cardan's rules to try both the substitutions of $y - z$ and $y + z$ instead of x in this equation, as well as in that 2d equation, in hopes of an extermination of equal terms that are marked with contrary signs, and a consequent reduction of the equation to another which, though of double the dimensions of the equation $qx - x^3 = r$, should have been of a simpler form and more easy to be resolved. But it will be found on trial, that neither of these substitutions will answer the end proposed.

Art. 15. For, in the first place, let us suppose x to be $= y - z$. Then we shall have $x^3 = y^3 - 3yz \times (y - z) - z^3$, and $qx = q \times (y - z)$, and consequently $qx - x^3 = q \times (y - z) - y^3 + 3yz \times (y - z) + z^3$. Therefore, $q \times (y - z) - y^3 + 3yz \times (y - z) + z^3$ will be $= r$. Now in this equation it is evident that the terms $q \times (y - z)$ and $3yz \times (y - z)$ have the same signs, and therefore can never destroy each other. Therefore no such method of resolving this equation $qx - x^3 = r$, as was found above for the 2 former equations $x^3 + qx = r$ and $x^3 - qx = r$, can be obtained by substituting the difference $y - z$ in it instead of x .

Art. 16. We may now try the substitution of $y + z$ instead of x in the terms of this equation. Now if x be supposed to be $= y + z$, we shall have $x^3 = y^3 + 3yz \times (y + z) + z^3$, and $qx = q \times (y + z)$, and consequently, $qx - x^3 = q \times (y + z) - y^3 - 3yz \times (y + z) - z^3$. Therefore, $q \times (y + z) - y^3 - 3yz \times (y + z) - z^3$ will be $= r$. In this equation it is true indeed that the terms $q \times (y + z)$ and $3yz \times (y + z)$ have different signs. But they cannot be equal to each other: for since the 3 terms y^3 and $3yz \times (y + z)$ and z^3 are all marked with the sign $-$, or are to be subtracted from the first term $q \times (y + z)$, and the remainder is $= r$, it is evident, that $q \times (y + z)$ must be greater than the sum of all the 3 terms y^3 , $3yz \times (y + z)$, and z^3 , taken together, and therefore, à fortiori, greater than $3yz \times (y + z)$ alone. Therefore no such extermination of equal terms marked with contrary signs, as took place in the transformed equations derived from the 2 former equations $x^3 + qx = r$, and $x^3 - qx = r$, can take place in this transformed equation, derived from the equation $qx - x^3 = r$, by substituting $y + z$ in its terms instead of x ; and consequently no such method of resolving the equation $qx - x^3 = r$, as has been found for the

resolution of the equations $x^3 + qx = r$ and $x^3 - qx = r$, can be obtained by means of that substitution.

Art. 17. These are the methods of investigation by which I conceive it to be probable, that Cardan's rules for the resolution of the cubic equations $x + qx = r$ and $x^3 - qx = r$, together with the limitation of the rule relating to the latter of those equations, and their inapplicability to the 3d equation $qx - x^3 = r$, may have been discovered by the first inventor of them.

XIII. A New Method of treating the Fistula Lachrymalis. By Mr. Wm. Blizard, Surgeon, F. A. S. p. 239.

After premising some remarks on the causes of obstruction in the nasal duct, Mr. B. proceeds to state that from whatever cause the obstruction had its origin, in its early state, when unattended with a morbid change of the contiguous parts, it is considered as the first and most simple stage of the fistula lachrymalis. It is in this stage that the means of obviating the necessity of a troublesome and uncertain operation should be employed, with any rational expectation of success. The principal of these means are: 1. Compression; declared by experienced practitioners to be injudicious. 2. The passing an instrument into the nostril, and up the duct; an operation very painful to the patient, and exceedingly troublesome to the operator. 3. The introducing a probe through one of the puncta into the duct, after M. Anel's manner; by experience proved to be inadequate to the design. 4. The impelling a fluid, by a syringe, through one of the puncta, as directed by M. Anel; allowed by judicious and experienced surgeons to be sometimes useful.

Reflecting on the last method, Mr. B. was induced to think, that if a fluid, of a great degree of specific gravity, as quicksilver, could be passed through one of the puncta, so as to fill the sac and duct, and press on the obstructed part, it might be reasonably expected to remove the obstruction in the first and simple stage of the disease; at least, to have a much better chance of producing this effect, than a watery fluid, urged through the punctum in an unfavourable direction: besides, it would be no bar to the use of proper general means.

Flattered with the seeming reasonableness of the suggestion, and convinced of the safety of the experiment, he resolved on making a trial the first opportunity; which soon occurred to him. Mr. M—B—, a sadler, in Mark-lane, had been troubled with a flux of tears and mucus down the cheek from the puncta of the right eye-lids, about 7 months. There was a degree of swelling or distension of the sac, attended with pain. On pressing the sac, much ropy fluid, of a whitish colour, was forced through the puncta. The discharge was always in greatest abundance in the evening; at which time he had a dimness of sight in that eye. The usual means had been employed, without success, by his sur-

geon, who approved of the suggested experiment, and the patient agreed to have it tried.

Messrs. Nairne and Blunt provided an instrument for the purpose. It consists of a fine steel pipe, a little curved, cemented in a glass tube about 6 inches long. At the top of the tube is a wooden funnel; and at the bottom of this is a valve, which may be elevated by a silken string conveyed through a hole in the brim of the funnel, and hanging down by the side of the tube.* The steel pipe was passed into the inferior punctum, without pain or difficulty. The quicksilver was then poured into the funnel, and let down the tube by pulling the string of the valve. When the quicksilver regurgitated out by the superior punctum, the instrument was withdrawn. The quicksilver lay in the sac and duct, without exciting pain, about 30 hours, when it passed into the nose, and the patient caught some of it in his hand. Mr. B. thought it best at this time not to compress the sac; apprehending it would discharge the quicksilver through the puncta, and so frustrate the intention.

On the 3d day the operation was repeated; when, on gently compressing the sac, some of the quicksilver passed into the nose, and with it a piece of congealed whitish mucus. A small quantity of the quicksilver, on making the pressure, returned through the puncta. At the 3d and 4th times of repeating the operation, without any compression, at intervals of a few days, the quicksilver passed readily into the nose. Mr. B. once introduced the point of a steel pipe, used for injecting the lymphatic vessels. It is cemented to a tube of glass 18 inches long. This pipe is not so fine as that of the other instrument, yet it was conveyed into the punctum without difficulty, and with little or no pain. To gain a greater degree of momentum, he raised the column of quicksilver to about 12 inches, when it flowed into the nose with a considerable degree of velocity. From the time that the quicksilver passed into the nose, less fluid trickled down the cheek than before. After the 2d or 3d operation, the swelling or distension of the sac entirely subsided. The patient at the above date had no discharge of mucus, and a tear but very seldom: the parts had a perfectly healthy appearance.

To ascertain the effects of medicines in diseases of the constitution, many experiments, under various circumstances, are necessary; but in matters determinable by a mechanical operation, the effect, as far as our senses can direct us, is in general very plain and explicable.

In the case related this is clear, namely, that previously to the injecting of

* Mr. B. has described the instrument as it was used; but he had since thought, that it would not only be more simple, but do as well, without a valvular apparatus, the quicksilver being poured in by an assistant. See pl. 7, fig. 1, where a is the steel pipe, b the glass tube, and c the funnel.

quicksilver, the tears, sebaceous matter, and mucus, did not pass through the nasal duct, or, but in a very small proportion to the quantity secreted; that at the first experiment, quicksilver did not pass; but that quicksilver, tears, &c., had since readily passed. Mr. B. could not, however, flatter himself that this method would avail, except in the first or simple stage of the disorder; but many cases have a favourable state for the trial in their early period, and that opportunity might be seized with a probability of success. The operation is simple, easily executed, productive of but little pain, and attended with no kind of danger.

XIV. A Continuation of a Meteorological Diary, kept at Fort St. George, on the Coast of Coromandel. By Mr. Wm. Roxburgh, Assistant-Surgeon to the Hospital at the said Fort. p. 246.

This is a meteorological register, mostly at 3 different hours in every day, from March 1, 1777, till May 31, 1778; viz. of the barometer, thermometers, the rain, the winds, and the weather; and a summary of the whole is contained in the following table, viz.

A Table of the greatest, least, and mean Heights of the Barometer, and out-doors Thermometer, in each Month, from March 1777 to May 1778.

Month.	Thermometer.			Barometer.		
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.
1777.				Inches.	Inches.	Inches.
March . . .	87°	67°	77°	30.03	29.18	30.00 $\frac{1}{2}$
April	88	73	80 $\frac{1}{2}$	30.00	29.17	29.18 $\frac{1}{2}$
May	102	76	88 $\frac{1}{2}$	30.00	29.15	29.17 $\frac{1}{2}$
June	100	79	89 $\frac{1}{2}$	29.18	29.14	29.16
July	99	75	87	29.19	29.15	29.17
August . . .	98	75	86 $\frac{1}{2}$	30.00	29.17	29.18 $\frac{1}{2}$
September.	91	76	83 $\frac{1}{2}$	29.19	29.16	29.17 $\frac{1}{2}$
October . .	88	76	82	30.02	29.16	29.19
November	85	65	75	30.04	29.17	30.00 $\frac{1}{2}$
December .	87	66	76 $\frac{1}{2}$	30.04	29.19	30.01 $\frac{1}{2}$
1778.						
January . .	82	64	73	30.04	30.00	30.02
February . .	86	65	76 $\frac{1}{2}$	30.04	30.00	30.02
March . . .	89	69	79	30.02	29.17	30.00 $\frac{1}{2}$
April	94	73	83 $\frac{1}{2}$	30.02	29.16	29.19
May	104	77	90 $\frac{1}{2}$	29.19	29.15	29.17
Mean of all	82			29.51		

XV. A Journal of the Weather at Montreal. By Mr. Barr, Purveyor to his Majesty's Hospitals in Canada. p. 272.

This is a journal of the thermometer, morning and evening, with the account of the snow, rain, winds, and remarks, from Dec. 1, 1778, till April 15, 1779. The highest rise of the thermometer was on the evening of April 15, when it

was at 50° ; and the lowest was -22° , or 22° below zero, viz. on Jan. 19, on which day 16 or 17 German soldiers were frozen to death in crossing the ice on the Lake St. Pierre, a lake near Trois Rivières; and about double the number were frost-bitten, many of whom lost their feet and hands.

XVI. Meteorological Journal kept at the House of the Royal Society. By order of the President and Council. p. 279.

Of this usual journal of the thermometers, barometer, rain, winds, and weather, kept at Somerset-Place, for the year 1779, the synopsis is contained in the following table.

1779.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Inches.
January . . .	50.0	25.0	37.2	48.5	31.5	38.2	30.61	29.63	30.24	0.216
February ..	60.5	37.0	47.9	53.5	44.0	48.8	30.53	29.84	30.26	0.239
March	73.0	33.0	49.2	56.0	35.0	48.1	30.63	29.63	30.19	0.523
April.	78.0	38.0	53.8	64.0	47.0	54.8	30.59	29.43	29.96	1.224
May	82.5	41.5	58.4	79.0	45.0	59.4	30.27	29.51	29.89	1.708
June	75.0	52.5	61.7	69.0	51.5	64.0	30.22	29.48	29.92	2.476
July	85.0	57.0	68.0	81.0	63.0	69.5	30.42	29.30	29.91	6.401
August	84.5	55.0	67.4	79.0	62.0	69.4	30.29	29.64	30.05	1.074
September	79.0	52.0	63.4	78.0	58.0	65.3	30.20	29.54	29.87	2.232
October ...	64.5	36.0	54.2	68.0	43.5	56.8	30.42	29.54	29.96	3.031
November	69.5	29.0	44.2	65.5	35.5	45.5	30.39	28.75	29.62	2.778
December	58.0	20.0	42.1	58.5	28.0	42.0	30.33	28.85	29.50	4.883
Whole Year			53.9			55.1			29.43	26.785

The mean variation for the year was $22^{\circ} 4' \frac{1}{2}$.

And the mean dip of the needle $72^{\circ} 21'$.

XVII. Theorems for Computing Logarithms. By the Rev. J. Hellins. p. 307.

After some introductory remarks on the utility of logarithms, Mr. H. adds, the following theorems, inferior to none as to convergency, are useful in deducing the logarithms of great fractions from those of small ones, or the logarithms of small numbers from those of great ones.

Theor. 1. The log. of $\frac{p+q}{p} = 2 \times \log.$ of $\frac{2p+2q}{2p+q} + \log.$ of $\frac{(2p+q)^2}{(2p+q)^2 - qq}$.

Cor. If $q = 1$, and we write n for p , the theorem becomes $\log. \frac{n+1}{n} = 2 \log. \frac{2n+2}{2n+1} + \log. \frac{(2n+1)^2}{(2n+1)^2 - 1}$, which expression perhaps is of more frequent use than that above.

Theor. 2. $\log. \frac{p+q}{p} = 2 \log. \frac{2p+q}{2p} - \log. \frac{(2p+q)^2}{(2p+q)^2 - qq}$.

Corol. Putting $q = 1$, and $n = p$, as before, we have

$$\log. \frac{n+1}{n} = 2 \log. \frac{2n+1}{2n} - \log. \frac{(2n+1)^2}{(2n+1)^2 - 1}.$$

These theorems Mr. H. demonstrates, and then he adds some examples in numbers, to show their utility in calculating the logarithms of several particular numbers.

XVII. On the Knowledge necessary for Judging of what New Kind of Sugar-Cane Mill can be proposed. By M. Cazaud, F. R. S. From the French. p. 318.

The first sugar mills, Mr. C. says, have been constructed without principles; and that few or no essential changes have been made. He says the common horse mills have their diameter from 45 to 55 feet, suppose 50 at a medium. To this mill is attached a couple of mules, and the bruising cylinder is about 18 inches in diameter; so that the resistance of the canes is at 9 inches distance from the centre of action. It requires, to overcome this resistance, the continued effort of 4 mules, applied to a lever of about 24 feet; which effort he estimated at 600 lb. or 150 lb. for each mule. In 24 feet there are 32 times 9 inches, or 32 times 600 lb.; so that the resistance of the canes is equal to about 19000 lb. in such a mill.

If it were only required to overcome (no matter in what time) this resistance of 19000 lb. we might conceive it to be by applying the continued effort of 2 men, estimated at about 50 lb. at the extremity of a lever of 388 feet, and then we should have a product equal to that of the 4 mules of the common mill; but then we should only obtain in 12 hours what we have in one only with the 4 mules.

It must be known also that the common mill, for the use of which it is necessary to devote at least 36 mules, gives only, one hour with another, but above 80 or 100 gallons of liquor; and that a good water mill, such as is necessary to make 250 or 300 barrels of sugar in the good seasons, ought to give 160 or 200 gallons per hour, on an average. It must also be known that, to give 160 gallons of liquor in the dry time of March or April, the cylinders which press the canes ought to turn $2\frac{1}{2}$ times in a minute, the same as to give 200 in the month of January.

It must be remarked also, that the difference in the product of the 2 mills above-mentioned, supposing necessarily an equal difference either in the resistances overcome, or in the times employed in overcoming them; the resistance which would be to overcome in a new mill, which should be equivalent to an excellent water mill, ought to be supposed of about 38000 lb. or the moving power to overcome it, ought to run through in 1 hour, the space which the mules, in the common mill, run through in 2.

XIX. On an Ossification of the Thoracic Duct. By Richard Browne Cheston, Surgeon to the Infirmary at Gloucester. p. 323.

James Jones, 22 years of age, was admitted into the Gloucester Infirmary, June the 5th, 1779, for very troublesome pains in his back and hip, which, from every circumstance of his description, were supposed rheumatic. On a particular examination a few days afterwards, the right hip was observed to be fuller than it should be; but the thigh seemed very little altered from its natural state. A blister was applied over the joint, and the usual anti-rheumatic remedies were prescribed by the physician under whose care he was admitted. In about a fortnight he complained of a violent pain in his knee, for which another blister was ordered to the head of the fibula. During this time he could move about the ward by the assistance of a stick, but soon after not without crutches.

His thigh now increased in bulk, and became œdematous; for which reason another blister was applied about the middle of it, and his knee getting into a contracted state, a volatile liniment was rubbed on that part. As he now could not move about, even with the assistance of crutches, he took to his bed altogether. From this time the enlargement of his thigh advanced very fast; and his knee became contracted in proportion; so that the thigh had got into the same kind of relaxed position the limb is frequently placed in when the bone is fractured. Soon afterwards he began to find some difficulty in the discharge of his urine, which by degrees increased so much, that the medicines prescribed for his relief in this particular, not having the desired effect, and his belly appearing to be distended from this cause, a catheter was attempted to be introduced, but it could not be made to pass the neck of the bladder: a bougie however entered the bladder with ease, and some water came off on withdrawing it.

From this period, by means of very great exertions of the abdominal muscles, and by occasional pressure externally, he used to discharge his urine; but it came away in small quantities only at a time, and seemed to empty the bladder but very little; for a tumour, which for many days had been perceptible on the left side, and which evidently contained a fluid, afforded the usual feel of a distended bladder. He now suffered very violent pains all over the abdomen, but particularly about the region of the pubes; so that he could not bear the pressure usually made for some time past to force off his urine; and, as the catheter could not enter the bladder, and the introduction of the bougie was of very little service, his urine now began to dribble away involuntarily. His fever, at intervals, was very considerable, his strength failed him very fast, and he received no benefit from any medicines but opiates. In this most deplorable condition he languished till the beginning of October, when the violence of his pains began to remit, and he gradually drooped into a state of insensibility till the 10th of October, when he died.

Dissection.—The integuments of the abdomen felt harsh and dry, like a piece of parchment. The veins were much enlarged, and their branches could be traced all over its surface in a very distinct manner. On the left side there still remained an evident fulness, which pushed the integuments forwards to the size of 2 fists, and which contained a fluid in considerable quantity. The thigh still continued in the same position and œdematous state before described, so that from those circumstances, and the distension of the abdomen, nothing particular could be observed externally, at the lower part of the abdomen, on the right side more than on the left.

On examining the cavity, the intestines presented themselves nearly in their natural situation: their appearance was sound, but in general they were much inflated. The tumor on the left side proved to be the bladder distended with urine, slightly adhering to the peritoneum: and this, together with the colon passing on to its termination in the rectum, filled up the iliac region on that side, while the right side, and indeed more than half the pelvis, was fully occupied by a confused irregular mass, seemingly formed of scirrhus cartilage, bone, and stone. As a large cartilaginous substance* arising from this mass seemed to cover the bodies of the vertebræ, Mr. C. removed the intestines, and pursuing this singular appearance, traced it upwards in the course of the spine, and of the large blood vessels, to its termination somewhat above the kidneys.

On laying bare the sternum and ribs, to inspect the cavity of the thorax, the cartilages presented themselves in the whitest state Mr. C. ever saw, approaching nearly to the colour of writing paper, still retaining their natural firmness and texture. The lungs were in a full state of distension, and studded in many parts with the same kind of cartilaginous substance which appeared so plentifully in the lower cavity.

Suspecting that the arterial system might, in some measure, be affected by this prevailing disease, Mr. C. separated the heart from the lungs, for the purpose of examining its larger system of vessels, and, dividing the aorta just below its curvature, found, on examination, the heart very flaccid, empty, and of a much smaller size than usual in adults of the age of this patient, but sound in every respect. The semi-lunar and mitral valves were not at all diseased, nor was there the smallest deviation in any part of the aorta from the most healthy state, though it was entirely surrounded by this singular substance from the passing off of the cœliac artery to where it bifurcates into the two iliacs.

On raising the aorta from the spine, after he had divided it at its curvature, he found a singular firmness on its right side, like a piece of hard pack-thread, and exactly in the situation of the thoracic duct. He continued his dissection there-

* This substance, when first taken out of the body, appeared cartilaginous; but when dried was perfect bone.—Orig.

fore with great caution, and at such an extent as to take in the vena azygos, and afterwards found, on clearing it at his leisure, that it really was the thoracic duct, entirely plugged up with ossific matter, from immediately above the receptaculum chyli.

Unfortunately, being much straitened for time when he opened the body, he was obliged to take out the parts for a more careful examination at home, which deprived him of the opportunity of ascertaining the above circumstances at the time he separated the parts, and consequently of inquiring how much farther up this singular ossification extended, and in what state the duct might be at its entrance into the subclavian vein. The vena azygos, as well as the aorta, was perfectly sound. The vena cava was not so free from disease; for though it bore externally a natural appearance, when he laid it open from finding a singular feel within, he found that its cavity was above half filled with a firm inelastic substance: this substance originated from its internal surface near the entrance of the emulgent vein, attached to it here and there by small points, till about the projection of the sacrum, where the cavity of the vein was almost filled up with a continuation of the same substance.

The spleen, pancreas, and liver, were perfectly sound: the gall-bladder very small, and quite empty; its ducts in a natural state. The kidneys were much increased in their substance, externally more livid than usual, and seemingly in a state of inflammation. The ureter on the right side was much enlarged, and contained a considerable quantity of urine, which seemed retained there by the distension of the bladder. The left ureter was of its natural size and appearance. The coats of the bladder were considerably thickened, but preserving externally as well as internally its most healthy appearance. As the bladder could not expand itself laterally, it was extended upwards in an oblong form, not unlike that of a calf's, but did not appear capable of containing more than a quart.

The tumor which occupied the right iliac region, extended itself irregularly in all directions, and appeared outwardly to have destroyed or brought on absorption of a principal part of the os innominatum, so far as this could be ascertained by thrusting a probe into it in different directions. The manner in which a portion of this tumor pressed on the neck of the bladder readily accounts for the difficulty of passing the catheter, though a bougie easily slipped in, and shows the reason why the patient was unable to empty his bladder for so long a time before his death. The degree of injury the os innominatum has sustained could not then be certified, the tumor not being sufficiently reduced by maceration. Where the bone was cleared from the surrounding soft parts, it appeared to have suffered a great loss of substance, and, as the tumor dissolved, a large quantity of bony matter, deprived of its connecting medium, was continually subsiding to the bottom of the vessel in which the tumor was macerating.

The preparation of the thoracic duct was at first put up in spirits to preserve its original appearance, and in this state Mr. C. took it with him to London. On showing it there to several anatomical gentlemen, they were in doubt whether there might not be yet remaining some small cavity in the duct, and were therefore desirous he should take the preparation out of the spirits to be examined more accurately. This he readily complied with, and it was accordingly examined very circumstantially by Dr. Hunter, Mr. Watson, Mr. J. Hunter, and Mr. Cruikshanks. The appearances which these gentlemen particularly noticed were, that the duct was completely filled up, excepting at the lower bulbous part, commonly called the receptaculum chyli, where, indeed, there was room enough for air to pass between the coat of the duct and the adventitious substance within it; so that the receptacle, which before appeared flat, on throwing in air became rounded and fully distended: but this air was totally confined to the receptacle, and could not be forced up the duct in the smallest degree. The receptacle was then slit open, and an attempt made to pass a bristle up the duct, but this was found impossible. Mr. Cruikshanks afterwards endeavoured to force mercury up the duct, but not the smallest particle would pass.

From these different examinations, (says Mr. C.), we were all thoroughly convinced, that the receptaculum chyli was not so completely filled up but that it might receive a small quantity of fluid, yet the duct itself was totally impervious, without a possibility of admitting any. The coats of the duct did not appear to have undergone any morbid change: for in some places, where the substance it contained was not so strongly attached but that the coat would admit of being raised from it, they were found in a perfect natural condition. At other places, where the attachment was inseparable, there was a greater appearance of ossification externally; but this we were convinced arose merely from the thinness of the coats in that part, where the receptaculum chyli was laid open; the substance within it appeared of a membranous nature very similar to that found in the vena cava of this same subject, but more laminated. We presumed, that the same kind of membrane had been continued through the whole of the duct, but was now become pretty completely ossified in all that portion of the duct which we were in possession of. A small body, resembling a lymphatic gland on the side of the upper part of the duct, was opened by Mr. J. Hunter, who found the same ossific disposition in this little body, as we before noticed in the duct itself.

We next examined the substance that partly filled up the vena cava. It was in length about 4 inches; at the upper end, broad and conical; at the lower, much narrower and rather rounded. Its surface appeared irregular and granulated with small bony particles. It appeared flattened, perhaps from the pressure of the blood constantly moving over it; for having made a small opening with a lancet

into the narrow part of this substance, we could pass a probe very readily both upwards and downwards, so as to be convinced, it was really hollow all the way. On introducing a blow-pipe, the upper part was expanded into a large cul de sac and the lower distended pretty much like a large vessel. The appearance of this man during his illness, as well as at the time of his death, was exactly similar to what Mr. C. had frequently observed in patients who had lingered under, and been destroyed by, slow inflammations of the viscera. His complaints in the abdomen only indicated a diseased bladder, and for that reason Mr. C. opened him: for he was not even so much emaciated as patients under that disease often are.

XX. On the Effect of Electricity in Shortening Wires. By Mr. Edward Nairne, F. R. S. p. 334.

Exper. Mr. N. took a piece of hard-drawn iron wire, 10 inches long, and $\frac{1}{10}$ of an inch in diameter. This wire was held in a perpendicular position between two brass pincers, the upper one being connected with a glass pillar, that the whole charge of an electrical battery might pass only through the wire fastened between them. These upper pincers were moveable, for the sake of slackening the wire occasionally; and in the experiment they were fixed with a screw, so that the wire hung somewhat loose between them and the lower ones. He then charged a battery, containing 26 feet of coated surface, till the index of the electrometer was raised to 50° : it was then discharged through the wire, and immediately after the wire was seen to shorten by its drawing nearer to a straight line between the fixed pincers. If the wire was put so loose that the moving of the upper pincers $\frac{3}{40}$ of an inch would draw it just straight, one discharge of the battery through it would then draw it to a straight line. Mr. N. discharged the same battery 9 times through a piece of the same wire, which was also of the same length, which was slackened each time before the discharge went through it. After the 6th and 9th time the wire was measured, and found to have shortened in the proportion of $\frac{3}{40}$ of an inch each time.

Mr. N. afterwards discharged the battery 6 times more through the same piece of wire, which made 15 times in the whole, and found it had continued shortening nearly in the same proportion, the wire having been shortened by the 15 strokes full one inch and $\frac{1}{10}$, viz. its whole length being now barely $8\frac{9}{10}$ inches, instead of 10 inches that it was at first. He then weighed it in a pair of scales that would turn with less than the 8th part of a grain; but could not perceive, that there was any sensible difference in the weight. He tried it with a pair of callipers, and it seemed to be rather thicker. He intended to have passed several more discharges of the battery through the wire, but the 16th melted it.

Dr. Priestley took a piece of the same wire, of exactly the same length, and heated it red-hot in the common culinary fire. This wire, being afterwards measured, was found to continue of its original length of 10 inches, not being in the least shortened. Mr. N. generally found, that if the iron wire was first annealed in the culinary fire, the same strength of charge melted it.

Mr. N. also tried a piece of copper wire, gilt with silver, and of the same dimensions as the iron wire, and found that the same charge, discharged through it, shortens it $\frac{1}{10}$ of an inch, viz. $\frac{2}{3}$ of what the iron wire was shortened. The same strength of electrical charge, as near as could be measured by an electrometer, which passing through the iron wire, made it visibly red-hot in a bright day, the sun shining at the same time, did not heat a piece of copper wire of the same dimensions so as to appear of a red heat, though the room was made dark. If the battery was but a little more charged, the iron wire then would be melted; but it had no such effect on the copper wire. This seems to point out that iron wire resists the passage of the electric fluid much more than copper; and also that the culinary fire and electrical fire have different effects on iron and copper: for malleable iron, it is said, is one of the most difficult metals to melt by the culinary fire, and requires a much greater heat to melt than copper; whereas, on the contrary, the iron is melted with a much less charge of electric fire.

XXI. Astronomical Observations on the Periodical Star in Collo Ceti. By Mr. William Herschel,† of Bath. p. 338.*

This remarkable star, we are told,‡ “ was first observed by David Fabricius, the 13th of August, 1596, who called it the *Stella Mira*, or wonderful star; which has been since found to appear and disappear, periodically, 7 times in 6 years, continuing in the greatest lustre for 15 days together, and is never quite extinguished.”

My own observations, says Mr. H., on this wonderful star, are but few, yet sufficiently verify the surprizing appearances that have been ascribed to it. They are as follow.

October 20, 1777, I looked out for it, but it was not visible. If its period is 312 days, it may be expected to be seen about Christmas. Dec. 18, 1777, I saw the periodical star in collo Ceti. It was in magnitude about equal to ζ , but not so large as δ .§ Jan. 26, 1778, the periodical star was larger than δ , but less than ν .

* Bayer's character for this star is ϵ .

† See some authentic memoirs of this extraordinary character, in the *European Magazine* for January 1785.

‡ See Ferguson's *Astronomy*, § 366.

§ ζ is marked by Bayer as a star of the fourth magnitude, and δ of the third.

Sept. 18, 1779, the periodical star was visible to the naked eye, when I first looked for it. October 6, 1779, the periodical star was exceedingly bright this evening. It exceeded α and β Ceti; which latter is considerably larger than the former, and affords a proof of the change in the magnitude of the fixed stars; as we can hardly suppose Bayer should have made a mistake in the magnitude of the first two stars of this constellation. The apparent magnitude of \circ Ceti was not round, but elliptical, when observed through the telescope, and not very well defined; but as it was too near the horizon, this shape might arise from that cause, for other low stars were also irregular in their forms, yet Bellatrix was exceedingly fine and quite round.

October 7, 1779, the periodical star was perfectly round in the telescopes, and its apparent diameter well defined, full, and very large, for a star of that magnitude. When I speak of the apparent diameter, I would be understood to distinguish it not only from the real diameter, but also (if I may be allowed the expression) from the real apparent diameter. To explain this a little more at large: the body of the sun, for instance, is of a certain dimension, which we call his real diameter, and this remains always the same. His apparent diameter (which I here call real apparent) is changeable, according as we approach to, or recede from him, and is between $31' 33''$ and $32' 39''$; but were he removed to the distance of one of the nearest fixed stars, neither his real, nor real apparent diameter, could then be known to us by any method we have hitherto been acquainted with: for at the distance of at least 20 billions of miles, his real apparent diameter could not much exceed $30'''$ of a degree; and a telescope must magnify above 14000 times to make him appear of only 2 minutes in diameter, which still is hardly sufficiently large to distinguish a square from a circle: and yet I doubt not, but that we should observe some apparent diameter or other of the sun thus removed from us; and this is what I here have called the apparent diameter. This must be owing to some optical deception. De La Lande explains it thus: “ Si l'on voit dans les lunettes une lumière éparsse qui environne les étoiles, qui les amplifie et les fait paroître comme si elles avoient 5 à 6" de diametre, on doit attribuer cette apparence à la vivacité de leur lumière, à l'air environnant et illuminé, à l'aberration des verres, à l'impression trop vive qui se fait sur la rétine.”

October 19, 1779, the periodical star preceded a very obscure telescopic star at the distance of $1' 45''.16$, or $1' 50''.47$. At 12 o'clock, the periodical star is now about the meridian, and brighter than α Arietis.

October 30, 1779, the periodical star is still increased, and visibly larger than α Arietis, though not in the meridian at present.

—— o'clock, \circ Ceti being now in the meridian, is almost of a middle size between Aldebaran and α Arietis. Its apparent diameter by the telescope is also

increased. Nov. 2, 1779, the lustre of the periodical star is still increased. The body is very full and round in the telescope. I magnified it 449 times very distinctly, the evening being so fine; but my usual power is only 222. Nov. 20, 1779, the periodical star seemed to be as bright as before, but no brighter. Nov. 30, 1779, \circ Ceti is considerably decreased. Its magnitude is less than β but greater than α . December 4, 1779, the lustre of \circ Ceti is only equal to α . I measured the distance of this star from the obscure one which follows it: first measure $1' 53''.437$; second measure $1' 50''.625$.

The weather was so cold, that I could hardly finish this last measure; but I believe it to be too small. The disagreement is owing to the difficulty, and not to want of accuracy in the micrometer. January 4, 1780, the periodical star is very much diminished. Feb. 7, 1780, the periodical star was invisible to the naked eye.

Maupertuis accounts for the periodical appearances of changeable stars, by supposing that they may be of a flat form, like Saturn's Ring, which becomes invisible when the edge is presented to us. As the periodical star in collo Ceti appeared always full and round when I viewed it with a telescope, this might at first appear to contradict the supposition of Maupertuis; but, on proper consideration, will be found not to be at all against it: for suppose the real apparent diameter of this star to be $\frac{1}{3}$ of a degree; then since it appeared to me at least of $1''$, when at the full, it will follow, that there was an aberration, whatever might be the cause of it, which amounted to $59'''$, by which its real apparent diameter was increased from $1'''$ to $1''$. Now if this star, in one certain position, should present its circular disc of $1'''$ in diameter, and in another situation only its flat edge which would appear as a line of $1'''$ in length, both appearances, with the aberration included, will still remain of a circular form: for adding the aberration $59'''$ to the length $1'''$, the whole becomes $1''$; and adding $59'''$ to scarce any breadth at all, the whole breadth will also become nearly $1''$.

Keill says, "It is probable, that the greatest part of this star is covered with spots and dark bodies, some part thereof remaining lucid; and while it turns about its axis, sometimes shows its bright part, sometimes it turns its dark side to us; but the very spots themselves of this star are liable to changes, for it does not every year appear with the same lustre. Sometimes it resembles a star of the 2d magnitude; in other years it can scarcely be reckoned among stars of the 3d order; nor are the times of its visiting us always of the same duration."

XXII. An Account of a new and cheap Method of preparing Pot-ash, with Observations. By Thomas Percival, F. R. S., and A. S. p. 345.

Reprinted in Dr. P.'s collected works, edited by his son.

XXIII. On the Degree of Salubrity of the common Air at Sea, compared with

that of the Sea-shore, and that of Places far removed from the Sea. In a Letter from John Ingenhousz, M. D., F. R. S., to Sir John Pringle, Bart., F. R. S. Dated Paris, January 22, 1780. p. 354.

SIR,—As you had recommended to me the examination of the air at sea by the nitrous test, I followed your advice in my return to the Continent in the beginning of November last; and I embraced that opportunity with the more eagerness, as I knew that you had given credit to the account of several consumptive people having recovered their health by going on sea voyages, after the common means for curing that distemper had failed. I was in hopes also to find in this inquiry, a confirmation of what you conjectured in your anniversary discourse in the year 1773, viz. that great bodies of water, such as seas and lakes, are conducive to the health of animals, by purifying and cleansing the air contaminated by their breathing in it: so that the salutary gales, by which this infected air is conveyed to the waters, and by them returned again to the land, though they rise now and then to storms and hurricanes, must nevertheless induce us to trace and to revere in them the ways of a beneficent Being, who, not fortuitously, but with design, not in wrath, but in mercy, thus shakes the waters and the air together, to bury in the deep those pestilential effluvia which the vegetables on the face of the earth are insufficient to consume. I was not without hope, that such experiments might tend to throw new light on the cause of that almost universal effect of the sea air, to wit, its increasing the powers of life, and giving a keener appetite by hastening the digestion of food.

I shall now give you an account of the experiments I made in consequence of your suggestion, in the same order as they were made; and beg you to present them to the R. S., if you think them worthy the attention of that learned body. I must first however premise, that as I wrote this paper in noisy inns, on ship-board, and places little adapted to philosophical application, I hope you will make some allowance for the inaccuracies which you may find in it. I began my experiments at Gravesend, where I was obliged to wait 2 days for a favourable wind. I found the air of that place, on November 1, of a tolerable good quality, as one measure of it with one of nitrous air occupied, in several trials, about 104, or one measure and $\frac{4}{100}$ of a measure; so that I took it to be nearly of the same quality as the air of London.

The ship in which I went from London to Ostend happened to be becalmed about 2 or 3 miles from shore, in the mouth of the Thames, between Sheerness and Margate. The weather was very agreeable, warm, and the sun shone very bright, on the 3d of November. I was provided with a travelling apparatus, made on purpose by Mr. Martin, the whole of which was packed up in a box about 10 inches long, 5 broad, and $3\frac{1}{4}$ high. The glass tube or great measure, which was 16 inches long, and divided in 2 separate pieces, lay in a small com-

pass, and could be put together by brass screws adapted to the divided extremities. Instead of a water trough, such as is used commonly, I made use of a small round wooden tub, which I found on board the ship, and which I filled with sea-water, fixing to the edge of this tub, by means of a screw, the brass funnel, through which the air was to be let up into the glass tube.

After having exercised myself during some time in performing the experiment in a water tub much too small for the purpose, I at last acquired a habit of doing it tolerably well. I then began to make my experiments regularly at about 11 o'clock; and I found the sea air at the place indicated of a superior purity to any common air I ever met with since the month of June last (when I began to engage in the course of experiments which have afforded me the materials of my work lately published on Vegetables) either in my country retirement, or in London. In 6 different trials made one after another in the short manner described in my book, p. 278, et seq.* I found, that the 2 measures of air (one of common and one of nitrous air) occupied from 0.91 to 0.94; which difference in the result, though but small in itself, was owing to the disadvantage of not having a vessel deep enough to move the glass tube in with ease, for the purpose of mixing the 2 airs together, and to my not having yet acquired the habit of using the portable apparatus.

I tried also the air of this spot in the manner used by Abbé Fontana, which I have described in my book, p. 155;† the result of which trial was, that after the first measure of nitrous air was let up, the column of both airs occupied 1.86, or one measure and $\frac{86}{1000}$ of a measure; after the 2d measure of nitrous air was let up, the bulk of both airs occupied 2.02, or 2 measures and $\frac{20}{1000}$ of a measure; and after the 3d measure of nitrous air was let up, the remaining bulk of both airs occupied 2.96, or $\frac{96}{1000}$ of a measure: so that the remaining bulk of both airs employed in the experiment (viz. 2 measures of common and 3 of nitrous air, each making 100 sub-divisions of the glass tube) amounted to 2 measures and $\frac{96}{1000}$ of a 3d measure, or to 296 sub-divisions, which being subtracted from the 5 measures, or from the 500 sub-divisions employed, the remainder was 2.04, which was exactly the quantity of both airs destroyed.

Give me leave, before I proceed further, to recal to your memory some of the experiments relative to this subject, made in my country retreat in the course of last summer, and related in my book, principally in pp. 155 and 282, together

* It consists in letting up into the glass tube one measure of common air, and after this one measure of nitrous air, and shaking the tube forcibly in the water trough just at the moment the two airs come into contact with each other.—Orig.

† It consists in letting up into the divided glass tube 2 measures of common air, and afterwards 3 measures of nitrous air, one after another, shaking the tube in the water trough after each measure of nitrous air, and beginning constantly this motion exactly at the moment the 2 airs come into contact with each other, or even before they meet, which is still better.—Orig.

with some further experiments made just before my setting out for the continent: this recapitulation will make the nature of the experiments just mentioned, and their result, much more easily understood.

The different degrees of salubrity of the atmosphere, as I found it in general in my country house at Southall Green, 10 miles from London, from June to September, lay between 103 and 109, that is, that of the 2 measures of air, viz. 1 of common and 1 of nitrous air, the remaining bulk or column occupied between 103 or 109 sub-divisions in the glass tube. I was somewhat surprized when, on my return to my former lodgings in Pall-Mall Court, I found the common air purer in general in October, than I used to find it in the middle of summer in the country; for on the 22d of October, at 9 in the morning, the weather being fair and frosty, I found, that one measure of common air and one of nitrous air occupied 100 sub-divisions in the glass tube, or exactly one measure. It gave by the Abbé Fontana's method abovementioned the following result, 184, 208, 304; so that the quantity of both airs destroyed in this trial was exactly 1 measure and $\frac{9.6}{100}$ of a measure, or 196 sub-divisions. The same day at 2 o'clock in the afternoon, it being then rainy weather, the air was somewhat altered for the worse; for at that time 1 measure of common and 1 of nitrous air occupied 102.

The next day, October 23, it being rainy weather, the state of the atmosphere was the same in the morning as it was the day before in the afternoon: for 1 measure of common and 1 of nitrous air occupied 102; and the result of Abbé Fontana's method was as follows, 184 $\frac{1}{2}$, 211, 307, so that the quantity of both airs destroyed amounted to 193.

October 24, I again examined the state of the atmosphere at 9 in the morning, the weather being serene; I found it restored to its former goodness; for 1 measure of common and 1 of nitrous air occupied 100, or exactly 1 measure; and the result of Abbé Fontana's method was 184, 207, 304. At 7 in the evening of the same day the air was again worse; for 1 measure of common and 1 of nitrous air occupied 103.

October 25, I examined the air at 11 in the morning, the sky being cloudy, and found, that 1 measure of common and 1 of nitrous air occupied 102. I put it again to the test at 11 at night, and found, from 5 different trials, that 1 measure of it with 1 of nitrous air occupied 105. October 26, the weather being very dark and rainy, 1 measure of common and 1 of nitrous air occupied 105.

Though I have some reason to believe, that the vicinity of the trees at the back of my lodgings, did really contribute somewhat to render the air purer at the abovementioned place; yet I believe also, that the frosty weather of itself contributes to purify the atmosphere, perhaps by checking a great many causes

of corruption of various substances; and that the liveliness we commonly enjoy in frosty weather is in a great measure owing to the superior degree of purity of our common element at that time.

From what is already related it appears, that the difference between the best atmospheric air I have yet found and the sea air, as I found it by the first examination on the spot where chance carried me, is as 91 to 100, the less number indicating the best quality. Now as I found the sea air of such a pure quality so near land, I thought it might, with some degree of probability, be expected, that the common air, at a distance from land, would prove of a still superior quality; for I could hardly believe, that in the first trial, made without choice of place or time, I had just hit on a time and a place where the sea air is of the first quality.

I would have repeated the same experiment next day, November 4, when we were in the middle of the channel between the English coast and Ostend; but the motion of the ship, which was very great, made it impracticable. Not entirely however to lose the opportunity which the voyage afforded me, I filled 3 phials, made on purpose for such use, with air, when we were in the middle of the sea. I kept these bottles shut till next day, November 5, when I examined the air confined in them at Ostend, and found it of an inferior quality to that which I had tried on the 3d of November, in the mouth of the Thames; for 1 measure of it with 1 of nitrous air occupied, in 3 different trials, 097. I found the common air at Ostend near as good the same day at 10 in the morning, the weather being cloudy; for 1 measure of it with 1 of nitrous air occupied 098. In the afternoon, the weather being very rainy, the common air of the place was become worse, though still of a very good quality; for 1 measure of it with 1 of nitrous air occupied 100, or exactly one measure.

About 5 in the evening, the weather continuing rainy, and the wind blowing at that time very hard from the sea, I went to the sea-shore on purpose to gather in a phial, fitted for the purpose, the sea-air just as it blew towards the land. When I had got it I went immediately home, and put it directly to the nitrous test. I found it, by several trials, of such good quality, that it was nearly as good as that which I had met with in the mouth of the Thames; for 1 measure of it with 1 of nitrous air occupied 094 and 095, whereas the common air of the inn was somewhat of an inferior quality, though still remarkably pure; for 1 measure of it with 1 of nitrous occupied 097 in 5 repeated trials.

As the difference in the quality of the sea air examined on the spot in the mouth of the Thames, Nov. 3, and that which I gathered in the middle of the sea in rainy and windy weather, was so remarkable, I suspected the reason of this difference to be, that the air, put to the test Nov. 3d, had been exposed during several days to the influence of the sea without any mixture of land air, as it

had been remarkably calm all that time; and that the air gathered on the sea in windy weather was mixed with air driven from the land by the wind, and incorporated with the sea air. This suspicion was afterwards strengthened when I found the air gathered at the sea shore, on the evening of Nov. 5, near as good as that which I gathered on a fair day in the mouth of the Thames, Nov. 3; for the wind being N. W. the air driven on the coast was to be considered as true sea air, without any mixture of land air.

But after making up my mind about the difference of the above experiments, a doubt rose about a circumstance to which this difference might have been owing, at least in some measure; the circumstance I mean was this: I had made use of sea-water for the experiment on Nov. 3, whereas I had made use of pump-water in examining the sea air kept in bottles at Ostend. I thought I had no right to draw any conclusion from the fact till I was convinced that the making this experiment in common or in sea-water would make no difference in the result. This consideration made me stay one day longer at Ostend, on purpose to satisfy my mind on this head, lest I should never find another opportunity of doing it. I immediately ordered a pail full of sea-water to be brought to my lodging, and made several comparative trials with atmospheric air in common water and in this sea-water; but I could not observe any real difference in the result. Thus all degree of suspicion about the difference of the result from this cause was now at an end.

There now only remained some little suspicion that the air gathered on the sea, and kept in phials, might have undergone some alteration; this might have been the case, as I had found in some former experiments made in England, that air kept in bottles was sometimes liable to alterations, which I think is partly to be ascribed to the difficulty of finding bottles so well secured by a ground stopper as to shut out every communication with the external air, and partly perhaps to the nature of air, which is not in itself an unalterable substance, as I have attempted to prove in my book on Vegetables, p. 107: in the present case however, I rather incline to think, that the reason why the air, gathered in rainy and windy weather on the sea, was found of an inferior quality, is to be ascribed chiefly to the land air being driven by the wind on the sea, and thus to the mixture of both airs.

Nov. 6, about 9 in the morning, the weather being cold, windy, and cloudy, I again put the common air of Ostend, which I gathered in my inn, to the test, and found it of near as good a quality as that in the mouth of the Thames; for 1 measure of it with 1 of nitrous air occupied $0.94\frac{1}{2}$ in 3 repeated trials. The same morning, about 11, the wind blowing very hard from the sea, I went to the shore on purpose to gather some air just as it came from the sea. I found its quality inferior to that which I had examined 2 hours before, though still su-

perior to any air I have yet found in England; for 1 measure of it with 1 of nitrous air occupied 097. The common air, as I found it in my lodging, was at 098. The wind had not shifted much, though I cannot ascertain the exact point from which it then blew. It seems probable, from the foregoing experiments, that though in general the sea air surpasses the land air in purity, yet there are the same inconstancies in its degree of goodness as in the land air.

This experiment being finished, I closed my inquiries at Ostend, and set off for Bruges. We arrived at dark, and about 7 in the evening I tried the common air of that place, and found it inferior in purity to that of Ostend in more than 10 experiments; 1 measure of it with 1 of nitrous air occupied about 105. I had the mortification to find the stoppers of the phials, in which I had kept the air of Ostend, all loosened, so that I could not make any comparative trial with both airs. Nov. 8, I set out for Ghent, where I spent the next day, Nov. 9. I tried the air of that place about 3 in the afternoon, and found it better than that of Bruges; for 1 measure of it with 1 of nitrous air occupied about 103, in several experiments.

As all the following trials with common air are made in the same way as the foregoing, viz. by mixing 1 measure of common with 1 of nitrous air, I will hereafter only mention the numbers of the result, for the sake of abridging the paper, by avoiding continual repetitions. Nov. 12, at Brussels, I found the air, at 7 o'clock in the evening, at $105\frac{1}{4}$. Nov. 13, the air of the lower part of the city at 106, that of the highest part at 104; the weather was rainy and damp. It is a common opinion at Brussels, that the air in the lower parts of the city is more unwholesome than in the higher parts. Nov. 14, the air of the lower and higher parts of the city of the same goodness, each being at 103; the weather was fair and frosty. Nov. 15, the air was the same as yesterday in both parts of the city; the weather fair and cold. Nov. 22, I arrived at Antwerp, where I found the air in the evening at $109\frac{1}{2}$; the weather was rainy, damp, and cold. Nov. 23, I set out for Breda. I filled a phial with air, when I set out from Antwerp at 8 in the morning. I also filled a phial with air on the middle of the heath or common called De Lange Hey: the weather was remarkably close and damp. I tried these 2 airs at night, at Breda; that of Antwerp was found at 106; that of Breda the same; that of the heath at $105\frac{1}{4}$. Nov. 25, I examined the air at Breda in the morning, about 11 o'clock, the weather being fair, cold, and inclining to frost; it was at 102. At 7 in the evening it was at 103. Nov. 25, the air at Breda was at 104; the weather rainy and cold. Nov. 26, the air was in the morning and in the evening 103; the weather very rainy, cold, and stormy.

Nov. 27, I set out from Breda for Rotterdam, and crossed the water at the Moordyke. I tried the air at the Moordyke close to the water, and found it at:

101 $\frac{1}{2}$; the weather was fair and cold, but not frosty. This spot is reckoned very healthy; the inhabitants have a sound look, and generally live to a great age. Nov. 28, the air at Rotterdam was at 103; the weather rainy and cold.

Nov. 29, being at Delph, I gathered some air in the middle of the day, the weather being stormy and rainy. I examined it next day at the Hague, and found it at 103; and that of the Hague 104. Nov. 30, the air at the Hague was at 104; the weather cold, the wind northerly.

Dec. 1, being still at the Hague, the weather underwent a sudden and remarkable change. The wind was southerly and stormy; the air was become so warm, that on going out of the house into the street, I felt the same sensation as on going from a cold air into a room heated by a German stove. I suspected that this sudden change would alter the constitution of the atmosphere in point of salubrity. Having no time to make any experiment, I contented myself with filling some phials with this air, and sending my servant to Schevelingen to gather some air close to the sea. Dec. 2, the wind and weather remained the same as yesterday. In the evening, about 8 o'clock, when Fahrenheit's thermometer stood at 54°, I put the common air to the nitrous test, and found it at 116; the air gathered the day before at 117: and that gathered close to the sea at 115. As I had never found the common air near so bad, I had some apprehension that my eudiometer was out of order, or that something was the matter with the nitrous air. I made therefore fresh nitrous air, and repeated the experiment many times, but the result was nearly the same. In the mean time, I had the following accidental meeting. The father of the landlady of the house having been informed by the servant, that I was about some extraordinary pursuit, of which he could have no conception, came to see what I did. He had scarcely been a minute with me but I perceived he laboured under a severe asthma. He explained his case to me, knowing me to be a physician, and told me, that he had passed these 2 days very uncomfortably, finding the air so uncommonly heavy, that he could hardly draw his breath: which convinced me that the element was in reality become of an inferior quality.

Dec. 4, at Amsterdam the air was at 103; the weather being rainy, windy, and cold. Dec. 5, the air was at 102; the weather nearly as yesterday. Dec. 10, I returned to Rotterdam, and found the air at 101; the weather rainy.

In the beginning of last year they made an end of draining a large meer, about half the size of the Haerlemmer Meer, situated in the neighbourhood of Rotterdam, which was turfed out in former ages. It was now laid into arable land and turned out to be very fruitful. When this land was quite cleared of the water, an uncommon epidemical disease broke out in all the places situated on the borders of this lake; it began about August, and abated when the winter season set in. This distemper broke out afresh last summer, and was now again

on the decline. It carried off a great number of inhabitants. It appeared chiefly under the habit of an irregular intermittent, a bilious remitting, and a putrid fever. There was scarcely a single house to be found in which there were not some persons sick. The villages at a quarter of a league distance from the former lake were free from it. This distemper was ascribed to the putrid exhalations of this newly uncovered land; which exhalations were very offensive to the smell. This was so much the more probable, as the disease abated when the stench, checked by the cold, abated. I tried the air of this former lake on the spot, and found it as good as that of Rotterdam; but there was a great deal of wind that day, and no perceptible stench. However, Dr. Bicker, an eminent physician of that city, got me a phial filled with air of this lake, which he took from a spot where he still perceived some of the former bad smell. This air proved to be in reality of an inferior quality to that of the city.

Dec. 12, being in the middle of the water between Dort and the Moordyke, I found the air on what is called Holland's Diep of an inferior quality, the weather being remarkably dark, rainy, and windy; it was at 109. Dec. 13, being returned to Breda, I found the air of that place at 109 in the morning, the weather continuing as it was yesterday. In the afternoon it was somewhat better, viz. at $106\frac{1}{2}$, the weather having cleared up. Dec. 16, having returned to Antwerp, I found the air of the lower part of that city at 105; and that of the higher part at 104, the weather being rainy and temperate. Dec. 17, the air at Antwerp was 107, the weather continuing to be nearly as it was the day before. Dec. 19, being returned to Brussels, I found the air at 109, the weather being rainy, windy, and rather warm. Dec. 21, the air at Brussels at 106, the weather being dry and cold. Dec. 22, the air of Brussels was the same as yesterday; the weather nearly the same also. Dec. 23, I arrived at Mons, and found the air of that place at 104; the weather rainy and cold. Dec. 24, being near Bouchain, I found the air at $104\frac{1}{2}$; the weather cloudy and cold. Dec. 25, I tried the air at Peronne, and found it at $102\frac{1}{4}$; the weather frosty. Dec. 26, being at Cuvilli, a village 4 leagues from Roye, I examined the air of that place, and that which I had gathered on the road about 12 o'clock in the day-time; I found them both at 103; the weather frosty. Dec. 27, the air at Senlis, and that gathered in the middle of the day on the road, were both at $102\frac{1}{2}$; the weather continued frosty. Dec. 29, being at Paris, I found the air in that capital at 103; the weather frosty. Jan. 8, 1780, the air of Paris was at 100; the weather very frosty. Jan. 13, the air of Paris was 98; it froze very hard.

It appears from these experiments, that the air at sea and close to it, is in general purer and fitter for animal life than the air on the land; though it seems to be subject to the same inconstancy in its degree of purity with that of the land; so that we may now with more confidence send our patients, labouring under

consumptive disorders, to the sea, or at least to places situated close to the sea, which have no marshes in their neighbourhood. It seems also probable, that the air will be found in general much purer far from the land than near the shore, the former being never subject to be mixed with land air.

It appears also, that the air in frosty weather is in general wholesomer than it is in winter when it does not freeze; and that uncommon warm weather, happening in the winter season, is apt to render the atmosphere very unwholesome: the reason of which I apprehend to be, that the frost totally checks that general tendency to corruption, which, being revived by warmth again, increases the infection of the common air, which at that time is so much the greater, because the plants, which are deprived of their leaves in winter, have no power in them to counteract it.

It seems also probable, that those countries which are, by their local situation, exposed to noxious exhalations, are in general much wholesomer in the winter; and that it is much safer to cross such countries in summer time when it is windy weather than in a calm, &c. Dr. Damman, an eminent physician and professor royal in midwifery at Ghent, told me, that when he was formerly a practitioner at Ostend, during 7 years, he found the people there remarkably healthy; that nothing was rarer there than to see a patient labouring under a consumption or asthma, a malignant, putrid, or spotted fever; that the disease to which they are the most subject, is a regular intermittent fever in autumn, when sudden transitions from hot to cold weather happen.—People are in general very healthy at Gibraltar, though there are very few trees near that place; which is probably owing to the purity of the air, arising from the sea. Most small islands are very healthy. At Malta people are little subject to diseases, and live to a very advanced age.

XXIV. The Principal Properties of the Engine for Turning Ovals in Wood or Metal, and of the Instrument for Drawing Ovals on Paper, demonstrated. By the Rev. Mr. Ludlam, Vicar of Norton, near Leicester. p. 378.

The instrument for drawing ovals on paper or board is so common, that a particular description of it is needless. It is much in use among the joiners, and called by them the trammels. One part of it consists of a cross with 2 grooves at right angles: the other is a beam carrying 2 pins which slide in those grooves, and also the describing pencil; we shall distinguish these 2 parts by the names of the cross and the beam. It is very well known, that all the engines for turning ovals are constructed on the same principles with the trammels; the only difference is, that in the trammels the board is at rest, and the pencil moves upon it; in the turning engine, the tool, which supplies the place of the pencil, is at rest, and the board moves against it.

Let Aa and Bb (fig. 2, pl. 7,) be 2 indefinite lines, intersecting each other at right angles in c . Let LSM be the beam, or a rigid right line, in which assume 2 fixed points L and s at pleasure. If the fixed point L be kept always sliding on the line Bcb , and the other point s always sliding on the line Aca ; then, any point M in the line LS , or that line produced, will describe an ellipse.

Bisect LS in E , and through c and E draw the indefinite right line CEH . On LS as a diameter, with the centre E , describe a semi-circle, and because Lcs is a right angle, it will pass through c , and $EC = EL$. Through M draw MPH perpendicular to AC , meeting CE produced in H ; and because MH is parallel to CL , the triangles MEH and CEL are similar, and $HE = ME$, and $HE + EC = ME + EL$, or $CH = LM$. The point H therefore always falls in the circumference of the circle $HADA$ described with the centre c and radius $CH = LM$. Now the similar triangles CHP and SMP give $CH : SM :: PH : PM$. But when L arrives at c , then $LM (= CH)$ coincides with CA ; and when s arrives at c , then SM coincides with CB ; therefore $CA : CB :: PH : PM$, and $CA^2 : CB^2 :: PH^2 : PM^2$, or $CA^2 : CB^2 :: AP \times Pa : PM^2$, which is the property of an ellipse, whose first semi-axe is CA or LM , and 2d semi-axe is $CB = SM$.

Produce PM till it meets the circle in N , and draw the radius cn ; then $PH = PN$ and $CA : CB :: PN : PM$. Again, because $PCH = PCN$, therefore $NCD = ECL = ELC$ and cn is parallel to LM , and $CL = NM$. Draw mp perpendicular to Bb , cutting cn in n , and for the like reason $cn = SM = CB$, and $cs = mn$. While the point M describes an oval, the point E describes a circle whose centre is c and radius $CE = \frac{1}{2}SL$.

To the ruler MEL (fig. 3 and 4) fix another ruler or right line MEK passing through E , so that the ruler MEK may be carried about by the ruler MEL , keeping the angle MEM between the 2 rulers invariable. On MEK take $EV = EK$, and each $= ES$ or EL ; then the point v will describe a right line $avc\alpha$ passing through c , and making an angle acs with CA , equal to half MEM the angle made by the 2 rulers; the point K will also describe a right line $hkc\beta$ passing through c , and making an angle hcl , with CL , also equal to half MEM .

On the centre E (fig. 3 and 4) and with the radius EC , describe a circle, and it will pass through the points s, v, c, L, K ; draw the lines vc and Kc , and the angles sev , and scv , both stand on the same arch sv ; the former at the centre E , the latter at the circumference c ; therefore the former is double the latter. In like manner the angles KEL and KCL both stand on the same arch KL , the former at the centre, the latter at the circumference; therefore the former is double the latter. Now as this holds in every position of the rulers during their joint motion, it is manifest that the points v and K will each describe right lines, namely, $ac\alpha$ and $bc\beta$, passing through c , and making the angles aca and bcl ($= bc\beta$) each equal to half MEM .

Hence the lines $ac\alpha$ and $bc\beta$, traced by the points v and k , are at right angles, and the ruler $mvek$ moves exactly in the same manner as if it was guided by the points v and k sliding on the lines $ac\alpha$ and $bc\beta$, at right angles to each other; just in the same manner as the ruler msl is guided by the points s and L , sliding on the lines AC and BC . Therefore if any point m be assumed in the line kvm , as a describing point, the figure described will be an ellipse, the position of whose principal axes are the lines $ac\alpha$ and $bc\beta$; the centre of the ellipse being still in c as before. If mK be taken equal to ML , the ellipse thus described by the point m will be the same with that described by the point M , only in another position: its greater semi-axis ac making an angle with AC , the greater semi-axis of the former ellipse, equal to half MEM , the angle which the rulers or lines ME and mE make with each other.

Scholium.—This proposition is demonstrated in Schooten's Exercitationes, &c. p. 305; but he makes 12 cases of it: had he made use of the 20th of the 3d El. they might have been all comprehended under one. In the turning of ovals, the top of the rest which supports the tool is always made to pass through s and L , (fig. 2,) the two centres round which the oval engine turns; and in this case the ruler or line $msel$ represents the top of the rest. If the tool be held on any part of the rest between the workman and the nearest centre, as at m , an oval will be turned having its longer axis Aa (in one position of the work) coinciding with the top of the rest. As the tool is removed towards s , the oval will grow narrower, and at s become a right line. Beyond s towards E it will grow rounder, and at E become a circle; beyond E it will grow narrower, and at L become a right line at right angles to the right line described when the tool was at s . If the tool be removed beyond L , it will describe an oval again, whose longer axis is at right angles to the longer axis of the oval first described when the tool was at m . It may be very convenient to mark the points s and L , and also their middle point E , on the top or face of the rest that supports the tool. If any thing be interposed between the tool and the top of the rest, so as to raise the tool above the line passing through the centres s and L , an oval will yet be described, whose centre will be the same with that of the oval first described when the tool was at m ; but its principal axis will cross the principal axis of that oval (fig. 3 and 4.) Draw right lines, both from M the old place of the tool, and from m the new place of the tool, to the point E marked on the rest. Half the angle which these two lines make with each other will be the angle which the principal axis of the new oval makes with the principal axis of the old one.

It is well known, that when the oval engine is set in order for working, there is a part which slides back, and is then fixed, which separates the 2 centres of motion, and gives the eccentricity; for the difference between the 1st and 2d semi-axes, will be just as much as the centres are thus separated: call the dis-

tance between the 2 centres E; let now the tool be fixed in any place, upon, above, or below the rest; call mE the distance of the tool from the middle point between the centres (marked E on the rest) D; then the greater semi-axis of the oval so described will be $D + \frac{1}{2}E$, and the lesser semi-axis $D - \frac{1}{2}E$; and thus both the form and position of the oval will be known. All workmen know the tool must never be raised above the place where it was at first held, and we see the reason; it would destroy the oval first begun to be turned, and form a new one in a different position.

But there is another difficulty in turning ovals, especially such as have mouldings, as picture-frames, &c. The tool generally has all the mouldings formed on it: now if it be laid flat upon the rest, and the engine set to work; the mouldings will in some places cross the plane of the tool (or the top of the rest) at right angles (as in turning circles,) in other places obliquely. This will make the several members of the mouldings leaner or smaller in one part of the work than another. Nor will the case be altered if the mouldings be turned separately. Analogous to this, when an oval is drawn by the trammels, the line described by the pencil will not, as in a circle, be always at right angles to the beam of the trammels. The oval line so drawn will be at right angles to the describing beam only at the extremity of the 2 principal axes where the beam coincides with those axes; in all other places the oval line and beam make an oblique angle. It may be proper therefore to inquire how much this angle deviates from a right angle. This we shall call the angle of deviation.

All things as in fig. 2, draw the tangents TM and TN, to the point M in the ellipse and the point N in the circle, corresponding to each other; then from the nature of the ellipse these tangents will meet each other in the axis CA produced. Draw MG perpendicular to TM, then GMS will be the angle of deviation sought. Then the angle MTN, between the tangents to corresponding points in the ellipse and circumscribing circle, is equal to the angle of deviation GMS. For because TNC is a right-angled triangle, and NP perpendicular to TC; therefore $TNP = NCP = MSP$, that is, in the triangles MTN and GMS, the angles TNM and MSG are equal. In like manner, because TMG is a right-angled triangle and MP perpendicular to TG, therefore $TMP = MGP$, and (in the triangle MTN and GMS) the angles TMN and MGS are equal; therefore in the same triangles, the remaining angles MTN and GMS are also equal.

To compute the angle MTN, we have by trigonometry $TP^2 + PM \times PN : TP :: MN : \tan. MTN$, radius being unity. Call now $CA = t$, $CB = c$, $CP = x$, $PM = y$, $CA - CB$ (or $t - c$) = d , and we have $PN = \sqrt{(tt - xx)}$; also $CD : CB :: PN : PM = \sqrt{(tt - xx)} \times \frac{c}{t}$, whence $PM \times PN = (tt - xx) \times \frac{c}{t}$. Again, $CP : PN :: PN : PT$, whence $TP = \frac{tt - xx}{x}$. Lastly, $CD : BD :: PN : MN = \sqrt{(tt - xx)}$,

$\times \frac{d}{t}$; whence the tangent of MTN, the angle sought, is $\frac{dx\sqrt{(tt-xx)}}{ttt-axx}$, and this is a maximum when $\frac{ttt}{2t-d}$, or $\frac{ttt}{t+c} = xx$, or when $\frac{ccc}{t+c} = yy$, or when CP and PM have such a proportion that $CP^2 : PM^2 :: CA^3 : CB^3$.

Let AMBab (fig. 5) be an ellipse whose centre is c; draw the circumscribed and inscribed circles as before; the former cutting the 2d axis produced in D, the latter cutting the first axis in d, and the 2d axis in b. On Db as a diameter describe a circle cutting the first axis Aa in a, draw Da and ba. Set off CR = Da, join DR, and draw DP at right angles cutting the first axis in P, draw PM an ordinate to that axis; then M will be the point in the oval line where the angle of deviation is greatest. Otherwise, on cb produced set off cr = ba, join dr, and draw dp at right angles cutting the 2d axis in p: draw pM an ordinate to that axis, and M will be the point where the angle of deviation is greatest.

At the maximum (when $xx = \frac{ttt}{t+c}$) $PN^2 = \frac{ttc}{t+c}$, $PM^2 = \frac{ccc}{t+c}$, and $TP^2 = \frac{tcc}{t+c}$: whence $TP^2 = PM \times PN$. Also, PN, PM, TP, are to each other as CA, CB, and $\sqrt{(CA \times CB)}$, respectively. Therefore, $\frac{CA - CB}{\sqrt{(Aa \times Bb)}}$ is the tangent of MTN, radius being unity. Also $\sqrt{\frac{CA}{CB}}$ is the tangent of NTP; and MTP is the complement of NTP: therefore MTN is twice the excess of NTP above 45° .

In fig. 5, draw a circle through the points N, M, T; and at the maximum where $TP^2 = PM \times NP$, this circle will touch CA produced in T. From E the centre of this circle draw EF perpendicular to NM, also the radii EN and EM; then FN is the sine of NEF, or half NEM, or of its equal MTN, to the radius EN. But $EN = ET = PF = \frac{PN + PM}{2}$, and $FN = \frac{PN - PM}{2}$. Therefore $PN + PM$ is to $PN - PM$, or $CD + CB$ is to $CD - CB$, or $CA + CB$ is to $CA - CB$, as radius is to the sine of the greatest angle of deviation, which is therefore equal to $\frac{CA - CB}{CA + CB}$, radius being unity.

XXV. Of Cubic Equations and Infinite Series. By Charles Hutton, LL. D., F. R. S. p. 387.

As this tract will be found at large in Dr. Hutton's works collected, the abridgment of it is omitted in this place.

XXVI. Of a most Extraordinary Degree of Cold at Glasgow in January last (1780); with some New Experiments and Observations on the Comparative Temperature of the Hoar-frost and the Air near it, made at the Macfarlane Observatory belonging to the College. By Patrick Wilson, M. A. p. 451.

On Tuesday, Jan. 11, 1780, there was a slight frost, and, on the evening of

that day, a fall of snow to the depth of 12 inches. Next day the cold continued to increase, but so gradually, that at sun-set Fahrenheit's thermometer pointed only to 22° . About midnight, a very accurate thermometer hung out at a high north window, soon after pointed to 6° . At this time the air was very still and serene, and the barometer stood at 30 inches.

Thursday morning, Jan. 13, thermometer pointed as here annexed:

At 6 o'clock this morning Mr. W. carried the thermometer over to the Observatory Park, and there laid it down on the snow, when the mercury sunk to 13° below 0.

At 1 o'clock.	$+6^{\circ}$
$1\frac{3}{4}$	$+6$
$2\frac{1}{2}$	$+4$
2	$+6$
4	$+3$
$4\frac{1}{2}$	$+2$
5	$+2$
$5\frac{1}{2}$	$+0$

At this time he thought it unnecessary to stay abroad so long in the cold as to try the temperature of the air by hanging up the thermometers, especially as he imagined that this had been done more readily, and as truly, by taking the degree from the surface of the snow which had been exposed to the open air during the night: but reflecting afterwards on the snow at the observatory being so much below 0, the greatest cold of the air at the college, and having on other occasions found a difference of only 4° at most in air at these two stations, Mr. W. was led into a suspicion that the snow might perhaps have been so far cooled down by an evaporation at the surface. With a view to this opinion, he projected the experiment with the bellows described below, by which he was not without expectations of producing a still more remarkable fall of the thermometer when lying on the snow. All the afternoon the cold was very intense, and at 7 o'clock at night the thermometer at the high north window pointed to 0. At 8 Mr. W. repaired to the observatory, and made choice of a station at a sufficient distance from the house, and to the windward, as a light air was felt coming from the east. Here he laid down two thermometers on the snow with their balls half immersed, and hung up other two freely exposed to the air at two feet and a half above the surface. In the following observations, the interruption of the series from $2\frac{1}{2}$ to $6\frac{1}{2}$ o'clock, was owing to an accident having befallen one of the thermometers while the other was employed in the trials, of which an account is subjoined.

Thursday evening, Jan. 13, the two thermometers pointed at the degrees below 0, as in the following table, at the times annexed.

Exper. 1. At half past one o'clock, when the thermometer pointed to -22° , the snow contiguous to the ball was blown on for 2 minutes by a pair of hand-bellows, held with the pipe nearly horizontal, and half a foot above the surface of the snow. The bellows had been lying out on the snow to cool from the time Mr. W. first came over; and, in order to promote their cooling, they were now and then wrought in the open air. Care was also taken to stand to leeward of the thermometer, and to extend the bellows as far as possible from the body in the time of blowing. He was surprized to find however, notwithstanding all the precautions, that the thermometer at the end of the experiment had got up no less than 10° , for it now pointed only to -12° . In this experiment the nozzle of the bellows was held about 6 inches from the thermometer, but the blast, though moderate, frequently drifted away the snow from the ball.

At Night.	Therm. on the snow.	Therm. in the air.
$8\frac{1}{2}$	-12°	0°
9	-14	2
10	-14	4
11	-17	6
$11\frac{1}{2}$	-18	6
Friday morn.		
$\frac{1}{2}$	-20	8
1	-23	7
$1\frac{1}{2}$	-22	8
2	-22	9
$2\frac{1}{2}$	-21	8
3	9
$3\frac{1}{2}$	10
4	12
$4\frac{1}{2}$	12
5	12
$5\frac{1}{2}$	12
6	14
$6\frac{1}{2}$	-22	13
7	-22	13
$7\frac{1}{2}$	-22	13
8	-19	10

Exper. 2. At half past 2 o'clock, a bread-basket was filled with snow, taken up near the ground at $+14^{\circ}$. The contents being relatively so warm, the basket was placed to leeward of the common station, and the thermometer laid on the surface of this snow. At the several hours in the morning, the thermometer on the basket pointed as annexed: viz.

At 3 ^h	-10°
$3\frac{1}{2}$	15
4	$16\frac{1}{2}$
$4\frac{1}{2}$	18
5	18
$5\frac{1}{2}$	18
6	18

Exp. 3. At 4 in the morning, when the thermometer in the basket had got down to -16° , a piece of thin fir plank about a foot square was laid on the snow, on which was placed a small plate of tin which accidentally lay at hand. On this was laid one of the thermometers which had been hanging in the air. At the several times it pointed as annexed.

At 5 ^h	16
$5\frac{1}{2}$	16
6	18

During the whole time not a cloud was perceivable, but there was a faint haze in the air when viewed towards the horizon. There was little or no tremor in the atmosphere, which made the stars shine with a full and steady light like that of the planets. Many of the town's people, who had thermometers hung out at their windows in different parts of the town, found them pointed several degrees below 0 at 9 o'clock in the morning. On the afternoon of this day,

Jan 14, the air became much warmer, and the barometer had now fallen $\frac{1}{10}$. Next day a thaw came on, and continued for some time.

As the above experiment with the bellows favoured so little the opinion, that the difference of temperature was caused by evaporation, Mr. W. wished for another opportunity of making further experiments, and of inquiring into circumstances still more attentively. A good occasion offered on Saturday, Jan. 22. The frost, which before this time had again returned, became on this night very keen; and a good deal of the former snow yet remaining on the ground, the following observations and experiments were made at the observatory, viz. on Sunday morning Jan. 23, at the several hours the 2 thermometers pointed as in the annexed table.

Morn.	Therm on the snow.	Therm. in the air.
$\frac{1}{2}^h$	+ 4.....	+ 14
$\frac{3}{4}$	+ 5.....	+ 14
$1\frac{1}{4}$	+ 4.....	+ 11
$1\frac{3}{4}$	+ 3.....	+ 11
$2\frac{1}{4}$	+ 3.....	+ 11
$2\frac{3}{4}$	+ 3.....	+ 11
$3\frac{1}{2}$	+ 1.....	+ 8
4	+ 1.....	+ 6
$4\frac{1}{2}$	0.....	+ 6
5	— 1.....	+ 5
$5\frac{1}{2}$	— 1.....	+ 6
$6\frac{1}{4}$	— 1.....	+ 6
7	— 0.....	+ 6
$7\frac{1}{2}$	— 3.....	+ 5
$7\frac{3}{4}$	— 2.....	+ 5
$8\frac{1}{2}$	+ 1.....	+ 7

Exper. 4. This night instead of blowing on the snow, Mr. W. fanned it by means of a sheet of brown paper fitted to the end of a long slender stick. This apparatus was previously cooled by lying on the snow, and in fanning he took care to stand to leeward of the thermometer. The effect was, that the mercury rose nearly to the same degree given by the thermometer in air at the same time.

Exper. 5. At $\frac{3}{4}$ past 1 o'clock, when the thermometer on the snow pointed to $+ 3^\circ$, it was screened by 2 sheets of brown paper set up on their edges, and so inclined against each other as to stand. The paper had been previously cooled by lying on the snow. At $2\frac{1}{4}^h$ the thermometer thus sheltered pointed to $+ 9^\circ$. This experiment was afterwards repeated with the same event.

Exper. 6. Mr. W. next went up to the leads of the east wing of the observatory. Here he hung a thermometer to the hook of a long pole, and raised it in the air about 24 feet from the ground, and at the same time inclined the pole over the ballustrade, so as to put the instrument fully to windward of the house. On suddenly lowering the pole, after half an hour, and examining the thermometer, the air at that elevation was found to be pretty constantly 4° warmer than at the station below.

Exper. 7. The result of this trial appeared more remarkable than any thing which had hitherto occurred. Mr. W. lowered the pole till the thermometer was brought down within half a foot of the ballustrade, but keeping it still a few inches to windward of the building; and by this means it was found that the air here was never colder than $+ 10^\circ$. On the ballustrade there happened to be several detached bodies which had attracted a very thick hoar-frost. When the thermometer was taken off the hook of the pole, and laid on this hoar-frost,

there was always a remarkable fall of the mercury, not less than 6° . In shifting the instrument from the pole to the ballustrade, it was commonly laid on some hoar-frost $\frac{3}{4}$ of an inch deep, which had settled on a piece of thin board which had been for years exposed to the weather. Some fragments of the hoar-frost were also made to touch the upper part of the ball; which was done by pushing them on with a long frozen straw.

Exper. 8. When the thermometer, taken from the pole as in last experiment, was laid on pieces of stone, from which the hoar-frost had been brushed away for some time before, the mercury sunk but very little by such a change of situation. Next night, being that of Sunday Jan. 23, the thermometers were placed in their former station below, when they pointed as annexed.

At Night.	Therm. on the snow.	Therm in the air.
At 9 ^h	+ 5	+ 6
9 $\frac{1}{2}$	+ 5	+ 8
10	+ 6	+ 8
10 $\frac{1}{2}$	+ 6	+ 10
11	+ 6	+ 9
11 $\frac{1}{2}$	+ 5	+ 8
12	+ 5	+ 8
12 $\frac{1}{2}$	+ 4	+ 7
1 Morn.	+ 4	+ 8
1 $\frac{1}{2}$	+ 4	+ 8

From these observations it appears, that the cold now was very moderate when compared to that of the 14th, and somewhat more moderate than that of the preceding night. Experiment 7th was again repeated with a similar result, though the difference of temperature was not now so great. This night Mr. W. made another experiment with a view to the evaporation, not so liable to objections as those of the bellows and the fan, as follows.

Exper. 9. When the thermometer in air at the lower station had contracted a considerable film of frozen matter all over the ball, it was swung round at the end of a pack-thread, about a yard and a half long. On stopping the motion at the expiration of 2 minutes, and making the servant who waited approach quickly with a lighted candle, he found the mercury had got up 2° . In this experiment, which was repeated 4 times with the same result, care was always taken to keep the instrument to windward of their bodies, and of the lighted candle.

The 2 following experiments afford some grounds for believing that no kind of evaporation was going on at the time the remarkable excess of cold in the snow and hard frost was observed.

Exper. 10. On Sunday morning, Jan. 23, before 1 o'clock, Mr. W. repeated the experiment with the metal speculum which was tried here in 1768. A large spare metal of a 2-foot telescope was laid out to cool, after which a film of ice was imparted to its polished surface by breathing on it 4 or 5 times. It was then exposed as before, and in half an hour the whole film disappeared in the way of evaporation. But when the experiment was again repeated, and a thicker film imparted, some of this, towards the middle of the speculum, remained fixed, and would not go off after long exposure. The speculum was next warmed,

and its polished surface made quite clean, and then laid out for $2\frac{1}{2}$ hours. Before the expiration of this time it began to draw frozen matter from the air, which settled all over the polished surface in long parallel lines, which gradually multiplied, till at length it was mostly covered with a thin film resembling a spider's web.

The evaporation shown in the first part of this experiment was probably owing to the speculum not having been sufficiently cooled when the film was first communicated to it from the lungs, and to its being further heated by that very operation. In the 2d part of the experiment the evaporation seems to have stopped when the heat in the metal which favoured the process was exhausted; that is, when the speculum had arrived at the temperature of the ambient air; for after that, no heat could pass from the metal in order to contribute to the evaporation. But from the last part of the experiment, the true disposition of the air at that time, relative to bodies as cold or colder than itself, seems to be determined, namely, that of giving out or depositing hoar-frost.

Exper. 11. On Sunday night, Jan. 23, several things were laid out at the observatory, such as sheets of brown paper, pieces of boards, plates of metal, glasses of several kinds, &c. which all began to contract hoar-frost seemingly as soon as each body had time to cool down to the temperature of the air. The sheets of brown paper, being so thin, acquired it soonest, and when beheld in candle-light they became beautifully spangled over by innumerable reflections from the small crystals of hoar-frost which had parted from the air. Evident symptoms of the same tendency of the air to deposit, occurred on all the former nights of observing, by which the tubes of the thermometers were so much stained, that it required some attention to keep that part which corresponded to the scale quite clear.

These experiments indeed rather favour the opinion of the excess of cold, at present treated of, depending on a principle the very reverse of evaporation. But till opportunities offer in this, or in a colder climate, of making more experiments, it will be too early to say any thing decisive concerning the nature or extent of a cooling process, which has so recently come under observation. All that can at present be affirmed is, that in certain circumstances such a process goes on, and that it depends probably on principles different from evaporation or chemical solution. At the same time some of the experiments show that a free communication between the hoar-frost and external air, perhaps while in motion, is necessary; but in what manner this promotes the refrigeration does not as yet appear.

It would be going too far were we to conclude, from the experiments related above, "that very cold air is never disposed to deposit its contents except on bodies as cold or colder than itself." And yet that this is frequently the case

seems probable from a number of common appearances. We often find, after a night of frost, the slates and other thinner parts about a house whitened with hoar-frost, when the walls and more solid parts of the building remain quite free. In like manner the smaller branches and twigs of trees often acquire this frozen ornament, when the main branches and trunk remain naked for a long time; and, in general, any thin or detached body, capable of being easily cooled, attaches hoar-frost the soonest.

In favour of this general position, the following remarkable case lately occurred. Between the public library and the buildings of the new court, there is a long rail composed of bars of cast iron, but divided into 2 parts by 2 massy stone pillars, which support the iron gate-way that leads into the garden. The bars are about 6 feet high, and an inch square, and fastened with lead into a stone parapet below in the usual way. A few bars much larger are set in among the rest at regular distances, to give the rail more stability. On Sunday morning, Feb. 13, when there was a slight frost accompanied with a fog, it was entertaining to observe how the hoar-frost had settled during the night on these bars. Very little was to be seen on the flat sides, but a great deal on the angles, by which means from the top downward every bar was garnished with 4 fringes, which made the whole rail look very gay and ornamental. Running the eye along the foot of the bars near the parapet, it was observed, that the fringe of hoar-frost on the corners stopped short about 12 inches from the bottom, and that so much of every bar was entirely free. Two bars next the house and 2 next the library were likewise perfectly clear of it from top to bottom. One bar next the pillar of the gate was quite free, and the 2d had contracted but little. The same thing precisely may be said of the 2 bars contiguous to the other pillar. And it was also observed, that the few thicker and stronger bars were less fringed at the corners, and were quite free much farther above the parapet than the others.

It is manifest, that during the night the air surrounding the bars must have been constantly endeavouring to make them as cold as itself: while they, on the other hand, resisted this change by drawing heat from every neighbouring source which offered it, namely, from the parapet, from the pillars in the middle, and from the pillars at both ends immediately adjoining to the library, and to the house in the new court: for these bodies, from their great bulk, must have been but very little cooled in the course of the night. Wherever the air seems to have got the better in this struggle, as at the angles of the bars, which evidently must be the parts the soonest cooled, there we find that the hoar-frost was deposited, but no where else.

Several other instances were found quite of the same kind with that of the rail. Among the rest, a figure of a unicorn in stone, which stands within the college, had resisted the attacks of the air all to the tip of his horn, which ac-

cordingly was the only part distinguished by a patch of hoar-frost. Besides this kind of hoar-frost which joined itself to bodies by a regular arrangement, there was some of a different sort found on the uppermost surface of such bodies as were fully exposed to the open air. But this always lay scattered like very thin flakes of meal, or hair-powder, and was found to proceed from minute parts, mostly columnar, previously formed in the air, falling down by their own gravity.

XXXVI. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1779. By Thomas Barker, Esq. p. 474.

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the House.			Abroad.			
					Hig.	Low	Mean	Hig.	Low	Mean	
Jan.	Morn.	30.13	29.22	29.81	44½	34	38½	44	25	34	0.212
	Aftern.				45	35	39	49½	29	38	
Feb.	Morn.	30.06	29.26	29.77	49	41	46	44	29	40	2.239
	Aftern.				52	42½	47	55½	40	49	
Mar.	Morn.	30.13	29.18	29.73	53	42½	47	46	26½	38	0.131
	Aftern.				55	44	48	60	42	50½	
Apr.	Morn.	30.07	28.94	29.49	56½	45	51	52	32½	43½	1.888
	Aftern.				63½	48½	53	72½	47	55	
May	Morn.	29.84	29.14	29.44	64	45½	54	59	36	48½	1.266
	Aftern.				65½	46	55½	76	45½	59½	
June	Morn.	29.32	29.00	29.52	62	55	58½	60½	46½	53	2.416
	Aftern.				63½	56	60	74	59	63½	
July	Morn.	29.94	28.87	29.47	73½	60	65	66½	53	59	4.036
	Aftern.				75½	61	67	81½	60½	72	
Aug.	Morn.	29.89	29.24	29.62	70½	61	64½	65	49	58	1.508
	Aftern.				75	62	67	81½	61	72	
Sept.	Morn.	29.71	29.05	29.42	68	55	61	60½	42½	53	1.227
	Aftern.				69	57	62½	73	55	65	
Oct.	Morn.	29.97	28.89	29.50	60	50½	54	55	34	45½	1.769
	Aftern.				59	52½	55½	64	47	56	
Nov.	Morn.	29.86	28.24	29.19	54	36	44½	50	24½	38	2.050
	Aftern.				54	36½	45	57½	31½	43½	
Dec.	Morn.	29.87	28.35	29.21	50½	30	40	52	14½	35	3.136
	Aftern.				50	31	41	55	22½	38½	
Mean of all				29.51	53			50			19.878

XXIX. Journal of the Weather at Senegal, during the Prevalence of a very Fatal Putrid Disorder, with Remarks on that Country. By J. P. Schotte, M. D. p. 478.

Dr. S. having kept a meteorological journal at the island St. Lewis, in the river Senegal, in Africa, during a time when the greatest part of the garrison, and a great number of the inhabitants on the island, as well as on the continent, died of a putrid disorder, he communicated the same, as he thought this fatal circumstance a sufficient reason to make it acceptable to the R. S.; hoping that it may afford matter to determine the cause of it, and lead to find out remedies to

prevent it in future in the like climates. Previous to the journal, in order to illustrate it, Dr. S. makes a few remarks on the situation of the island, the country about it; its seasons, the manner and time in which the disorder appeared and ceased, &c.

The island St. Lewis, otherwise called Senegal, is situated in 16° N. lat. and 16° W. long. It is separated from the island of Soar on the east by the main river, which, on account of the smallness of the creek by which it is formed, is esteemed a part of the continent. It has the Atlantic Ocean on the west, from which it is separated by a small neck of land, or rather sand, called Barbary Point. This neck of land is in several places not above 5 or 600 yards broad. A branch of the river runs between it and the island itself, communicating with the main river above and below the island. It is about a mile in length, 700 feet in breadth, and contains 5 or 6000 black inhabitants. In the months of August, September, and October, it is usually about 2 or 3 feet above the level of the river at high water; but there are years in which the whole island is overflowed; in the other months of the year it may be about 5 or 6 feet above its level in the highest places. The continent and islands near it are as low, and in many places much lower, being overflowed for the most part during the rainy months; the latter are formed by creeks communicating with the main river, and thickly beset with mangroves. The water of the river is fresh during the rains, but very thick and troubled, the current being so rapid and strong as to stop the flood-tide; but in the dry months the river water is salt, and no other water is to be had, but such as is procured by digging a pit into the sand, more or less deep according to the height of the ground, into which the water filtrates from all sides, and gathers up to the level of the river. This water is brackish; but as no better is to be had thereabouts, the garrison, as well as the inhabitants, make use of it, except when the river is fresh.

The year is commonly divided by the Europeans, as well as the inhabitants, into two seasons; viz. the rainy and dry; by others called the sickly and healthy seasons. The rainy or sickly one generally begins about the middle of July, and ends about the middle of October; during this time the wind is generally between the points of east and south, the quarter from which the tornados come. It has been observed, that the season is more or less unwholesome in proportion to the greater or lesser quantity of rain that falls. A tornado is preceded by a disagreeable closeness and weight in the air, which seems to be much hotter than the thermometer shows it to be; and it is known to come on by the rising of the clouds to the south-east, which by joining become darker, so as to make the horizon look quite black, accompanied with lightning and thunder at a distance. The breeze dies away by degrees as the tornado advances, and an entire calm succeeds; the air becomes yet darker; animals and birds retire and shelter them-

selves; every thing is silent, and the aspect of the sky, whence the tornado approaches, is most dreadful. A violent storm comes on all at once, which is so cold as to occasion the thermometer to fall 7 or 8 degrees in a few minutes, and is strong enough to overset negro huts and vessels, or drive the latter from their anchors, and throw them on shore. The storm abates, and heavy rain follows, accompanied with much lightning and strong claps of thunder. Sometimes tornados happen without rain, or at least with very little; but then the storm is more violent, and lasts longer. It has been imagined by some, that this kind of storm brings some pestiferous quality with it, because they had observed, that out of a number of people several fell sick in one night after a tornado.

This Dr. S. in some degree experienced himself; for in September 1776, feeling himself very well, and having dined as usual, the storm of a tornado suddenly tore down the window shutters, and blew into the room where he was: about an hour after he had rigors, and in the evening he had a high fever, which turned out to be a very severe bilious one; but notwithstanding this, it has, in his opinion, no such ill quality, and the above phenomenon may be attributed to the change it produces on the air, and of consequence on the body; it may therefore be considered as the occasional cause of a disorder to which the body was pre-disposed long before.

The dampness of the atmosphere during this season is so great, that it is more or less perceptible in every thing. Leather, wearing apparel, and books, become mouldy; and polished metals rusty. Sea salt, sugar, and other saline substances, which were perfectly dry before, melt; and the meat of cattle killed in the evening is spoiled the next morning, so as not to be fit for use. Calms are very frequent, and disagreeable on account of the musquetoës and other insects, which then quit their retreats from among the mangroves and marshes, and spread over the face of the country.

The dry or healthy season begins commonly about the middle of October, and lasts to the middle of July. It is called dry, because then it hardly ever rains, or at least but very seldom; and healthy, in opposition to the sickly one; for though pleurisies and peripneumonies will happen in the months of December and January, and fluxes in the months of April, May, and June, few people die; which, when compared with the numbers that die in the other season, justifies the denomination. When the rains cease, the wind shifts its quarter, and is for the most part east or north-east in the morning; but as the sun rises on the horizon, the wind changes more and more towards the north, till about noon, sooner or later, it gets to the west of north, which is called sea-breeze, and is very refreshing, though it happens sometimes, that as the sun falls again on the horizon, the wind will return towards the east, and continue there all night. This wind blows sometimes very strong, and is always excessively hot, drying up

the lakes and pools, which had been formed by the heavy rains and the overflowing of the river, and producing, in such as partake of sea-water, a fine sea salt in large crystals, not unlike fossil salt. In the months of February, March, April, May, and June, the wind blows almost constantly from between north and west, called sea-breeze, except now and then a day or two it will be east, which when it happens in April makes it excessively hot, the sun being then in and about the zenith of Senegal, heating the vast plains of sand over which this wind is to pass before its arrival there, which, reverberating the received heat, may contribute to increase it; for Dr. S. had observed, that the same month in the river Gambia was not hotter than any other wind, owing in all appearance to the difference of the soil of the country, which is not sandy like that of Senegal. Dr. S. thinks it is the dust of the sand raised by this wind which makes the atmosphere look hazy. He saw, in the year 1775, in the month of April, in a morning preceded by an easterly wind, such a dust imitating a fog in the air, that he could not see above 20 yards.

The weather grew calm, and about 11 o'clock in the forenoon the atmosphere became clear, by depositing a brownish impalpable dust, which covered every thing near a line in thickness. The same thing Dr. S. observed at sea from on board a vessel in the month of March 1775, at the distance of about 5 or 6 leagues from the land near the latitude of Senegal. The wind having blown east in the night, he found in the morning the sails, shrouds, and deck, covered with an impalpable dust. The description given by Dr. Lind of the Harmattans of the Coast of Guinea, seems to agree with the east wind at Senegal in almost every respect, except that the damp vapour in the former is not perceptible in this, for it dries every thing that will admit of it. Water poured on the floor of a room, for the purpose of cooling the air, is dried up in an instant, and there is some effect on the thermometer placed in such a room. Salt, sugar, and the like substances, which are half melted by the damp air during the rainy season, dry again in a few days into hard lumps. Such household furniture as is made of wood, though it has been ever so well seasoned, shrinks and loosens where joined, or splits and cracks where glued. It dries and parches the skin of the white people as well as the blacks, and makes it sometimes as rough as any clear frosty weather in Europe would. The sky is commonly clear and without clouds; but the atmosphere is hazy, which, in his opinion, as already observed, is occasioned by the dust, perhaps in conjunction with vapours arising from the surface of the earth and waters. These vapours, though not to be seen in the open air, Dr. S. has perceived by their shadow on white walls, arising from pools which were close to them; but the air being so dry they are absorbed by it, and no more perceptible as vapour. That the evaporation must be very great when this wind blows, the method the blacks have of cooling water will evince. They

fill tanned leather bags with it, and hang them up in the sun; the water oozes more or less through the leather, so as to keep the outer surface wet, which, by its quick and continued evaporation, occasions the water within the bag to become considerably cooler.

This wind is in general not reckoned unwholesome, either by the inhabitants or Europeans, though it feels very disagreeable, and by depriving the body of its thinner fluids, may be considered as the immediate cause of some diseases, and the pre-disposing one to others. When it sets in sooner or later in the month of October, it is considered by the inhabitants as producing a cessation of the sickly weather, and the beginning of healthier. In the months of December and January, when the sun is at his greatest distance, it makes the weather feel very cold in the nights and the mornings.

The putrid disorder, which proved so fatal to the garrison and the inhabitants of Senegal, made its appearance in the beginning of August. The preceding month of July had been remarkably healthy; though the weather was very hot and sultry, there were only 3 soldiers in the hospital for slight venereal disorders; but he learnt by some black messengers, who came from Goree, that there was a fever raging there, which had carried off numbers of the French garrison and inhabitants of the island. On the 2d of August one of the soldiers, who was in the hospital for a gonorrhoea, was discharged as cured. The 4th of August he was again reported as very sick in the barracks. Dr. S. found him in a high fever with the worst symptoms. He ordered him to be carried to the hospital, where he died the 3d day, with all the symptoms of the greatest putridity. The orderly man of the hospital was seized on the 6th of August with the same disease, and died the 9th. One of the venereal patients, who remained still in the hospital, was taken with the same fever, and died a few days after. Some of the soldiers of the fort, having access to the hospital to visit their sick comrades, took the contagion, and spread it through the whole garrison. Dr. S. believed that the disorder was brought to Senegal by the black messengers from Goree; for he understood that one of them had died soon after his arrival in Senegal, and it may be that the soldier who died first of it got the infection from them; for it is probable, that being discharged the hospital on the 2d of August, and having leave to take a walk on the island on the 3d, he had been in company with some of these black messengers, or in the huts where they resorted, for the sake of hearing some news from Goree, where he was acquainted. It may perhaps be observed, that the soldier taking the contagion on the 3d of August, it could not make so rapid a progress as to manifest itself the next morning in the highest degree; but this Dr. S. supports by the following cases. One of the surgeon's mates dressed a blister on the back of a soldier, ill of the disorder, with a digestive softened with oil of turpentine: having done, he came into the

surgery, and looked quite pale, saying, that the soldier's back had smelled so putrid and offensive, that it made him quite faint and sick at the stomach. He took some tincture of bark and bitters, and went home, when a fever, with a train of the worst symptoms, made its appearance in the evening, and he died the 3d day. Another gentleman, who was sent for by the said surgeon's mate in the morning of the 2d day of his illness, and requested to draw up a will for him, arrived while Dr. S. was present. He spoke with the patient for a few minutes, and then took Dr. S. aside, saying that there was a certain smell about the room, which made him faint and sick at the stomach, and that he should be obliged to retire; he did, but in the evening was seized with the fever and all its bad symptoms, went through several of its stages, but recovered. A black boy, who had been waiting on the said surgeon's mate during his illness, was taken with the same disorder, and died of it in a few days. Dr. S. could produce several other cases to strengthen what he had advanced concerning the quick appearance of the disorder itself after the contagion had taken place, but he thinks the 3 related ones sufficient.

The cessation of this contagious disease may be dated from about the middle of September. Governor Clarke, who died the 18th of this month, concluded the dreadful scene. He had avoided the communication with all sick people, but did not hesitate in admitting Dr. S. who was the only one who dined with him for several weeks; and as the Dr. was continually among the sick in the hospital and on the island (of the former of which he gave him a return every morning) he might probably have conveyed the infection to him in his clothing, though he was not affected himself. A few people died in the months of October, November, and December; some of relapses of the same fever, and others of severe fluxes and abscesses in the liver, in which the disorder had terminated. It is remarkable, that a fleet of merchant-men, under convoy of a sloop of war, which left Senegal on the 4th of August, and sailed for England, had it seems been entirely free from this disorder; neither did it reach so far as the river Gambia, for the garrison at Fort James in that river enjoyed a pretty good state of health during all this time, and lost only 2 men, who died of fluxes.

Dr. S. has remarked that at Fort James Gambia in the year 1776, from Feb. 4, to the last of April, that the alteration the weather produced on the barometer was so little as hardly to be perceptible. The equality of the weather during this time (which is part of the dry season in which the sky is always clear and without clouds, though the different winds produce sensible changes in the atmosphere) may perhaps account for it; but Governor Clarke, who had a barometer placed in one of his rooms in the fort at Senegal, told him, that the greatest changes in the weather during the rainy season had so little effect on that instrument that it was hardly worth notice.

The journal of the barometer, which Dr. S. annexed, showed that its greatest variation is about 30° , ranging between 60° , the lowest state, and 92° , the highest.

XXIX. Astronomical Observations relating to the Mountains of the Moon. By Mr. Herschel. p. 507.

When the telescope was first invented this noble instrument was immediately applied to astronomical observations with the most surprizing success. Several eminent persons have given us an account of their discoveries; and, notwithstanding the imperfect state of telescopes in those times, we still owe a great deal of our knowledge of the heavenly bodies to the observations that were made by those first telescopic observers, who made amends for the deficiencies of their instruments by their uncommon diligence and attention. It may perhaps be esteemed a mere matter of curiosity to search after the height of the lunar mountains. Mr. H. allows that there are more necessary and more useful objects of inquiry in the science of astronomy; but when we consider that the knowledge of the construction of the moon leads us insensibly to several consequences, which might not appear at first; such as the great probability, not to say almost absolute certainty, of her being inhabited, we shall soon agree that these researches are far from being trifling.

Mr. H.'s reason for repeating observations that have been made by very good astronomers, was not that he doubted either their veracity or diligence. The names of Galileo, Hevelius, Kircher, and several more, will always deserve to be mentioned with particular respect for the eminent services they have rendered to astronomy; but as we know that their instruments were far from being arrived to that degree of perfection we have now obtained, he thought it by no means improper or useless to repeat their observations on the lunar mountains, and to extend them to other parts of the moon's visible hemisphere, and thus to establish this theory on the firmest evidence of a survey taken by a very excellent instrument.

The method used by Hevelius and others, to find the height of a mountain in the moon, is this: let a ray of light SLM , fig. 6, pl. 7, proceeding from the sun, pass by the moon at L , and touch the top of a mountain at M : then the space between L and M will appear dark, and the top of the mountain will be seen to stand at some distance from the illuminated part of the moon's disc. With a good micrometer let the distance LM be taken by observation. Draw LC perpendicular to LM ; draw also MC from the top of the lunar mountain to the centre of the moon: then in the triangle MLC , right-angled at L , we have given the side LC , which is the moon's radius, and the side LM taken by observation. Therefore, by trigonometry, we can find the hypotenuse MC , from which

subtracting the part pc or radius; there remains the perpendicular height of the mountain mp . Mr. H. has followed the same method, as being the least liable to error.

Galileo takes the distance of the top of a lunar mountain from the line that divides the illuminated part of the disc, from that which is in the shade, to be equal to a 20th part of the moon's diameter; but Hevelius affirms, that it is only the 26th part of the same. When we hence calculate the height of such a mountain, it will be found, in English measure, according to Galileo, almost $5\frac{1}{2}$ miles; and according to Hevelius something more than $3\frac{1}{4}$ miles; admitting the moon's diameter to be 2180 miles. Mr. H. then notices some other remarks of Hevelius; and then adds, the observations of Hevelius have always been held in great esteem; and this is probably the reason why later astronomers have not repeated them. M. De Lalande, who is one of our most eminent modern astronomers, agrees to the sentiments above cited, in his *Abrégé d'Astronomie*, p. 435, where he concludes the height of the lunar mountains to be about the 338th part of the moon's radius, or 1 French league, or rather 2643 toises. He also mentions the opinion of Galileo, and adds: but we ought to prefer the observations of Hevelius, as having been oftener repeated, as well as more detailed and exact.

Mr. Ferguson says (*Astronomy explained*, § 252) "some of her mountains, by comparing their height with her diameter, are found to be 3 times higher than the highest hills on our earth." And Keill, in his *Astronomical Lectures*, has calculated the height of St. Katherine's hill, according to the observations of Ricciolus, and finds it 9 miles.

After these observations Mr. H. says that, before reporting his own observations, it will be necessary to explain by what method he had found the height of a lunar mountain from observations that were made when the moon was not in her quadrature; for the method laid down by Hevelius will only do in that one particular case: in all other positions the projection of the hills must appear much shorter than it really is. Let SLM , or slm , fig. 7, be a line drawn from the sun to the mountain, touching the moon at L or l , and the mountain at M or m . Then, to an observer at E or e , the lines LM , lm , will not appear of the same length, though the mountains should be of an equal height; for LM will be projected into on , and lm into on . But these are the quantities that are taken by the micrometer when we observe a mountain to project from the line of illumination. From the observed quantity on , when the moon is not in her quadrature, to find LM , we have the following analogy. The triangles ool , rML , are similar; therefore, $Lo : LO :: Lr : LM$; but Lo is the radius of the moon, and Lr , or on , is the observed distance of the mountain's projection; and Lo is the sine of the angle $ROL = oLs$, which we may take to be the distance of the

sun from the moon without any material error, and which therefore we may find at any given time from an ephemeris.

Mr. H. then gives an account of his own observations relating to the mountains in the moon. The telescope he used was a Newtonian reflector, of 6 feet 8 inches focal length, to which a micrometer was adapted consisting of two parallel hairs, one of which was moveable by means of a fine screw. The value of the parts shown by the index, was determined by a trigonometrical observation of a known object at a known distance, and was verified by several trials. The power he always used, except when another is mentioned, was 222 times, also determined by experiment, which he had often found to differ somewhat from theory, on account of some small errors in the data, hardly to be avoided. The moon having sufficient light, he used no more aperture of the object speculum than 4 inches.

Observations.—Nov. 30, 1776, 6 o'clock in the morning, a rock, situated near what Hevelius calls *Lacus niger major*, was measured to project $41''.56$. To reduce this quantity into miles, put R for the semidiameter of the moon in seconds, as given by the Nautical Almanac at the time of observation, and a for the observed quantity, also in seconds and centesimals; then it will be in general, $R : 1090 :: a : on$, in miles. Thus it is found that $41''.56$ is 46.79 miles. This distance of the sun and moon at the same time was, by the Nautical Almanac, about $93^\circ 57'\frac{1}{4}$. The sine of which to the radius 1 is .9985 &c. and $\frac{on}{Lo}$ in this case, is $LM = 46.85$ miles. Then by Hevelius's method the perpendicular height of the rock is found to be about 1 mile. The same morning, a great many rocks, situated about the middle of the disc, projected from $25''.93$ to $26''.56$. This gives on about 29.3 miles, and these rocks are all less than half a mile high.

Jan. 13, 1780, 7 o'clock, Mr. H. examined the mountains in the moon; but there was not one of them that was fairly placed on level ground, which is a condition very necessary for an exact measurement of the projection. If there should be a declivity on the moon before the mountains, or a tract of hills placed so as to cast a shadow on that part before them which would otherwise be illuminated, it is plain that the projection would appear too large; and, on the contrary, should there be a rising ground before them, it would appear too little. As far as he was able to judge of the direction of the line of illumination, the highest hill projected $26''.31$, or 30.36 miles: thence we find, as before, that the perpendicular height (.42 mile) is less than half a mile.

Jan. 14, 11 o'clock, he took the projection of the highest mountain, which was situated at the western edge. It measured $24''.68$, or about 27 miles; and the perpendicular height comes out less than half a mile. There was not one mountain in the edge of the disc so high as this. Jan. 17, 7 o'clock, a very

high mountain projected no less than $40''.625$. Its situation is in the south-east quadrant. The moon's semidiameter, at the time of observation by the Nautical Almanac, was $16' 2''.6$; therefore, $\frac{10900}{R} = 45.98$ miles = *on*.

Sun's longitude at 7^h $9^s 27^\circ 39' 0''$

Moon's long. at 7 $2 \ 2 \ 46 \ 52$

Their nearest distance..... $4 \ 5 \ 7 \ 52$

or about $125^\circ 8'$; the sine of which is .8104: thence we find LM 56.73 miles; and the perpendicular height of the mountain is $1^m.47$, or less than a mile and a half.

Jan. 22, $8^h 20'$ the highest mountain, situated near Snell or Petavius, projected $11''.437$, which is $12'.34$; and LM comes out to be 35.3 mile: therefore the perpendicular height is .57 mile. Another, just behind Mare Crisium, measured only $7''$, therefore is less than half a mile high.

Jan. 25, $7^h 30'$ in the morning, a mountain near Aristoteles measured $18''.59$, which gives 20.6 miles; and LM is found 28.53 miles; the perpendicular height is therefore only .37 mile. Other mountains about Mare Nectaris measured about $23''.5$; but they had hills before them, and their situation was not so proper for the purpose. However, it is evident they were of no considerable height.

Jan. 28, 6 o'clock in the morning, the highest mountain in the disc measured $30''.937$; the moon's semidiameter at that time $15' 40''$; and *on* therefore equal 31.37 miles; but as the moon is within 4 hours of her quadrature; we may be assured that this mountain is less than half a mile high.

Feb. 19, Mons Sinopium projected $5''.781$; therefore *on* = 6.26 miles, and the quantity LM 56.54 miles; and consequently the height of this mountain, which it seems proves to be a very high one, is not much less than a mile and a half. However, the journal observes, that the measure was very full; therefore the mountain in all probability does not exceed a mile and a quarter. Besides; Mr. H. thinks that observations made so near the full or new moon are less to be depended on, because a small error in measuring will produce a great one in the height of a mountain.

From these observations Mr. H. believes it is evident, that the height of the lunar mountains in general has been greatly over-rated; and that, when we have excepted a few, the generality do not exceed half a mile in their perpendicular elevation. It is not easy to find any certain mountain exactly in the same situation it has been measured in before; therefore some difference must be expected in these measures. Hitherto he had not had an opportunity of particularly observing the 3 mountains mentioned by Hevelius; nor that which Ricciolus found to project a 16th part of the moon's diameter. If Keill had calculated the height of this last-mentioned hill according to the theorem Mr. H. has given, he

would have found (supposing the observation to have been made as he says, on the 4th day after new moon) that its perpendicular could not well be less than between 11 and 12 miles.

March 11, 1780, 7^h. Promontorium Archerusia projected 17".187. It is very properly situated for measuring. By a proper deduction from the moon's semidiameter, as given by the Nautical Almanac, at the time of observation, we find the quantity $on = 20.1$ miles, and $LM = 22.6$ miles; whence it appears, that the perpendicular height of this mountain is a little less than a quarter of a mile.

Antitaurus, the mountain measured by Hevelius, was badly situated, because Mount Moschus and its neighbouring hills cast a deep shadow, which may be mistaken for the natural convexity of the moon. A good, full, but just measure, 25".105; in miles 29.27: therefore, $LM = 31.7$ miles, and the perpendicular height not quite half a mile. At 7^h 45^m this was repeated, in 2 different observations: a narrow measure 21".562; quite full enough 24".062. These measures give the perpendicular height less than half a mile. At 8^h measured Lipulus, 19".063. It is also badly situated, though rather better than Antitaurus. Finding that the projection increased, therefore concluded that this was not the highest part of the mountain, and waited some time when he measured it again, viz. at 9^h. Lipulus now projected 28".75. Again at 10^h it measured 28".75: this gives $on = 33.64$ miles. Distance of sun and moon about $63^{\circ} 23'$: therefore $LM = 37.54$ miles. Hence the perpendicular height is .64 mile, or very near two-thirds of a mile.

March 12, 1780, 7^h one of the Appennine mountains between Lacus Trasimenus and Pontus Euxinus projected 44".062. This gives us $on = 1.511$ miles; and $LM = 52.9$ miles: therefore the perpendicular height of these mountains, which are very high, comes out to be $1\frac{1}{4}$ mile.

Mons Armenia, near Taurus, projected 31".406 = 36.43 miles; $LM = 38$ miles nearly, and the height $\frac{2}{3}$ of a mile. Mons Leucopetra 34".479 or 40 miles; $LM = 41.4$ miles, and the perpendicular height $\frac{3}{4}$ of a mile.

March 16, 10^h 30^m Mons Lacer projected 45".625; but Mr. H. was almost certain that there are 2 very considerable cavities or places where the ground descends below the level of the convexity, just before these mountains; so that these measures must of course be a good deal too large: but supposing them to be just, it follows, that on is 50.193 miles, $LM = 64$ miles, and the perpendicular height above $1\frac{3}{4}$ miles. Another of the same mountains situated on the borders of S. Sirbonis measured 41".875. This ridge of mountains is the same of which he measured one on Jan. 17, which was then found to be 1.47 miles high.

Memoranda showing the manner in which Mr. Herschel made his observations.—In fig. 7, the points L, s, E, r , are all supposed to be in one plane; and

as the illuminating ray SL is also in this plane, it follows, that the line Lr ($= on$) will always be perpendicular to the right line which joins the cusps of the moon;* and the truth of the theorem delivered in the observations, depends on this circumstance. For this reason he took care in all the observations to measure the line, which in fig. 8 is marked on , parallel to the line CD , or perpendicular to AB , and not the line rn , perpendicular to the elliptical curve $AROB$. The manner of taking it is easy enough: however, he occasionally used 3 different methods. The first method was to set the immoveable hair hh (fig. 9) of the micrometer parallel to a line AB , joining the cusps of the moon; then, by opening the moveable parallel hair till it included the projection on , intended to be taken, he marked that down as the measure of on . As this method required some attention (that part of the ellipsis of illumination AVB which is the vertex v of the lesser axis may serve as a direction) and took up some time, on account of the small field of view of the telescope, he used occasionally these 2 following ways.

When there was any remarkable figure on the disc of the moon near the line of illumination, he put on a compound eye-piece, whose magnified field of view is full 40° , and power about 90 times, so that it takes in the greatest part of the whole moon; by this means he was enabled to view the projection intended for measuring at the same time with the rest of the moon, and to fix on some mark in the disc very near to its edge towards which he judged the line on should be directed; then, putting on the eye-piece which carries the micrometer, he took the distance according to this judgment as well as he could.

The 3d method he took, was the following, which he considers as the best of all, and which he therefore most frequently put in practice. He took a view of some neighbouring shades of rocks or mountains, if there happened to be any near, and directed the measure of the micrometer by them, as they plainly pointed out the direction of the illuminating ray; or, which is the same thing, indicated the line perpendicular to a line joining the cusps. Mons Leucopetra was measured by this last method, which circumstance he mentioned in the observations of the 12th of March, where he saw the whole rock and its highest point, as well as the whole shade, and its last termination, on very even ground, at the same time that he directed the micrometer in that line, to take the projection on , of the abovementioned mountain. Sometimes he compared together a measure taken in the direction on , and one taken in the direction rn ; but as most of his observations were made on mountains not situated near the cusps or

* It is here supposed, that rays from the sun s , and the eye of the observer E , to any part of the moon L , may be taken for parallel; and therefore, that different planes, made by several sections of the moon, according as the point L is taken north or south of the diameter of the moon, which is at right angles to the line joining the cusps, may also be taken to be parallel to that diameter.—Orig.

limb of the moon, he never found so much difference between these 2 measures, as could have occasioned any very material error, if he had entirely neglected it.

By the nature of the ellipsis it will appear that when we do not come too near the limb or cusps of the moon, a tangent drawn to a point in the curve of illumination will seldom make, with the subtangent, an angle that exceeds, or is so much as, 26° ; and in all such cases the error that can arise from taking the line rn instead of on , will be less than the 10th part of the whole measure: but if the angle the tangent makes with the subtangent be only about 18° , the error will be less than a 20th part; and all the measures taken, he believes, will be found to be much within these last-mentioned limits. From this consideration it will appear, that if he had not been aware of this circumstance, his observations would still be sufficiently accurate to disprove the usually assigned great height of the lunar mountains; but as he took all the precaution the situation of each mountain would afford, by using any one of the above-mentioned 3 methods, which suited best, he believes there can hardly be a possibility of any error that should amount to a 40th part of the whole height of any mountain he measured.

The figure $ABCD$ (fig. 10) contained by the diameter AB , the arch CD , and the two curves AD , BC , shows in what portion of the moon's semi-disc we may safely measure the line rn , instead of on , without being liable to so great an error as $\frac{1}{10}$ part of the whole; and the figure $ABcd$ contains that part wherein the measure rn being taken instead of on , the error will be less than the 20th part of the whole measure. In a portion something more confined the error will soon vanish, so that the difference may be safely neglected entirely. Thus in the space $ABxy$ the error cannot amount to 100th part. These figures may be constructed by taking the several points D , d , y , and C , c , x , 26° , 18° , 8° , respectively, from the vertex, the curves AD , Ad , Ay , BC , Bc , Bx , being the loci of those points of the tangents which touch the several ellipses of illumination that may be contained in the semi-disc of the moon, when these tangents make those several angles of 26° , 18° , 8° , with their subtangents.

XXX. Of an Extraordinary Pheasant. By Mr. John Hunter, F. R. S. p. 527.

Reprinted in Mr. J. H.'s *Observations on the Animal Economy*, 4to. 1786.

XXXI. On the Distemper among the Horned Cattle. By Daniel Peter Layard, M. D., F. R. S. p. 536.

In consequence of the essay which Dr. L. published in 1756, he was called on in 1769, by Government, to assist with his advice towards stopping the progress of the contagious distemper among the cattle, which had broken out in Hampshire: and by mere accident he discovered how the infection was brought from

Holland to London, and was conveyed into that county. Speedily and effectually to extirpate the calamity, no assistance was permitted to visit the infected villages, lest the farmers should be induced to prolong the illness, by attempting to cure their cattle; but positive orders were issued that all the cattle should be killed and buried properly, by which vigorous and salutary directions the distemper ceased entirely in a short time. The same acts of parliament and orders of council, to kill the cattle and bury them deep, succeeded also soon after in North Britain; and to the former acts and orders issued in his late Majesty's reign, these alterations were made: to order that the infected cattle should be killed, without effusion of blood, by strangling; the hides to be neither cut nor slashed; but the carcasses buried whole; and that all the fodder, litter, excrement, &c. should be buried, instead of being burned. Since that time the contagious distemper had been brought twice into Essex, and once into Suffolk, from Holland, and as often stopped by the same means.

The orders and regulations which had happily succeeded in Great Britain were communicated to the Dutch, the Flemish, and the French, and copies of all papers delivered to Baron Nolcken, the Swedish minister. In Flanders, and Picardy in France, the system of killing was adopted, and succeeded. Afterwards in 1774, when the same contagion was carried into the south of France from Holland through Bourdeaux, many attempts had failed to effect a cure, the devastation was at last stopped by no other means than by killing the cattle, as in Great Britain. In Flanders the infection was also prevented from spreading a 2d time by the same method of proceeding; but unfortunately in Holland the cattle continued to be exposed to the same disease. The half-yearly returns which had been regularly sent, contained melancholy accounts of the severe loss of cattle; sometimes the whole had perished; at other times $\frac{2}{3}$ had died; and generally above half fell when the sickness was violent. In a country where the illness was become general, and constantly raging more or less, where the system of killing the cattle could not then be thought of, and where inoculation had met with so many opponents of all ranks, there could be no other hope of getting rid of the calamity than by admitting into the United Provinces no other cattle but such as were sound, or recovered from the infection.

In Denmark, where the contagious distemper was become naturalized and general, the Danish government had not only wisely adopted the orders and regulations issued in Great Britain, but had with unwearied application pursued the practice of inoculation. Count Bernsdorff and Dr. Struensee had all the necessary instructions, books, and papers, delivered to them when the King of Denmark was in England; inoculation was approved, recommended, and by authority established. Even in the first 3 years that inoculation was practised, of near 300 head of cattle which were inoculated in a Danish island, not a 6th

part were lost, notwithstanding the many disadvantages which unavoidably occurred. Professor Camper had before attempted to introduce inoculation in Holland; but his abilities, diligence, and perseverance, were so much counteracted by the obstinacy and interruption of the peasants, the badness of the situation, and inclemency of the weather, that out of 112 only 41 recovered; and yet that number is fully sufficient to prove his opinion of the disease, and of the use of inoculation.

Application was made, in 1770, to the lord president of the council by a famous inoculator, for leave to take matter from the infected beasts in Hampshire, and to inoculate the cattle in the southern and western counties of England: on a representation to his lordship, that by such an operation the contagion would not only be introduced in those counties where it had not yet appeared, but also might spread the sickness, so as to become general all over the kingdom as before, a positive and strict injunction was given to drop the intention; especially as by killing the cattle there was no doubt of extirpating the contagion out of Hampshire. The inoculator therefore made no attempt.

According to the several prejudices of different countries, various opinions have arisen of the nature of this sickness. Such as are averse to inoculation have obstinately refused to acknowledge it was similar to the small-pox in the human body, and have very idly asserted, that the only intention of declaring this contagion to be a sort of small-pox was purposely, and with no other view, than to promote inoculation for the small-pox. Others have as positively declared it to be a pestilential putrid fever, owing to a corrupted atmosphere, and arising from infected pastures; but unfortunately for the supporters of this opinion, while the contagious distemper raged with the utmost violence on the coasts of Friesland, North and South Holland, Zealand, and Flanders, there was not the least appearance of it on the English coast from the North Foreland to the Humber, though the coast and climate are the same.

It is needless to dwell on Mr. Turberville Needham's eloquent discourse read at Brussels; since he must have been convinced, when he came to England in 1776, that the illness was not of the kind that he imagined; for such a proof of the inefficacy of salt recommended by him as an antiseptic in this disease had been given as is positive and decisive; viz. that in Scania, a province in Sweden, where it is customary to place a large piece of rock salt, called salt-stein, for the cattle housed in winter to lick, that they may be urged to drink, all the cattle in that province were seized with the contagious distemper, and not one out-lived it. M. Bergius had insisted, that the contagion was not of the exanthematous kind, and therefore inoculation must be of no use; but this opinion was also fully refuted by the late Professor Erxleben of Gottingen, in his learned oration on the 20th of October, 1770.

From every information, domestic or foreign, and comparing the several opinions, experience and observation plainly and completely determine the dispute. The disease among the horned cattle, so fatal in many countries, is not endemial or natural to Europe, though it is become so in Denmark, from spreading all over the Danish dominions, and its long continuance in that kingdom. It is an eruptive fever of the variolous kind; and though the exanthemata, or pustules, may have been frequently overlooked, yet none ever recovered without more or less eruption or critical abscesses; but these differ from the pestilential sort; no otherways similar to the plague, but, like the small-pox, it is communicated by contact, by the air conveying the effluvia, which also lodge in many substances, and are thus carried to very distant places. Unlike other pestilential, putrid, or malignant fevers, it bears all the characteristic symptoms, progress, crisis, and event of the small-pox; and, whether received by contagion or inoculation, has the same appearances, stages, and determination, except more favourably by inoculation, and with this distinctive and decisive property, that a beast having had the sickness, naturally or artificially, never has it a 2d time.

XXXII. An Investigation of the Principles of Progressive and Rotatory Motion.
By the Rev. S. Vince, A. M. of Sidney Coll. Camb. p. 546.

The communication of motion by impact is well known to constitute a considerable part of that branch of natural philosophy called mechanics; and as all our inquiries in it are directed, either to assist us in those operations which add to the conveniencies of life, or to explain, for the satisfaction of the mind, those changes which we daily see arise from the effects of bodies on each other, it might naturally have been expected that the attention of philosophers would have been engaged, first in the investigation of such cases as most frequently occur from the accidental action of one body on another, before they had proceeded to others less obvious. A little consideration will convince any one how seldom it happens, in the collision of two bodies, that their centres of gravity and point of contact lie in the line of direction of the striking body; yet few writers on mechanics have extended their inquiries any further than this simple case. It must however be acknowledged, that the action of bodies on each other, in directions not passing through their centre of gravity, affords a subject at once curious in speculation, and useful in practice.

I. Bernoulli was the first who published any thing on this subject. He found the point about which a body at rest would begin to revolve when struck by another body; observing however that D. Bernoulli had also discovered the same: he also mentioned the curve described by that point in the progressive motion of the body, and directed a method of inquiry by which the velocities of the bodies may be found after the stroke; which comprehends all he has done on

the subject. Two years afterwards D. Bernoulli published a paper on progressive and rotatory motion, containing nothing more than what I. Bernoulli had given before, and, what is a little extraordinary, says in the introduction, *de tali quidem percussione nihil adhuc, quantum scio, publici juris factum fuit ab iis, qui de motu corporum a percussione egerunt.* Euler has also investigated the velocities of the bodies after impact in a manner somewhat different, but has rendered it much more intricate by a fluxional calculus. To any one however, who attentively considers the subject, the theory must still appear to be extremely imperfect, as, independent of principles not more self-evident than the propositions they are intended to demonstrate, which both I. and D. Bernoulli have assumed in their investigations, a great variety of other circumstances, equally interesting, naturally arise in an inquiry into this matter, and which are absolutely necessary towards understanding the principles of the motion of the bodies after impact.

PROP. 1. *Let A and B be two indefinitely small bodies connected by a lever void of gravity; and suppose a force to act at any point D, perpendicular to the lever; to find the point about which the bodies begin to revolve.*—From the property of the lever, the effect of the force acting at D, fig. 1, pl. 8, on the body A, is to the effect on B, as $BD : AD$; hence the ratio of the spaces Am , Bn , described by the bodies A and B in the first instant of their motion, will be as $\frac{BD}{A} : \frac{AD}{B}$; join mn , and if necessary produce that line and AB to meet in c , which will manifestly be the point about which the bodies begin to revolve. Hence, from similar figures, $BC : AC :: \frac{AD}{B} (\propto Bn) : \frac{BD}{A} (\propto Am) :: A \times AD : B \times BD$, or $DC - DB : AD + DC :: A \times AD : B \times BD$; consequently $DC = \frac{A \times AD^2 + B \times BD^2}{B \times BD - A \times AD}$, and therefore D is the centre of percussion or oscillation to the point of suspension c .

Cor. 1. Hence, whatever be the magnitude of the stroke at D, the point c will remain the same.

Cor. 2. If the force act at the centre of gravity G , the bodies will have no circular motion; for in this case $B \times BD - A \times AD = 0$, and therefore DC becomes infinite.

Cor. 3. If the force act at one of the bodies, the centre of rotation c will coincide with the other body.

Cor. 4. If the lever had been in motion before the stroke, the point c , at the instant of the stroke, would not have been disturbed.

PROP. 2. *Let a given quantity of motion be communicated to the lever at D; to determine the velocity of the centre of gravity G.*—The space Am , described by the body A in the first instant of motion, is as $\frac{DB}{A}$: now $CG = CD - DG = CD - AG + AD = \frac{A \times AD^2 + B \times BD^2}{B \times BD - A \times AD} - AG + AD = \frac{B \times BD \times BG + A \times AD \times AG}{B \times BD - A \times AD}$.

also $CA = CD + DA = \frac{A \times AD^2 + B \times BD^2}{B \times BD - A \times AD} + DA = \frac{B \times BD \times AB}{B \times BD - A \times AD}$; hence

$$\frac{B \times BD \times AB}{B \times BD - A \times AD} (AC) : \frac{BD}{A} (\propto mA) :: \frac{B \times BD \times GB + A \times AD \times AG}{B \times BD - A \times AD} (CG) :$$

$$\frac{B \times BD \times GB + A \times AD \times AG}{A \times B \times AB} \propto gw \text{ the velocity of the centre of gravity: hence if}$$

the motion be communicated at G , the velocity becomes as $\frac{B \times GB^2 + A \times AG^2}{A \times B \times AB}$.

Let now the motion, which is supposed to be actually communicated to the rod at D , be equivalent to the motion of a body whose magnitude is G , and moving with a velocity v ; then if that motion be communicated at G , the velocity of the centre of gravity is well known to be $= \frac{G \times v}{A + B}$; hence $\frac{B \times BG^2 + A \times AG^2}{A \times B \times AB} :$

$$\frac{B \times BD \times BG + A \times AD \times AG}{A \times B \times AB} :: \frac{G \times v}{A + B} : \frac{G \times v}{A + B} \times \frac{B \times BG \times BD + A \times AD \times AG}{B \times BG^2 + A \times AG^2} =$$

the velocity of the centre of gravity, when the same motion is actually communicated to any point D . Now $BD = BG + GD$, and $AD = AG - GD$; hence

$$B \times BG \times BD + A \times AD \times AG = B \times BG^2 + A \times AG^2 + GD \times (B \times BG - A \times AG) = (\text{because } B \times BG - A \times AG = 0) B \times BG^2 + A \times AG^2;$$

consequently the velocity becomes $\frac{G \times v}{A + B}$; and hence the centre of gravity moves with the same velocity, wherever the motion is communicated.

PROP. 3. *Let a given elastic body P, moving with a given velocity, be supposed to strike the lever at the point D, in a direction perpendicular to it; to determine the velocity of the centre of gravity G after the stroke.*—Suppose first the body to be non-elastic, and let v be the velocity of the centre of gravity after the stroke on that supposition, and v the velocity of the striking body: then $CG : CD :: v : \frac{v \times CD}{CG}$ = the velocity of the point D after the stroke, or of the body P : for the same reason $\frac{v \times CA}{CG}$ and $\frac{v \times CB}{CG}$ equal the velocities of A and B respectively. Now because, in revolving bodies, the momenta, arising from the magnitude of the bodies, their distance from the centre of rotation and velocity conjointly, remain the same after the stroke as before, we shall have $P \times v \times DC = \frac{v \times CD^2 \times P}{CG} + \frac{v \times CA^2 \times A}{CG} + \frac{v \times CB^2 \times B}{CG}$, and therefore $v = \frac{P \times v \times DC \times CG}{P \times DC^2 + A \times AC^2 + B \times BC^2} = \frac{P \times v \times CG}{(A + B) \times CG + P \times DC}$. Hence if P be supposed an elastic body, we shall have $\frac{2 \times P \times v \times CG}{(A + B) \times CG + P \times DC}$ for the velocity of the centre of gravity after the stroke, in ipso motus initio.

PROP. 4. *To determine the motion of the bodies after the first instant, or when they are left to move freely by themselves.*—The writers on mechanics, from considering the equality of motion on each side of the centre of gravity, when a body revolves about that point, have inferred, that if a body had a projectile as well as a circular motion communicated to it, the centre of gravity would con-

tinue to move in a right line, as that point would not be disturbed by the rotatory motion: yet, as in the case we are now considering, the bodies begin to revolve about a different centre, it may be proper to examine more accurately into this matter, and to show, from what principle it is that the motion of the centre of gravity is preserved in a right line.

Let a motion perpendicular to the rod be communicated to A (fig. 2) and then, by cor. 3, prop. 1, B will not be disturbed by such an action; and A will in the first instant have a tendency to revolve about B as a centre, and would actually describe the arc AH, if the body B were fixed: let the angle ABH be supposed infinitely small; and let GK be the arc the centre of gravity would have described; and draw the tangents AF, Gg to the arcs AH, GK respectively. Now, if A could have moved freely, it would (because $AF = AH$) have described AF in the same time the arc AH was described, on supposition that B was fixed; for the radius BA being perpendicular to the circular arc AH, the force of the lever could have no efficacy to accelerate or retard the motion of A in the arc AH, and therefore the velocity in that arc is the same as it would have been if it had moved freely in the tangent: hence HF is that space through which the centrifugal force of A would have carried that body, could it have moved freely; but as A is connected to B by means of the lever, it is manifest that the same force which would have carried A from H to F in the direction of the lever, will, when it has both bodies to move, carry it over a space which is to FH, as $A : A + B$, or as $Bg : BH$, or as $gK : FH$; hence that space, or the space through which the centrifugal force of A will draw the lever in the direction BH, is equal to gK ; that is, the point K, which is the centre of gravity of A and B, will be found at g, and consequently the centre of gravity has preserved its motion uniform in the right line Gg, inasmuch as the centrifugal force, acting perpendicularly to the direction of the centre of gravity, can neither accelerate nor retard its motion. In the same manner it may be proved, that the motion of the centre of gravity is continued uniform in the same right line, whatever be the position of the lever. Also, as the centrifugal force acts in the direction of the lever, it cannot alter its angular velocity, which will therefore remain as in ipso motûs initio. If now we suppose that, to the force impressed on A, two other equal accelerative forces be communicated to A and B at the same time, it is evident that no alteration can arise from the actions of the bodies on each other; and the case will then be similar to the motion of the bodies, supposing a single force had been impressed at any point D. The like method of reasoning may be extended to any number of bodies.

The same thing may also be easily demonstrated in the following manner. The centrifugal forces of A and B (fig. 1) are respectively $A \times AC$ and $B \times BC$; also the centrifugal force of the point G, considering it as having both bodies to

move in the direction of the rod, is $(A + B) \times GC$; but from mechanics $A \times AC + B \times BC = (A + B) \times GC$: hence the centrifugal forces of the bodies A and B give the centre of gravity a centrifugal force equivalent to its own centrifugal force, which, as the latter would cause that centre to move in the tangent Gg, the lever not being fixed at c, it is manifest that the former will cause the centre of gravity to continue its motion in the same direction.

That this motion of the lever, in a direction from the centre c, is the only motion which is communicated to it from the effect of the bodies A and B, is manifest from hence: the bodies begin to revolve freely about the point c, and consequently if the point c had been fixed, the bodies would have moved on with a uniform angular velocity about c; if therefore we suppose the lever not to be fixed at c, as the efficacy of the centrifugal force which acts in the direction of the lever is now suffered to take place, and no new external force is impressed on either of the bodies, it is manifest, that if in the former case the bodies had no efficacy to disturb the angular velocity of the lever, they cannot have any in the latter; consequently the angular velocity, and from what has been before proved, the uniform motion of the centre of gravity in a right line, remain unaltered, after the commencement of the motion.

PROP. 5. *In the time the bodies make one revolution, the centre of gravity will move over a space equal to the circumference of a circle whose radius is CG, (fig. 1.)*—From the last prop. the angular velocity of the lever is continued uniform: hence the time of a revolution is just the same as if the point c were fixed, and the bodies were to continue to revolve about that point as a centre, in which case the centre of gravity G, in the time of a revolution, would evidently describe the circumference of a circle whose radius is GC. This therefore is the space the centre of gravity describes in a right line when the bodies move freely; for by the last prop. that centre is carried uniformly forward with the same velocity.

Cor. 1. Hence, if the quantity of the force acting at D vary, the velocity of the centre of gravity will vary in the same ratio as the angular velocity.

Cor. 2. Hence the point D may be found, where a force being applied, the bodies shall make one revolution, while the centre of gravity moves over any given space s : for let p = the periphery of a circle whose radius is unity, then $p : 1 :: s : \frac{s}{p}$ = the radius of a circle whose circumference is the space to be passed over in the time of a revolution, and which must therefore, by the prop. be equal to GC: the point c therefore being determined, D may be easily found; for from mechanics $CG \times DG$ is given; and from cor. 3, prop. 1, when D comes to A, C will coincide with B: $CG \times GD = AG \times GB$, and consequently $DG = \frac{AG \times GB}{CG}$.

PROP. 6. *To determine the time of one revolution, supposing every thing given as in prop. 3.*—The point *D* being given, we have, from cor. 2 to the last prop. $CG = \frac{AG \times GB}{DG}$. Put *w* equal the circumference of a circle whose radius is *CG*; then it appears from the last prop., that *w* is the space the centre of gravity passes over in the time of one revolution; hence, because from prop. 4, the centre of gravity moves uniformly, we have, by prop. 3,

$$\frac{2 \times v \times P \times CG}{(A + B) \times CG + P \times DC} : 1'' :: w : w \times \frac{2 \times v \times P \times CG}{(A + B) \times CG + P \times DC} = \text{the time of one revolution.}$$

Cor. Hence the angular velocity, being inversely as the time of a revolution, will vary as $\frac{(A + B) \times CG + P \times DC}{v \times P \times CG \times w}$.

PROP. 7. *The point C, as the centre of gravity moves forward, will describe the common cycloid.*—From the description of the common cycloid, it appears that the centre of the generating circle passes over a space equal to the circumference of that circle while it makes one revolution. With the centre *G* (fig. 3) and radius *GC*, describe the circle *CXY*, and draw *CR*, *GW*, perpendicular to *ABC*, and let the circle *CXY* be supposed to revolve on the line *CR*: then will the centre *G* move over a space equal to the circumference of the circle *CXY* while it makes one revolution, and the point *C* will describe the common cycloid; but, from prop. 5, the point *G* will move over a space equal to the circumference of a circle whose radius is *GC*, while the bodies, and consequently *GC*, make one revolution; and hence the point *C* will describe the same curve as before, that is, the common cycloid.

PROP. 8. *Let a motion be communicated to the lever obliquely; to determine the point about which the bodies begin to revolve.*—Let *FD* (fig. 4) represent the force communicating the motion at the point *D*, which resolve into 2 others, *FH*, *HD*, the former *FH* parallel to the lever, and the latter *HD* perpendicular to it. Let *c* be the point about which the bodies would have begun to revolve, had the force *HD* only acted, and which may be found by prop. 1: and suppose in this case *mgn* to have been the next position of the lever after the commencement of the motion, or that the bodies *A*, *B*, and centre of gravity *G*, had been carried to *m*, *g*, and *n* respectively. But as the force *FH* acts at the point *D* at the same time in the direction of the rod, if we take *Gq* : *Gg* as *FH* : *HD*, then while the centre of gravity would have moved from *G* to *g* in consequence of the force *HD*, it will by means of the force *FH* be carried in the direction of the lever from *G* to *q*, and also every other point of the lever will be carried in the same direction with the same velocity; take therefore *Ap* and *Br* each equal to *Gq*, and complete the parallelograms *Aa*, *Gw*, and *Bb*; then the bodies *A*, *B*, and centre of gravity *G* will, at the end of that time, be found at *a*, *b*, and *w* respectively, and *awb* will be the position of the lever. Now it is evident that *c* is not the

point about which the bodies begin to revolve; for, considering the lever to be produced to c , that point must have moved over a space cc equal to Gg , when the lever is come into the position awb : draw co perpendicular to cb , and go perpendicular to gw , then o will be the centre of rotation at the commencement of the motion. For conceive co to be a lever; then the lever ABC has a circular motion about c , while that point is moving from c to c , and consequently the point o is carried forward in a direction parallel to cc by this motion; but as the lever co is carried by a circular motion about c in a contrary direction, it is evident that that point of the lever co must be at rest where these two motions are equal, as they are in contrary directions. Now the velocity of c in the direction cc : velocity of G about c :: Gg : Gg :: (by sim. triang.) co : cG , and the velocity of the point G about c : velocity of the point o about c :: cG : co ; hence ex æquo the velocity of c in the direction of cc , or of o in the direction op parallel to cc , is equal to the velocity of the same point o in a contrary direction arising from its rotation about c , and consequently o , being a point at rest, must be the centre of rotation in ipso motûs initio. Also, because ma is equal and parallel to nb , ab must be equal and parallel to mn , therefore the angular velocity is just the same as if the force FH had not acted. The centre o of rotation at the beginning of the motion being thus determined, every thing relative to the motion of the bodies, after they are at liberty to move freely, may be determined as in the preceding propositions.

Cor. 1. Hence it appears, that whatever be the magnitude or direction of the force communicating the motion, or the point at which it acts, the centre of gravity will move in a line parallel to the direction of the force; for the triangles FHD , Gqw being similar, gw must be parallel to FD .

Cor. 2. The same is manifestly true for any number of bodies. For let E (fig. 5) be a 3d body, and conceive it to be connected with the other 2 bodies A and B in their centre of gravity G : then if FD represents the force acting at the point D , it is evident from the last corol. and the 2d prop. that the centre of gravity moves with the same velocity and in the same direction, as if the same motion had been communicated at G in a line RG parallel to FD , and that the centre of gravity has the same velocity communicated to it, as if the 2 bodies had been placed at G : conceive therefore the bodies A and B to be placed at G , and let the force act at D , and then from the last corol. the centre of gravity g , of the 3 bodies, will move in a line parallel to the direction of the force communicated. In the same manner it may be proved for any number of bodies.

SCHOLIUM.—The method here made use of to determine the point of rotation in ipso motûs initio, when a single force acts at any point D , may be applied, when any number of forces act at different points at the same time. For let α , β , γ , &c. (fig. 1) represent the forces acting on the lever at the points D , E , F ,

&c. respectively; then from the same principles the effect of all the forces on A : the effect on $B :: \frac{\alpha}{AD} + \frac{\beta}{AE} + \frac{\gamma}{AF} + \&c. : \frac{\alpha}{BD} + \frac{\beta}{BE} + \frac{\gamma}{BF} + \&c.$ which quantities put equal to P and Q respectively, and then $\frac{P}{A} : \frac{Q}{B} :: Am : Bn :: AC : BC$; whence it appears that, (putting $GC + GA = AC$ and $GC - GB = BC$) the distance $GC = \frac{A \times Q \times AG + B \times P \times BG}{B \times P - A \times Q}$. The same conclusion might have been deduced from this consideration; that if any number of forces act on a lever, the effect on any point of that lever is just the same as if a force, equivalent to the sum of these forces, had acted at their common centre of gravity; find therefore their common centre of gravity, and conceive a force equivalent to them all to be communicated to that point, and the problem is reduced to the case of the first proposition. If any of the forces had acted on the opposite side of the lever, such forces must have been considered as negative.

If there be any number of bodies placed on the lever, and a single force act at D , it will appear from the same principles that the point c , about which they begin to revolve, will be the point of suspension to the centre of percussion D ; and the same conclusion will be obtained, if the bodies be not situated in a straight line. As a direct investigation however, is always to be preferred to conclusions drawn from induction, it may be thought proper, before we apply any of the foregoing principles to the case of the action of bodies on each other by impact, to show how such a direct investigation, to determine the point about which a body, having a motion communicated to it, begins to revolve, may be obtained; previous to which however, some further considerations are necessary.

PROP. 9. *If a force act on a body in any given direction, not passing through the centre of gravity; to determine the plane of rotation, and the direction in which the centre of gravity begins to move, with its motion after.*—Conceive a plane $AYBZ$ (fig. 6) to be supported on a line AB passing through its centre of gravity G ; and suppose a force to act at any point D in that line, and in a direction perpendicular to the plane; then it is manifest, that such a force can give the plane no rotatory motion about AB . Imagine now the support to be taken away while the force is acting at D ; then it is evident, that as the plane had no tendency to move about AB as an axis, and the taking away of the support can give it no such motion, it will, by cor. 2, prop. 8, begin its progressive motion in the direction in which the force acts; and as the force is supposed not to act at the centre of gravity, it must at the same time have a rotatory motion about some axis, which, as it has no motion about AB , must lie somewhere in the plane, and perpendicular to AB ; and consequently in ipso motûs initio the plane of rotation must be perpendicular to the plane $AYBZ$. Let LCM , perpendicular to AB , be the axis about which the plane begins to revolve, and p, q , be two equal

particles of the plane similarly situated in respect to AB , also qb , pa , perpendicular to LCM . Now the centrifugal force of p , or its force in the direction ap , is $p \times ap$, and that of q in the direction bq , is $q \times bq$; to determine now how these forces will affect the motion of the plane, we may observe in the first place, that the force $p \times ap$, acting at a in the plane, must tend to give it a motion about an axis perpendicular to the plane; but as an equal force $q \times qb$ acts at q to give it a motion in a contrary direction, it is evident that the two forces will destroy each other, so far as they tend to generate any motion in the plane about an axis perpendicular to it; and hence it is manifest, that if the parts of the plane AYB , AZB , be similar, and similarly situated in respect to AB , the plane, after the commencement of the motion, will have no tendency to revolve about an axis perpendicular to it. Also, as the centrifugal force of each particle acts in a direction parallel to AB , it can give the plane no tendency to revolve about that line as an axis, and consequently the plane of rotation will be preserved as in ipso motûs initio. Conceiving therefore the plane on each side the line AB to be similar, and similarly situated, suppose another plane to be fixed on this, whose parts on each side AB are similar, and similarly situated, and the force to act as before; then it is manifest, that as each plane endeavours to preserve the same plane of rotation, the two planes connected will also continue to move in the same plane of rotation; for the action of one plane on another, on each side the plane of rotation, being equal, cannot tend to disturb the motion in that plane; and as this must be true for any number of planes thus similar and similarly situated, it is evident, that if a force should act on a body, and each section, perpendicular to the direction of the force, should be similar on each side the plane passing through the direction of the force, and the centre of gravity of the body, that that plane would be the plane of rotation in which the body would both begin and continue its motion. It appears also from what has been proved, that if every section on each side of that plane had not been similar, the plane of rotation would not necessarily have continued the same after the commencement of the motion. Hence all bodies, formed by the revolution of any plane figure, will have the axis about which they were generated, a fixed axis of rotation. To determine however, every other axis of a body about which it would continue to revolve, would be foreign to the subject of this paper. Supposing therefore the plane of rotation to continue the same (for in this paper Mr. V. means to confine his inquiries to such cases) imagine all the particles of the body to be referred to that plane orthographically, which supposition not affecting the angular motion of the body, the centrifugal force of all the particles, to cause the body to revolve about an axis perpendicular to that plane, will remain unaltered. Let $LMNO$ (fig. 7) be that plane, and suppose a force to act at A in the direction PA lying in the same plane, which produce till it meets LN ,

passing through the centre of gravity G , perpendicularly in D ; then by cor. 2, prop. 8, the centre of gravity G will begin its motion in a line parallel to PA , or perpendicular to LN ; and consequently the centre c , about which the body begins to revolve, must lie somewhere in the line LN . Now the centrifugal force of any particle p , is $p \times pc$; let fall pa perpendicular to LN , then the effect of that force at c , in a direction perpendicular to LN , will be $p \times pa$, and in the direction CL it will be $p \times ca$; but as the sum of all the quantities $p \times pa = 0$, and the sum of all the quantities $p \times ca =$ the body multiplied into cg , it follows, from the same reasoning as in prop 3, that the point G will continue to move in a direction perpendicular to LN ; and also, as the forces $p \times ca$ act in a direction perpendicular to that in which the centre of gravity moves, its motion must be continued uniform. In the following propositions therefore, we suppose the axis of the body, after the commencement of the motion, to continue perpendicular to the plane passing through the direction of the force, and the centre of gravity of the body, and that the body itself is orthographically projected on that plane; also in the case of the action of two bodies on each other, the plane passing through the direction of the striking body and point of percussion is supposed to pass through the centres of gravity of each body; that the axis of each body, after it is struck, continues perpendicular to that plane, and that each body is reduced to it in the manner above described.

PROP. 10. *To determine the point about which a body, when struck, begins to revolve.*—Let $LMNO$ (fig. 7) represent the body, G the centre of gravity, and PA the direction of the force acting at A , which produce till it meets LN , passing through G , perpendicularly in the point D ; draw pb perpendicular to pc , on which, produced if necessary, let fall the perpendicular dw ; c being supposed the point about which the body begins to revolve, and which, from the last prop. is somewhere in the line LN . Because the body, in consequence of the force acting at D , begins to revolve about c , and consequently if, immediately after the beginning of the motion, a force were applied at D equal to it, and in a contrary direction, the motion of the body would be destroyed, it is evident, that the efficacy of the body revolving about c , to turn the body about D , should any obstacle be opposed to its motion at that point, must be equal to nothing; for were it not, the body, when stopped at D , would still have a rotatory motion about that point, and consequently two equal and opposite forces applied at D would not destroy each other's effects, which would be absurd. Now the force of a particle p , in the direction pw , being $p \times pc$, its efficacy to turn the body about the point D , is $p \times pc \times dw$; but by sim. triang. $dw : Db :: ac : pc$, therefore $dw = \frac{Db \times ac}{pc}$, and consequently the efficacy to turn the body about $D = p \times Db \times ac = p \times ca \times (DC - cb) = p \times ca \times DC - p \times pc^2$; hence the sum of all the $p \times ca \times DC$ — the sum of all the $p \times pc^2 = 0$, and conse-

quently $CD = \frac{\text{sum of all the } p \times pc^2}{\text{sum of all the } p \times ca}$; therefore D is the centre of percussion, the point of suspension being at c.

Cor. From the preceding prop. it appears, that every thing which was proved in prop. 5, 6, 7, holds here also in the case of the action of one body on another.

PROP. 11. *Let a body P (fig. 8) moving with the velocity v, strike the body a at rest in the point A, and in a direction AD passing through the centre of gravity of the striking body; to determine the velocity of each body after the stroke, supposing them to be elastic.*—The solution of this prop. depending on the same principles as that of prop. 3, we shall have, putting v equal the velocity of the centre of gravity G after the stroke, on supposition that the bodies were non-elastic (DGC being supposed perpendicular to AD, and c the point about which the body a begins to revolve) $v \times P \times CD = \frac{v \times P \times CD^2}{CG} + \frac{v \times \text{sum of all the } p \times cp^2}{CG}$,

and consequently $v = \frac{v \times P \times CD \times CG}{\text{sum of all the } p \times pc^2 + P \times CD^2}$; but it is well known that the sum of all the $p \times pc^2 = CG \times CD \times a$, and hence $v =$

$\frac{v \times P \times CG}{Q \times CG + P \times DC}$, and therefore if the bodies be supposed elastic, we have $\frac{2P \times v \times CG}{Q \times CG + P \times DC}$ for the velocity of the centre of gravity G after the stroke. Now

to determine the velocity of P, we have $\frac{P \times v \times CD}{Q \times CG + P \times DC}$ equal its velocity after the stroke from single impact, and consequently $v - \frac{P \times v \times CD}{Q \times CG + P \times DC} = \frac{Q \times v \times CG}{Q \times CG + P \times DC}$ is the velocity lost by P from simple impact; hence if the bodies be elastic, $\frac{2 \times Q \times v \times CG}{Q \times CG + P \times DC}$ will be the velocity lost by P if elastic, and consequently the velocity of P after the stroke $= v - \frac{2 \times Q \times v \times CG}{Q \times CG + P \times DC} = \frac{P \times DC - Q \times GC}{Q \times GC + P \times DC} \times v$.

Cor. 1. If the direction AD pass through G, then CG being equal to CD, we have $\frac{2Pv}{Q + P} = a$'s velocity, and $\frac{P - Q}{P + Q} \times v = P$'s velocity, which is well known from the common principles of elastic bodies.

Cor. 2. If $P \times DC = a \times GC$, or $P : a :: GC : DC$, then will the body P be at rest after the stroke.

Cor. 3. If a were infinitely great, the velocity of P after the stroke would be $= -v$ as it ought, for P would then strike against an immoveable obstacle.

Cor. 4. Whatever motion a gains from the action of P, it would lose, if, instead of supposing P to strike a, a were to move in an opposite direction, and strike P at rest with the same velocity with which P struck a; in such case therefore, the velocity of a after the stroke would be

$v - \frac{2P \times GC \times v}{Q \times GC + P \times DC} = \frac{(Q - 2P) \times GC + P \times DC}{Q \times GC + P \times DC} \times v$.

Cor. 5. Hence, if P be infinitely great, or a be supposed to strike an

immoveable object, its velocity after the stroke will be $= \frac{DC - 2CG}{DC} \times v$: hence when $DC = 2CG$, the body a will have no progressive motion after the stroke, but would in such case, if p were immediately taken away, continue to revolve about a fixed axis. It may also be observed, that when DC is greater than $2CG$, or the velocity of a is positive, that, because it is impossible for a to continue its progressive motion, it is only to be understood, that if immediately after the impact the body p were removed, the body a would then proceed with such a velocity.

Cor. 6. Suppose the bodies to be non-elastic, and let m be the magnitude of a body placed at D , which, being acted on by p , shall have the same velocity generated as was before generated in the point D of the body a ; then by the common rule for non-elastic bodies, the velocity of m after the stroke will be $\frac{p \times v}{p + m}$, and hence $\frac{p \times v}{p + m} = \frac{p \times v \times DC}{q \times CG + p \times DC}$, consequently $m = a \times \frac{CG}{DC}$.

Cor. 7. If a given quantity of motion were communicated to any point of the body a , the progressive motion of that body after the stroke would be the same. For suppose the magnitude of the body p to be diminished sine limite, and its velocity to be increased in the same ratio, then; because $\frac{p \times v \times CD}{q \times CG + p \times DC}$ (which is the velocity of p after the stroke, if the bodies be non-elastic) $=$ (because p is infinitely small) $\frac{p \times v \times CD}{q \times CG}$, the velocity of p after the stroke from simple impact, is finite, consequently its motion must be infinitely small, and therefore p must have communicated all its motion to a : now in this case the velocity of a ($= \frac{p \times v \times CG}{q \times CG + p \times CD}$) $= \frac{p \times v}{q}$, which quantity is independent of the place where the force acts; in the same manner it would appear if we had supposed the bodies elastic.

PROP. 12. Supposing every thing given as in the last proposition, except that the direction AD does not pass through the centre of gravity g of the striking body; to determine the velocity of each body after the stroke.—Let AD (fig. 9) be produced to meet Fgo passing through g , the centre of gravity of the striking body, perpendicularly in F , and suppose O to be the point of the body p which is not disturbed by the action of p on a : now it appears from cor. 6, prop. 11, that if both bodies were non-elastic, and a body equal to $a \times \frac{CG}{CD}$ were placed at D , the velocity of that body, from the action of p , would be equal to the velocity of the point D of the body a ; for the same reason therefore it appears, that if, instead of supposing p to strike a in the direction FA , a body equal to $p \times \frac{GO}{FO}$ were to strike a at the same point, and in the same direction, which direction is supposed to pass through the centre of gravity of that body, the effect on a would be the same: hence, if in the quantity $\frac{v \times p \times CD}{q \times CG + p \times DC}$, which from the

last prop. expresses the velocity of the point *D* after the stroke, on supposition that the bodies are non-elastic, we substitute for *P* a body equal to $P \times \frac{GO}{FO}$, we shall have $\frac{V \times P \times DC \times GO}{Q \times GC \times FO + P \times GO \times DC}$ for the velocity of the point *D* from the action of *P*; and consequently $\frac{2 \times V \times P \times GC \times GO}{Q \times GC \times FO + P \times GO \times DC} =$ the velocity of the centre of gravity *G* of the body *Q*, after the stroke, if the bodies be perfectly elastic. To determine now the velocity of the striking body, let *of*, perpendicular to *og*, be the space described by the point *o* in the first instant of time after the stroke, which, as that point is not disturbed by the action of the bodies on each other, may represent the velocity of *P* before the stroke, and let *fb* represent the velocity of the point *F* after the stroke; join *fb*, and draw *gd* perpendicular to *og*, then will *gd* represent the velocity of the centre of gravity *g* of the striking body after the stroke. Draw *fc* perpendicular to *FA*, and produce *gd* to meet *fc* in *e*; now the velocity lost by *P* at the point *F*, by simple impact, being equal to *v* —

$\frac{V \times P \times DC \times GO}{Q \times GC \times FO + P \times GO \times DC} = \frac{V \times Q \times GC \times FO}{Q \times GC \times FO + P \times GO \times DC}$, we shall have *bc* the velocity lost by the point *F*, on supposition that the bodies are perfectly elastic (supposing *of* to represent the value of *v*) equal to $\frac{2 \times V \times Q \times GC \times FO}{Q \times GC \times FO + P \times GO \times DC}$, and

therefore, by sim. triang. *fc* (*FO*) : *cb* :: *fe* (*og*) : *ed* = $\frac{2 \times V \times Q \times GC \times GO}{Q \times GC \times FO + P \times GO \times DC}$ = the velocity lost by the centre of gravity *g*, and hence *v* —

$\frac{2 \times V \times Q \times GC \times GO}{Q \times GC \times FO + P \times GO \times DC} = \frac{V \times Q \times GC \times FO + V \times P \times GO \times DC - 2 \times V \times Q \times GC \times GO}{Q \times GC \times FO + P \times GO \times DC}$

= the velocity of *P* after the stroke. Now as it appears, from prop. 9, that the progressive motion of a body, when left to move freely, continues uniform and in the same direction, it follows, that the expressions for the velocities of each body in the first instant after the stroke, both in this and the preceding propositions, will represent the uniform progressive velocities with which the bodies will continue to move, and consequently the place of each body, at the end of any given time after impact, may easily be determined.

Cor. 1. If the direction *FA* pass through *g*, then, *FO* and *go* becoming infinite, we shall have $\frac{2 \times V \times P \times GC}{Q \times GC + P \times DC}$ for the velocity of *Q*, and $\frac{V \times P \times DC - V \times Q \times GC}{Q \times GC + P \times DC}$ for the velocity of *P*, agreeable to what was proved in the last proposition.

Cor. 2. Hence the point about which *P* begins its rotatory motion, may easily be found; for produce (if necessary) *fb* and *of* to meet in *a*, and *a* will be the point required; and by sim. triang *bc* ($= \frac{2 \times V \times Q \times GC \times FO}{Q \times GC \times FO + P \times GO \times DC}$) : *cf* :: *fo* ($= v$) : *oa* = $\frac{Q \times GC \times FO + P \times GO \times DC}{2 \times Q \times GC}$, and hence *Fa* = $\frac{P \times GO \times DC - Q \times GC \times OF}{2 \times Q \times GC}$.

Cor. 3. If, instead of supposing *Q* to have been at rest, it had been moving forward in a direction parallel to that of the body *P*, with the velocity *v*, the

motion of each body after the stroke may easily be determined: for considering p as acting on q with the velocity $v - v$, putting $2M = \frac{2p \times GC \times GO}{q \times GC \times FO + p \times go \times DC}$ we shall have, by this prop. $(v - v) \times 2M =$ the velocity communicated to G ; therefore $v + (v - v) \times 2M =$ the velocity of q after the stroke: also $(v - v) \times M \times \frac{CD}{CG} =$ the velocity gained by the point D from simple impact, and consequently the velocity of that point after $= v + (v - v) \times M \times \frac{CD}{CG}$, hence $v - v - (v - v) \times M \times \frac{CD}{CG} =$ the velocity lost by p at the point F from simple impact; therefore p 's velocity after the stroke $= v - [v - v - (v - v) \times M \times \frac{CD}{CG}] = \frac{2go}{FO}$. In the same manner it might have been determined had q moved in an opposite direction.

Cor. 4. Hence also we may easily determine the motion of each body after the stroke, supposing q had not been moving in a direction parallel to the motion of p , by resolving q 's motion into two parts, one parallel to the motion of p , and the other perpendicular; and finding by the preceding what would be the effect of the parallel motions, and then compounding q 's motion after the stroke, from that consideration, with the motion it had in a direction perpendicular to it before the stroke.

Cor. 5. The point a of the body p will describe the common cycloid, when that body after the stroke has any progressive motion.

Cor. 6. Hence therefore the times of the revolutions of each body may be determined as in prop. 6.

Cor. 7. If the bodies had any rotatory motion before impact, every thing relative to the motion of the bodies after the stroke might have been determined from the same principles.

XXXIII. The Case of James Jones, continued from p. 684. By Richard Browne, Cheston. p. 578.

Mr. C. now could announce the state the bones of the pelvis appeared in after a maceration of 5 months: for though by very seldom changing the water, and keeping the vessel containing it rather in a warm place, he suffered the highest putrefaction to come on, it took up that space of time before the soft parts were entirely destroyed. Maceration has now shown, that the depth the probe entered, and the gritty resistance felt in the body of the tumor, was not from its passing through a carious or diseased part, as there was reason then to suppose, but from the quantity of osseous matter deposited on the outer side of the os innominatum; and that the part so loaded with it externally, and as it afterwards proved to be internally, was apparently in a sound state.

As the soft parts of the tumor decayed in maceration, great quantities of

bony matter, in irregular forms and of different sizes, were found in the water at the bottom of the pan; and as no force, nor even motion, had been used which could have separated this matter from what remained adherent to the bone, it is highly probable it was ever deposited in, and dispersed through the tumor in a detached state. The tumor externally bore the usual appearances of a diseased or enlarged gland; but, by degrees the whole appearance was changed, and the bony matter, as the maceration proceeded, seemed surrounded by a hard, white, and rather transparent substance, not much unlike suet, in which state it principally resisted the dissolvent power of the water. When the bone in general seemed sufficiently cleansed for drying, there were in one part some remains of this suety substance; but on exposing it to the open air, in the course of 3 days it was entirely dissipated, scarcely a trace of it remaining, unless that, in the particular portion alluded to, the bony matter was of a more dusky colour than elsewhere.

The left os innominatum now appeared perfectly free from any unnatural appearance in every part, even to its junction at the symphysis of the pubis; but there the line was drawn, and disease immediately began to shew itself through the whole of the right os innominatum, and to advance as it were from a superficial ulceration to excrescences in the greatest quantity. It is remarkable that the cartilage connecting the ossa pubis should be so complete a boundary to the disease; for though the external lamella was in all that part of the os pubis and ischium (particularly at the ramus of the latter) which united forms the foramen ovale, not the least deficiency was to be observed in the left os pubis. So interesting to the knowledge of the nature of this disease is it to observe that the extent of the tumor, which terminated exactly at this part, should likewise as exactly have limited its effects or consequences.

So far then as the external lamella of the os pubis and ischium was deficient, so far these bones presented that roughness and irregular loss of substances, which is commonly denominated ulceration or superficial caries. The bottom of the acetabulum had likewise suffered in a similar manner: but over the whole of the ilium, both externally and internally, the lamellæ seemed very little injured, though covered by vast quantities of bony matter branching out into various forms of different sizes, which a minute examination and careful attempt to separate ascertained to be mere, though firm, adhesions to the surface of the bone, and adventitious to the part on which they were found.

XXXIV. Thermometrical Experiments and Observations. By Tiberius Cavallo, F. R. S. p. 585.

Mr. C. having been appointed by the President and Council of the R. S. to write the annual dissertation, pursuant to the institution of Henry Baker, Esq.

F. R. S. he presented to the Society the following account of some thermometrical experiments and observations, the greatest part of which he says were made so long since as the year 1776. Having read in a volume of the Philos. Trans. the account of an experiment made with a thermometer, whose bulb was painted black, and was exposed to the rays of the sun, in which case it had been found, that the thermometer showed a much greater degree of heat than when not blackened, he was desirous of trying the ultimate limits of this difference. For which purpose he constructed two thermometers, the scales of which, being made by trial, coincided so perfectly well together, that when the thermometers were put in equal circumstances, no difference could be perceived between the degrees of heat shown by them. The length of a degree on the scale of those thermometers was a little more than $\frac{1}{40}$ of an inch, and though those scales were divided into degrees only, yet by inspection a person a little versed in these observations could easily distinguish the height of the quicksilver within a quarter of a degree.

These thermometers were both fixed on the same frame, at the distance of about an inch from each other, having the balls quite detached from the frame, and in this manner they were exposed to the sun, or to the light of a lamp.

When these thermometers were exposed to the sun, or kept in the shade, they showed the same degree precisely. The difference between the degree shown by these thermometers when exposed to the sun, and when kept in the shade at about the same time of the day, was very trifling. When the ball of one of those thermometers, which we shall call A, was painted black with Indian ink, or with the smoke of a candle, and that of the other thermometer B was left clean, on being exposed to the sun they showed different degrees of temperature; the quicksilver in the tube of A was much above the quicksilver in the tube of B. This difference sometimes amounted to about 10° but it was never constant, varying according to the clearness of the sun's light as well as of the air, and also according to the different degrees of temperature of the atmosphere.

Keeping the frame with those thermometers, one of which had the ball painted black, hung on the side of a window, Mr. C. observed a remarkable fact, viz. that these thermometers showed unequal degrees of heat, not only when presented to the sun, but also when exposed to the strong day-light. He cleaned the bulb of the thermometer A, and blackened that of B, but the effect was constant, viz. the quicksilver in the tube of the thermometer, whose bulb was painted black, was constantly higher than the other, whenever they were exposed to the strong day-light. This difference was commonly about $\frac{1}{2}$ of a degree, but sometimes it amounted to $\frac{3}{4}$, and even to a whole degree. The situation in which those thermometers were usually placed, was such that the light of

the sun could not be reflected on them by any object standing before; but the experiment answered even when the sun was hidden by clouds.

This observation seemed to show that perhaps every degree of light is attended with a proportionate degree of heat; and induced Mr. C. to try, in a similar manner, whether, by directing the concentrated light of the moon on the blackened ball of one of these thermometers, he could render sensible the effect of that light.* But though he attempted it some time ago with a large lens several times, and had lately tried it again with a burning mirror of 18 inches diameter, yet sometimes for want of proper means of observing the height of the mercury in the tubes of the thermometers, sometimes for want of a continued clear light of the moon, and in short from one unfavourable circumstance or other, he had not yet been able to make a fair and decisive trial of this experiment.

The light of the sun being very inconstant on account of clouds and of its diurnal motion, Mr. C. thought to make some experiments with the above-mentioned two thermometers, by exposing them to the light of a lamp, and he found that this light had a considerable effect upon them. The ball of one of the thermometers being blackened, and both being set at 2 inches distance from the flame of a lamp, they both rose from 58° , at which the mercury stood before the lighting of the lamp, to $65^{\circ}\frac{1}{2}$, and the blackened thermometer to $67^{\circ}\frac{1}{2}$. Another time, being set at the same distance from the lamp, the uncoloured thermometer came up to $67^{\circ}\frac{3}{4}$, and the blackened one to $68^{\circ}\frac{3}{4}$. In short, by various repeated trials it appeared, that the difference generally amounted to about 1° . When the thermometers were put farther than 2 inches from the lamp, this difference decreased, and at about 14 or 15 inches it quite vanished.

It is mathematically true, that emanations which proceed from a centre, and expand in a sphere, must continually become more and more rare in proportion to the squares of the distances from the centre. Thus it is said, that the intensity of light proceeding from a luminous body at the double, treble, quadruple, &c. of a given distance from that body, must be respectively 4, 9, 16 times less dense. The same thing may be said of heat. Being willing to ascertain this truth by actual experiment, Mr. C. placed several thermometers, whose balls were not painted, at different distances from the flame of the lamp, and expected to find, when the thermometer at 4 inches distance was 1° above that placed at 8 inches distance, the thermometer placed at 2 inches distance should be 4° higher.

* The concentrated light of the moon has often been thrown upon thermometers without any effect; but it does not appear that any blackened thermometer was ever used before for this purpose.

—Orig.

But on trying this experiment various times, placing the thermometers at different distances from the flame of the lamp, and making the proper calculations agreeable to those distances, it appeared, that the intensity of the heat did not decrease exactly in the duplicate proportion of the distances from the flame of the lamp, but showed a very odd irregularity. It seemed to decrease faster than the duplicate proportion of the distances for the space of 2 inches and a half or 3 inches, after which it decreased much slower. Whether this effect may be attributed to some different state of the air's purity at different distances from the flame of the lamp, or to the vapours proceeding from the flame, he could not determine.

The above-mentioned experiments gradually induced him to try the effect of the light of the sun and of a lamp on thermometers whose balls were painted with different colours. Dr. Franklin's experiment with the pieces of cloth set upon snow that was exposed to the sun is very well known. The doctor found, that those pieces of cloth, whose colour was darker, sunk deeper in the snow than the others, by which it appears, that they became hotter. Mr. C.'s view was to examine those different degrees of heat imbibed by different coloured substances with precision, to observe if they kept any proportion to the spaces occupied by the prismatic colours in the prismatic spectrum, or if they followed any other discoverable law; but those attempts met with many difficulties, the greatest of which was the choice of colours. The water colours that are commonly used, as carmine, sap-green, &c. are of so different a nature from each other, that when the balls of the thermometers were painted with them, their surfaces were not equally smooth, which occasioned great difference in the effect; for two thermometers, whose balls had been painted with the same colour, but the paint laid smoother on one than on the other, showed different degrees of heat when they were both exposed to the rays of the sun.

He attempted to make thermometers with tubes of differently coloured glass; but when a ball was formed with any of those tubes, the substance of the glass in the ball, being much thinner than in the tube, differed very little from clear coloured glass. To include the thermometers in close boxes, in which the rays entered through coloured glasses, was also found ineffectual; not only because the colours so transmitted were far from being homogeneous, but especially because some of those glasses are much more opaque than others, even of the same colour.

The least ambiguous method therefore, was that of painting the balls of the thermometers with water colours, taking care to lay them as equally smooth as possible. In this manner Mr. C. repeated several experiments, using sometimes a dozen of thermometers at once, whose balls were painted with various colours, and were exposed to the sun; and from a vast number of experiments, and after

some weeks observation, it could be only deduced, that if the colours, with which the balls of the thermometer were painted, were pretty like the prismatic colours, those thermometers showed a greater degree of heat, whose colours were nearer to the violet in the order of the prismatic colours, and contrarywise; but they were all, even that painted with white lead, in some intermediate degree between the blackened thermometer, and the naked or unpainted one. If the colours had not the proper degree of density, the effects were very different: thus, a thermometer painted with a light blue, was lower than another thermometer painted red with good carmine.

Mr. C. here describes the method in which he made and adapted the scales to his thermometers; and concludes this paper with mentioning an experiment, which, though not thermometrical, is yet useful in removing a wrong notion some persons have concerning the effect of light. Having seen in some book that the common black phosphorus, or Homberg's pyrophorus, was impaired by light, he was desirous to try the truth of this assertion. Accordingly towards the beginning of the last year he prepared some pyrophorus, and inclosed portions of it in 3 glass tubes, which were immediately sealed hermetically, and on the 20th of May, 1779, two of them were suspended to a nail out of a window, and the 3d was wrapped up in paper, and was inclosed in a box, where not the least glimmering of light could enter. In this situation they were left for above a year, after which he broke one of those that had been kept out of the window, and that which had been in the dark; but the pyrophorus of each tube seemed to be equally good, taking fire within about half a minute after being taken out of the tubes, and exposed to the air on pieces of paper; which shows that neither the presence nor absence of light had injured it.

END OF THE SEVENTIETH VOLUME OF THE ORIGINAL.

END OF VOLUME FOURTEENTH.

Erratum.—p. 194, l. 56, for unhappy read happy.

